

THE
Philosophical Works

Of the HONOURABLE

ROBERT BOYLE Esq;

Abridged, methodized, and disposed under the
GENERAL HEADS

O F

PHYSICS,
STATICS,
PNEUMATICS,



NATURAL-HISTORY,
CHYMISTRY, and
MEDICINE.

The whole illustrated with NOTES, containing the *Improvements*
made in the several *Parts* of natural and experimental *Knowledge*,
since his time.

By PETER SHAW, M. D.

V O L. II.

L O N D O N:

Printed for W. and J. INNYS, at the *West End* of *St. Paul's*; and
J. OSBORN, and T. LONGMAN, in *Pater-Noster-Row*.

M. DCC. XXV.

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By J. F. B. Esq. M.D.

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M. DCC. LXXV.

The RIGHT HONOURABLE

The Earl of ORRERY.

MY LORD,

YOUR known parts and learning justly entitle your Lordship patron to any deserving production of genius or study : But the volume here inscribed to your Lordship, has a farther right to your countenance and favour ; as not only treating of subjects, for a thorough knowledge whereof your Lordship is become eminent ; but being part of a work originally proceeding from one, whose immortal name adds to the dignity of your illustrious family.

VOL. II.

A 2

Mr.

Mr. *Boyle* survives in his works; but if we would see him express'd by real life, we must turn our eyes upon your Lordship, and your noble family, in whom reign, to perfection, the fine taste, and the comprehensive genius; the candid, generous, and communicative temper, so eminently predominant in Mr. *Boyle*.

I am,

May it please your Lordship,

Your Lordship's most humble,

most obedient,

and most devoted Servant,

Peter Shaw.

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EXPERIMENTS
AND
OBSERVATIONS
UPON
COLOURS.

SECT. I.

TIS often thought that a diversity of colours constantly argues an equal diversity in the nature of the bodies wherein they reside: but I cannot wholly give into this opinion. For not to mention changeable taffaties, the blue and golden feathers on the necks of pigeons, and several water-fowl, natural and artificial rainbows, &c. the colours whereof philosophers call not real, but apparent; we see that the contiguous feathers in various birds are some of them red, others white, blue, yellow, &c. and that in several parts of the same feather there is frequently the greatest disparity of colours: so in tulips, july-flowers, and other vegetables, even several parts of the very same leaf are frequently found of different dyes; tho' no difference is observed in their other properties. And such a variety we have much more remarkably in the marvel of *Peru*; for of the great profusion of fine flowers which that gaudy plant affords, I have scarce observed any two dyed perfectly alike. But tho' such particulars keep me from affirming, that a diversity of colours always denotes some great difference in bodies, yet that it often signifies considerable alterations in the disposition of their parts, appears from the extraction of tinctures, wherein the change of colour is the chief, and sometimes the only thing by which the artist regulates his procedure in their preparation. Instances of this are also obvious in several sorts of fruit, wherein according as the vegetable sap is ripened, by passing from one degree of maturation to another, the external part of

Diversity of colours, what it signifies.

Experiments and Observations

PHYSICS. the fruit changes from one colour to another. A less obvious instance of this kind is afforded us by the method of tempering steel for gravers, drills, springs, &c. which is this. First, the steel to be tempered is hardened, by heating it in glowing coals, and not quenched as soon as taken from the fire, but held over a basin of water till it descend from a white heat to a red one; when 'tis immediately quenched in cold water. The steel thus hardened will, if it be good, look whitish; and being brighten'd at the end, and held in the flame of a candle, that the bright end may lie about half an inch distant from the flame, it will swiftly pass from one colour to another, as from a bright yellow to a deeper and reddish yellow, from that to a fainter first, and then to a deeper blue; each of which succeeding colours argues such a change made in the texture of the steel, that if it be taken from the flame, and immediately quenched in tallow, whilst it is yellow, it will be of such a hardness as fits it for drills; &c. but if kept for a few minutes longer in the flame, till it turns blue, it becomes much softer and proper to make springs for watches; which are therefore commonly of that colour; lastly, if you keep the steel in the flame after the deep blue has appeared, it will grow too soft even for penknives. Any person may easily satisfy himself of the different hardness of steel of different colours, either by the file, or by breaking some slender wires thus tempered, and observing how they vary in point of brittleness.

Colour, what?

But before we descend to a more particular consideration of our subject, 'tis proper to observe, that colours may be regarded either as a quality residing in bodies to modify light after a particular manner, or else as light itself so modified as to strike upon the organ of sight, and cause the sensation we call colour; and that this latter is the more proper acceptance of the word colour, will appear hereafter. And indeed it is the light itself, which after a certain manner, either mixed with shades, or otherwise, strikes our eyes, and immediately produces that motion in the organ which gives us the colour of an object. * Yet because there is in the

* The words of Sir Isaac Newton excellently clear and illustrate this matter.

" The homogeneous light and rays (says that great philosopher) which appear red, or rather make objects appear so, I call rubrific, or red-making; those which make objects appear yellow, green, blue, and violet, I call yellow-making, green-making, blue-making, violet-making, &c. And if at any time I speak of light and rays as coloured, or endued with colours, I would be understood to speak not philosophically and properly, but grossly, and according to such conceptions as vulgar people would be apt to frame. For the rays, to speak properly, are not coloured. In them there is nothing else than a certain

" power and disposition to stir up a sensation of this or that colour; for as sound in a bell, or musical string, or other sounding body, is nothing but a trembling motion, and in the air nothing but that motion propagated from the object, and in the sensorium 'tis a sense of that motion under the form of a sound; so colours in the object are nothing but a disposition to reflect this or that sort of rays more copiously than the rest: in the rays they are nothing but their dispositions to propagate this or that motion into the sensorium, and in the sensorium they are sensations of these motions under the forms of colours. *Newton. Optic. p. 108, 109.*

coloured

coloured body a certain disposition of the superficial particles to send the light reflected or refracted to our eyes, colour may also, in some sense, be said to depend upon the visible body; and therefore I shall not reject the popular form of speaking of colours, provided we may have recourse occasionally to the distinction laid down. But still colour is so far from being inherent in objects, that light itself produces the sensation only as it causes a determinate kind of local motion in some part of the brain; for if the like motion happen from any other cause, wherein the light is unconcerned, a man shall think he sees the same colour*. Thus it is usual in dreams to fancy we behold coloured images; and the strange imaginations of distracted persons, with the flashings of light, and other appearances, upon some disorders of the brain, farther confirm this particular.

The like effect may also be produced from internal causes which affect the optic nerve; for I remember in myself that, upon coughing with vehemence, sudden flashes of vivid flame have appeared, as it were, before my eyes †. And a lady, to whom I am related, assured me, that all the objects of her sight, once, of a sudden, appeared of various unusual colours, surprizingly bright and vivid; which symptom was, the next day, followed by a fit of the hysteric disease. The like symptom a physician also informed me he had observed to be a certain prognostic of the plague, in a season when that distemper raged; but that it generally went off after the exhibition of an emetic. As an appearance of colour may, therefore, be produced by internal motions, without the assistance of an external object; so the colour, which would otherwise be produced by an outward object, may, possibly, be sometimes changed by a motion or new texture in the organ, as long as that motion or new disposition continues.

Thus I have often observed, upon looking at the sun thro' a telescope, darkened so as to make the splendor of that luminary supportable, the impression upon the retina, would nevertheless be so vivid and permanent, that if afterwards I turned the eye, therein made use of, towards a flame, it would appear of a colour very different from its natural one.

* 'Tis certain, that whenever the same eye is affected twice alike, vision will be the same in both cases; but different when the eye is differently affected. And this is a just, not an imaginary, foundation for distinguishing red light from blue, yellow from green, &c. for that is properly a red light which excites in us the idea of red, a blue which excites in us the idea of blue, &c. These several colours, therefore differently affect the eye, and consequently have different dispositions.

† To this purpose Sir Isaac Newton speaks thus: "When a man in the dark, says he, presses either corner of his eye with his finger, and turns his eye away from his finger, he will see a circle of

colours like those in the feather of a peacock's tail. If the eye and the finger remain quiet, these colours vanish in a second minute of time; but if the finger be moved with a quavering motion, they appear again. Do not these colours arise from such motions excited in the bottom of the eye by the pressure and motion of the finger, as at other times are excited there by light for causing vision? And do not the motions, once excited, continue about a second of time before they cease? And when a man by a stroke upon his eye sees a flash of light, are not the like motions excited in the retina by the stroke?" *Newton. Opt. p. 321, 322.*

PHYSICS.

And if I several times successively shut and opened the same eye, this new colour seem'd changed or impaired by degrees; till at length the same appeared to me of the same colour it did to other spectators. The like effect I have also found by looking upon the moon, when near the full, thro' a telescope, without employing a coloured glass to defend the eye. And here 'tis remarkable, that tho' my right eye, with which I looked thro' the telescope, were thus affected by the light, yet if I shut that eye, and looked upon the same object with the other, it appeared of its usual colour; but if I again opened and made use of the dazzled eye, the vivid adventitious colour would again return. 'Tis further observable, that a vehement blow upon the organ of vision, especially if it be naturally weak, may for a long time vitiate the action of vivid objects. I know a lady of unquestionable veracity, who having, by a desperate fall, received several hurts, and particularly a considerable one near her eye, had her sight so disordered, that the next morning, when one of her servants came to her bed-side, his clothes appeared adorned with such a variety of dazzling colours, as presently obliged her to command him to withdraw; and even the images in her hangings did, for many days after, appear to her, if the room were not greatly darkened, embellished with several offensively vivid colours, which nobody else could see in them. She said also, that she sometimes thought she saw colours so new and glorious, that they were of a peculiar kind, and such as she could not describe, by their likeness, to any she had seen either before or since; and that white objects greatly disordered her sight; that for several days after her fall, if she looked upon the inside of a book, she fancied, she there saw colours like those of a rainbow. And even when she seemed pretty well recovered, and left her chamber, happening to come into a place where the walls were white, they appeared to her of such dazzling colours as much offended her sight. She added, that this disorder of her eyes lasted for five or six weeks; tho' since that time she hath been able to read and write considerably, without finding the least inconvenience. A man of great learning coming to advise with me about a distemper in his eyes, told me, that having once looked too attentively upon the sun thro' a telescope, without a dark glass, the excess of light so strongly affected his eye, that ever since, when he turns it towards a window, or any white object, he fancies he sees a globe of light about the same bigness the sun then appeared to him; tho' it were now ten years since he first observed it. 'Twere easy from some remarkable symptoms, observed by *Epiphanius Ferdinandus*, in persons bitten by the tarantula, to shew, that without any change in the object, an alteration in the instrument of vision may, for a great while, make some colours appear delightful, and others disagreeable, and both to a high degree; tho' they had no such effects before. But these already mentioned may suffice for our present purpose.

We before observ'd, that colour may, notwithstanding all this, be consider'd as a quality residing in the body said to be colour'd; and indeed most of the following experiments refer to it principally under that notion.

For

For there is in colour'd bodies, and chiefly in their superficial parts, a certain disposition whereby they disturb * the light that comes from them to our eye, so as to make that distinct impression, upon whose account we say the visible body is either white or black, red or yellow. But because we shall hereafter more fully shew, that the changes, and consequently the production and the appearance of colours often depend upon the continuation or alteration of the texture of the object, we shall here previously intimate two or three particulars relating to this matter. And first, it is not without reason, that I ascribe colour chiefly to the superficial parts of bodies; for, not to examine how plentifully opaque corpuscles may abound even in those bodies we call diaphanous, it is plain, that we see little else than the superficies of dark objects; for if we found the rays of light reflected from the object, pierc'd deep into the body, we should not judge it opaque, but either pellucid or semi-diaphanous †.

Whether colours depend upon the surfaces of bodies.

The schools seem to teach, that colour is a penetrating quality, reaching to the innermost parts of the object; as if a piece of sealing-wax be broken into ever so many pieces, the internal fragments will be as red as the external surface. This is indeed a particular example; but it will not overthrow the foregoing doctrine; especially since other examples of a contrary nature may be alledged. And two or three negative instances are sufficient to overthrow the generality of a positive rule; at least when that is built but upon one, or a few examples. Not then to mention cherries, plums, &c. wherein the skin is of one colour, and the inside of another; I shall offer an instance or two of the colours of durable bodies, that are thought tolerably homogeneous, and whose parts are neither organical, nor of a nature approaching thereto. And first, I need only repeat, that blue, red, and yellow may be produced upon a piece of temper'd steel: for these colours, tho' very vivid, yet if you break the metal they adorn, will appear to be superficial; not only the innermost parts of the steel, but those within a hair's breadth of its superficies, having none of these colours, but retaining that of the metal itself. Secondly, we melted a large quantity of pure lead with a strong fire; then immediately pouring it into a clean iron vessel, and carefully and nimbly

* It follows from Sir Isaac Newton's discoveries, relating to light and colours, that all the productions and appearances of them in the world are derived not from any physical change caused in light by refraction or reflection, but only from the various mixtures or separations of rays, by virtue of their different refrangibility or reflexibility. And in this respect, says that great author, "the science of colours becomes a speculation as truly mathematical as any part of optics, so far as they depend on the nature of light, and are not produced or altered

" by the power of imagination; or by " striking or pressing the eye." Newton. Optic. p. 219.

† Perhaps all opaque bodies, when reduced sufficiently thin or fine, will appear transparent, if viewed against the light. This evidently happens, when metals are dissolved in proper menstrua; when gold is beat thin; when ink ascends in slender glass tubes; when an opaque stone, or other object, is viewed thro' a hole made in the window-shutter of a dark room, &c.

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The atomists of old, and some learned men of late, have attempted to explain the variety of colours in opaque bodies from the various figures of their superficial parts. The attempt is indeed ingenious, and the doctrine seems partly true; but I confess other things appear to me necessary to be taken in, as contributing to those different forms of asperity whereon the colours of opaque bodies depend. But in order to prove it, we must assume, that the surfaces of all such bodies, how smooth soever they may appear to our sight and touch, are only so in a popular, or a physical sense. The truth hereof is evident from the use of microscopes, which shew us in many bodies, that seem smooth to our naked eyes, little protuberancies rising above that which may be conceiv'd to be the plain of the surface; as also numerous depressions beneath that level. And of this sort of cavities we have, by the help of an excellent magnifier, on the surface of a thin piece of cork, that appeared smooth to the eye, observ'd sixty in a row, within the compass of that glass, tho' not above $\frac{1}{16}$ of an inch; and these too, which made that little piece of cork look almost like an empty

empty honey-comb, were not only very distinct, and of similar figures, but considerably large, and prodigiously deep; so that their distinct shadows and sides were very conspicuous, and easily to be reckoned, and might have been well distinguished, had they been ten times less than they were: whence we may make some estimate, what a strange inequality, and what a multitude of little shades there may really be in a scarce sensible part of a physical superficies, tho' they remain invisible to the naked eye. There are also several experiments which confirm the same thing in other like substances: so that every sensible part of an opake body, may be conceiv'd to consist of a multitude of corpuscles singly insensible. But in giving these surfaces a disposition to alter the light reflected thence to the eye, after the manner requisite to make the object appear colour'd, the figures of these particles have a great, tho' not the only share. 'Tis true indeed, that the protuberant particles may be of very various figures, spherical, elliptical, conical, &c. according to the nature whereof, and the situation of the lucid body, the light must be variously affected; as, after one manner from surfaces consisting of spherical, after another from those made up of conical or cylindrical particles; some being fitted to reflect more of the incident rays * of light, others less; and some towards one part, others towards another. But besides this difference of figure, many other things may greatly concur to vary the forms of asperity, whereon colours have so great a dependance: for allowing the figure of the particles, the superficial ones may be bigger in one body, and less in another; and consequently fitted to allay the light falling on them with greater shades. The protuberant particles may also be set at a greater or a smaller distance from one another in different parts; and how these qualities may serve to produce colour, we may guess from what happens in the agitation of water; for if the bubbles that are thereby made, be large and few, the water will scarce acquire a sensible colour; but if it be reduced to a froth, which consists of bubbles very minute, and contiguous to each other, it exhibits a very manifest whiteness. Besides, it is not necessary that the superficial particles which exhibit one colour, should be all of the same shape; but different figures may be mixed upon the surface of the opake body: as when the corpuscles that make a blue colour, and those that make a yellow, come to be accurately and skilfully united, they afford a green; which tho' it seem one simple colour, yet in this case appears to be made by the union of corpuscles of very different kinds. Moreover, the figure and magnitude of the little depressions, cavities, furrows, or pores intercepted betwixt these protuberant corpuscles, are as well to be considered, as the sizes and shapes of the corpuscles them-

* Sir Isaac Newton proves, that the cause of reflexion is not the impinging of light on the solid or impervious parts of bodies, as is commonly supposed; and makes it probable, that the reflexion of a ray is effected not by a single point of the

reflecting body, but by some power of the body which is evenly diffused all over its surface, and by which it acts upon the ray, without immediate contact. *Newton Optic. p. 237—241.*

PHYSICS. selves. For we may conceive the physical superficies of a body to be horizontally cut by a mathematical plain; and then, as some parts of that physical superficies will be protuberant, or swell above the plain, so others may be depressed beneath it. Thus in many places of the earth's surface, there are hills, trees, &c. rais'd above the horizontal level of the valley; and rivers, wells, pits, and other cavities, depressed beneath it. And that such protuberant and concave parts of a surface may remit the lights so differently as greatly to vary a colour, we shall see hereafter. But at present it may suffice to say, that the different degrees or kinds of asperity in the two flat sides of the same piece of red marble, the one whereof is polished, and the other left rough, will so diversify the light reflected from the several different plains to the eye, that a painter would employ two different colours to represent them. The situation also of the superficial particles is considerable. This I distinguish into the posture of the single corpuscles, in respect of the light, and of the eye; and the order of them in regard to one another: for a body may reflect the light otherwise, when its superficial particles are more erect upon the plain conceiv'd to pass along their basis; and when the extremities of such particles are obverted to the eye, than when those particles lie so inclined, that their sides are in great measure discernible. Thus the colour of plush or velvet will appear various, if you stroak part of it one way, and part another; the posture of the particular threads in regard to the light, or the eye, being thereby varied. And 'tis observable, that in a field of ripe corn, blown upon by the wind, there will appear waves of a colour different from that of the rest of the corn; because the wind, by depressing some of the ears more than others, causes one to reflect more light from the lateral and strawy parts than another. Thus when dogs are so enraged, as to erect the hairs upon their necks, and other parts of their bodies, those parts seem to acquire a colour different from that of the same hairs, when, in their usual posture, they lay more inclined. And that the order wherein the superficial corpuscles are ranged is considerable, we may guess by the turning of water into froth, the beating of glass, and the scraping of horn; in which cases the corpuscles that were before so marshall'd, as to be transparent, become, by a disturbance of that order, disposed to stop and reflect more light, and thereby to appear whitish.

There are also other ways wherein the order of the protuberant parts, in regard to the eye, may greatly contribute to the appearance of a particular colour: for I have often observed that when pease have been planted or set in parallel lines, and are shot up about half a foot above the surface of the ground, by looking on the field ~~where they stood~~ ^{across the field} along the lines they stood in, much the greater part of the ground appeared of its own dirty colour; but if I view'd the field transversely, it would appear very green; the upper parts of the pease concealing the intercepted parts of the ground from the eye. And perhaps even the motion of the small parts of a visible object may in some cases contribute, tho' it be not easy to say how, to the production or variation of a colour: for I have several times

made

made a liquor, which, when well settled in a close vial, is transparent and colourless; but as soon as the glass is unstopped, begins to fly away very plentifully in a white, opaque fume: and there are other bodies, whose fumes, when they fill a receiver, would make one suspect it contain'd milk; and yet these fumes become a liquor that is not white, but transparent. And such white fumes I have seen afforded by unstopping a liquor, which is itself diaphanous and red; nor are these the only instances of this kind that our experiments can supply us with. And if the superficial corpuscles be of the grosser sort, and so framed, that their differing sides may exhibit differing colours, then the motion or rest of those corpuscles may be considerable, as to the colour of the superficies they compose; because sometimes more, sometimes fewer of the sides disposed to exhibit such a colour, may by this means become or continue more obverted to the eye than the rest, and compose a physical surface more or less sensibly interrupted. Thus I remember, that in some sorts of plants, whose leaves were thick set by one another, and the two sides of each leaf of a somewhat different colour, there would appear a notable difference in their colour, if look'd upon, first when the leaves being at rest, had their true upper sides obverted to the eye; and again, when a breath of wind passing thorough them, made great numbers of the sides of the leaves, that are usually hidden, become conspicuous. And tho' the little bodies above-mentioned, may singly and a-part seem almost colourless; yet when many of them are placed by one another so near, that the eye does not discern an interruption in a sensible space, they may exhibit a colour: as we see, that tho' a slender thread of dy'd scarlet silk, whilst look'd on singly, seems almost quite devoid of redness; yet when numbers of these threads are brought together into a skein, that colour then becomes glaring. And in changeable taffeties, where we see differing colours arise and vanish upon ruffling the same piece of silk; I have often, with pleasure, observed, by means of a convenient microscope, their component threads to pass under and over each other, in almost innumerable points: and if I thus look'd upon any considerable portion of the stuff that, for example, appeared red to the naked eye, I could plainly see, that in such a position the red threads were conspicuous, and reflected a vivid light: and tho' I could also perceive that there were green ones, yet, by reason of their disadvantageous position in the physical surface of the silk, they were in part hid by the more protuberant threads of the other colour; and, for the same reason, the reflection from as much of the green as was discover'd, appear'd but dim and faint. And if, on the contrary, I look'd thro' the microscope upon any part that appear'd green, I could plainly see the red threads were less fully exposed to the eye, and obscured by the green ones, which, therefore, made the predominant colour. And by observing the texture of the silk, I could easily so expose the threads either of the one colour or the other to my eye, as at pleasure to exhibit the appearance of red or green, or make those colours succeed each other: so that, observing their succession, I could mark how the predominant colour started up, when the threads that exhibited it,

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came to be advantageously placed; and by making little folds in the taffaty, the sides that met, and terminated in them, would appear to the naked eye, the one red, and the other green. And when threads of more than two differing colours chance to be interwoven, the resulting changeableness of the taffaty may also be somewhat different. And perhaps it were proper to try whether the microscope would give us the reason of the variableness of colour, that is so conspicuous in mother of pearl, opals, &c. 'Tis true indeed, that what I here propose concerning the different forms of asperity in bodies, whence the incident light either comes to be reflected with more or less of shade, and with that shade more or less interrupted, or happens to be otherwise modify'd, is but conjectural; yet I am apt to suspect, that if we had perfect microscopes*, we might discern in the physical surfaces of bodies, both a great many latent inequalities, and the particular size, figure, and situation of the extremely little bodies that cause them; and perhaps might perceive, among other varieties, how those little protuberances and cavities interrupt and dilate the light, by mingling with it a multitude of very small shades, some of them more, and some less minute; some less, and some more numerous, according to the nature and degree of the particular colour we attribute to the visible object. Thus with good telescopes we can discern many hills and vallies in the moon, whereof some are more, and others less strongly illumin'd; whilst these have a fainter, and those a deeper shade: tho' the naked eye can discern no such things in that planet. Thus also, where the naked eye could only see a green powder, a microscope enabled us to discover particular granules, some of a blue, and some of a yellow colour; tho' we had before caused them to be exquisitely mixed, to compound a green. And here, to confirm what we have delivered, as to the possibility of discerning the different forms of asperity in the surfaces of bodies of several colours, I shall add a remarkable relation which I had from Dr. Finch, anatomist extraordinary to the great duke of Tuscany. This gentleman told me, that he saw a blind man at *Maestricht*, in the *Low Countries*, who at certain times could distinguish colours by the touch with his fingers. This appear'd so extraordinary to me, that I freely propos'd many scruples concerning it to the doctor; and particularly inquired, whether care were taken to prevent the man from using his sight in the case, if he had any: but, upon the whole, I found the doctor had been very cautious and circumspect, to prevent being impos'd upon herein. And, that he might not, thro' any mistake in point of memory, misinform me, he presented me the notes he took for his own use; the substance whereof is as follows. The name of the man was *John Vermaasen*, at that time about thirty-three years of

A blind man,
who distinguish-
ed colours by the
touch.

* Sir Isaac Newton seems to be of opinion, that microscopes may be so far improved, if they are not, in some degree, already arrived at that perfection, as to discover all the particles of bodies on which their colours depend, except those of black substances, which he shews to be the most minute. *Newton. Optic. p. 236.*

age, who, when he was two years old, had the small pox, which render'd him absolutely blind, tho' he is at present an organist in a public choir. The doctor discoursing with him over night, the blind man affirmed, that he could distinguish colours by feeling, but not unless he were fasting; for that any quantity of drink deprived him of that exquisite touch which is requisite to so nice a sensation. Upon this, the doctor provided against the next morning seven pieces of ribbon of these seven colours, black, white, red, blue, green, yellow, and grey; but as for mixed colours, this *Vermaafen* would not undertake to discern them; tho, if offer'd, he could tell that they were mixed. To discern the colour of the ribbon, he places it betwixt his thumb and his fore-finger, but his most exquisite perception is in his thumb, and much better in the right than in the left. After the man had four or five times told the doctor the several colours, whilst a napkin was tied over his eyes, the doctor observed he twice mistook, for he called the white black, and the red blue; but still before his error, he would lay them by in pairs, saying, that tho' he could easily distinguish them from all others, yet those two pair were not easily distinguishable from one another. Then the doctor desired to know what kind of difference he found in colours by his touch. To which the blind man reply'd, that all the difference he observed, was a greater or less degree of asperity †; for, says he, black feels like the points of needles, or some harsh sand, whilst red feels very smooth; that black and white are the most rough, or unequal, of all colours, and so like, that 'tis very hard to distinguish them; but black the roughest of the two: that green is next in asperity; grey next to green; yellow the fifth in degree of asperity; red and blue so alike, that 'tis as hard to distinguish between them as between black and white, tho' red be somewhat more rough than blue; so that red has the sixth place, and blue the seventh in asperity. To this information the doctor was pleased to add the present of three of those very pieces of ribbon, whose colours, in his presence, *Vermaafen* had distinguished; pronouncing the one grey, the other red, and the third green; which I keep by me as rarities. Before I saw the notes from whence this account is taken, I confess I suspected this blind man might have thus distinguished colours rather by the smell than by the touch; for some

† The different magnitudes of the parts of coloured bodies, whereon their different colours, according to Sir *Isaac Newton's* theory, depend, appear to be too minute for the sense of feeling to take notice of. This sense, indeed, may, by great degrees, be more exquisite in one man than in another; and, if it could be supposed equal, or nearly equal to that of sight, which perhaps is only a finer degree of touch, this different asperity might, supposing the truth of the relation, become sensible to one, possessed of an exquisite

sense of feeling. Sir *Is. Newton* well observes, that there is a harmony and discord of colours, which he seems to think arises from the proportions of the vibrations propagated thro' the fibres of the optic nerve into the brain, as the harmony and discord of sounds arise from the proportions of the vibrations of the air. For some colours, if they be viewed together, are agreeable to one another, as those of gold and indigo, and others disagree. See *Newton. Optic.* p. 320.

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of the ingredients employ'd by dyers, have different and strong scents, which a very nice nose might distinguish; and this I the rather suspected, because he required that the ribbons he was to judge of, should be offer'd him in the morning fasting; for I have observed in setting-dogs, that the feeding of them greatly impairs their scent. And, indeed, I could wish for the opportunity of examining this man myself, about many particulars which I do not find to have been yet thought upon. And tho' it be not incredible, that since the liquors used by dyers are qualified to give a colour, by multitudes of little corpuscles of the pigment dissolved and extracted by the fluid wherein they float, those corpuscles of colour insinuating themselves into, and filling up the pores of the body to be dyed, may give a greater or less roughness to its superficies, according to the bigness and texture of the particles of the pigment; yet I can scarce believe that this blind man could distinguish all the colours he did, merely by the ribbons having more or less of asperity: but the forms of it might also assist him herein, tho' these would perhaps have been very difficult for him to describe; because those minute differences having not been taken notice of by men for want of a touch sufficiently exquisite, are things he could not have expressed intelligibly. Thus under the name of sharp, sweet, and sour, there are abundance of peculiar intermediate relishes, or tastes, in different sorts of wine; which tho' critical and experienc'd palates easily discern, yet the judges cannot have their idea's convey'd to others; such minute differences not having hitherto any distinct names assign'd them. And it seems that there was something here in the forms of asperity requisite to the distinction of colours, besides the degree of it; since this man found it so difficult to distinguish black and white from one another, tho' not from other colours. For I might urge, that he seems to contradict himself about the red, which in one place he represents as somewhat more rough than the blue, and in another very smooth; but because he speaks of this smoothness in that place where he mentions the roughness of black, we presume that he meant but a comparative smoothness: however, he found it difficult not only to distinguish red and blue, but black and white from one another, tho' not from other colours. And indeed, tho' in the ribbons offer'd him, they might be almost equally rough, yet in such minute corpuscles as those of colour, there may easily be conceiv'd not only a greater closeness of parts, a want of protuberant corpuscles, and little extant particles otherwise figured and ranged in the white than in the black; but the cavities also may be much deeper in one than in the other. And perhaps, where the particles are so exceeding slender, we might allow the parts exposed to the sight and touch to be a little convex in comparison of the erect particles of black bodies; as if they were wares many times slenderer than a hair, for whether you suppose them to be figured like needles, or cylindrically like the hairs of a brush, with hemispherical tops, they will be so very slender, and consequently the points both of the one sort and the other, so very sharp, that an exquisite touch will be able to observe no greater difference between them,

them, than that which our blind man allowed, when, comparing black and white bodies, he said, that the latter were the less rough of the two. Nor is every kind of roughness, tho' sensible, inconsistent with whiteness, for the physical superficies of a body is sometimes made, by the same operation, both rough and white; as when the level surface of clear water being agitated and made rough, with a multitude of unequal bubbles, thereby acquires a whiteness; and as a smooth piece of glass by being scratched with a diamond, discloses the same colour. This minuteness and figure of the extant particles being supposed, the depth of the little cavities intercepted between the extant particles, without being so much greater in black bodies than in white ones, as to be very perceptible to the gross organs of touch, may be vastly greater with regard to their disposition of reflecting the subtle rays of light. For in black bodies those small intercepted cavities, and other depressions, may be so figured, so narrow and so deep, that the incident rays of light, which the more extant parts of the physical superficies are disposed to reflect inwards, may be detained there, and prove unable to emerge; whilst in a white body the slender particles may not only, by their figure, be fitted to reflect the light copiously outwards, but the intercepted cavities being shallow, and perhaps somewhat wide, the bottoms of them may be so constituted, as to be fit to reflect outwards much of the light that falls upon them.

There is no necessity of concluding, from the blind man's relation, that tho', according to his touch, black was the roughest, as it is the darkest of colours, that therefore white, which is the lightest, should also be the smoothest; for he makes yellow to be two degrees rougher than blue, and as much smoother than green; tho' yellow not only appears to the eye a lighter colour than blue, but we shall hereafter shew, that yellow reflects much more light than blue, and manifestly more than green; which seems to strengthen our conjecture, that there was something in the kinds of asperity, as well as in the degrees of it, that enabled *Vermaafen* to distinguish colours; and, at least shews, that we cannot, in all cases, from the bare difference in the degrees of asperity in colours, safely conclude that the rougher reflects the least light. Thus much, however, we gain from the testimony of this blind man, that since many colours may be felt with the circumstances above related, the surfaces of such coloured bodies must certainly have different degrees, and in all probability different forms, or kinds of asperity belonging to them; which is all the use I here designed to make of the history of this blind man: for hence it sufficiently appears, that colour greatly depends upon the disposition of the superficial parts of bodies, and, probably, wherein such a disposition principally consists*.

But

* The permanent colours of natural bodies arise from hence, that some such bodies reflect some sorts of rays, and others other sorts more copiously than the rest. "Minium, says Sir Isaac New-

ton, reflects the least refrangible, or "red-making rays most copiously, and "thence appears red. Violets reflect the "most refrangible, most copiously, and "thence have their colour, and so of "other

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*How changes
are produced in
colours, by li-
quors.*

But to return ; the causes of the several forms of asperity, that may diversify the surfaces of coloured bodies, will, perhaps, afford us some general conjectures as to several of the ways whereby 'tis possible to produce the sudden changes of colours, we shall hereafter find in our experiments ; for most of those phenomena being produced by means of liquors, which generally abound with very minute, active, and variously figured saline corpuscles, they may well very suddenly alter the texture of the body they are employed to work on ; and so change their form of asperity, and thereby make them reflect to the eye the light that falls on them, after another manner than they did before, and by that means vary the colour so far as it depends upon the texture or disposition of

“ other bodies. Every body reflects the
“ rays of its own colour more copiously
“ than the rest, and from their excess
“ and predominance in the reflected light
“ has its colour.—Every body appears
“ most splendid and luminous when view-
“ ed in the light of its own colour. Cin-
“ nabar, in the homogeneal, red, prisma-
“ tic light is most resplendent, in the
“ green light it is manifestly less resplen-
“ dent, and in the blue light still less.
“ Indigo in the violet blue light, is most
“ resplendent, and its splendor is gra-
“ dually diminished, as it is removed
“ thence by degrees thro' the green and
“ yellow light to the red. By a leek
“ the green light, and next that the blue
“ and yellow, which compound green,
“ are more strongly reflected than the
“ other colours, red and violet, &c. But
“ to make these experiments the more
“ manifest, such bodies ought to be cho-
“ sen as have the fullest and most vivid
“ colours, and two of those Bodies are
“ to be compared together. Thus, for
“ instance, if cinnabar and ultramarine
“ blue, or some other full blue be held
“ together in the red homogeneal light,
“ they will both appear red, but the
“ cinnabar will appear of a strongly lu-
“ minous and resplendent red, and the
“ ultramarine blue, of a faint, obscure,
“ and dark red ; and if they be held to-
“ gether in the blue homogeneal light,
“ they will both appear blue, but the ul-
“ tramarine will appear of a strongly lu-
“ minous and resplendent blue, and the
“ cinnabar of a faint and dark blue ;
“ which puts it past dispute, that the
“ cinnabar reflects the red light much
“ more copiously than the ultramarine

“ doth, and that the ultramarine reflects
“ the blue light much more copiously
“ than the cinnabar doth. The same expe-
“ riment may be made successfully with
“ red lead and indigo, or with any other
“ two coloured bodies, if due allowance
“ be made for the different strength or
“ weakness of their colour and light.
“ And since these reflected lights, which
“ differ in colour, differ also in degree
“ of refrangibility, it's certain that some
“ bodies reflect the more refrangible,
“ and others the less refrangible rays
“ more copiously ; and that this is the
“ only reason of these colours, appears
“ from hence, that the colour of homo-
“ geneal light cannot be changed by the
“ reflexion of natural bodies. For if
“ bodies, by reflexion, cannot in the
“ least change the colour of any sort of
“ rays, they cannot appear coloured by
“ any other means, than by reflecting
“ those which are either of their own
“ colour, or which, by mixture, must
“ produce it. But in making experi-
“ ments of this kind, care must be had
“ that the light be sufficiently homoge-
“ neal.” *Newton. Optic. p. 156—158.*
“ In transparently coloured liquors,
“ 'tis observable, that their colour uses
“ to vary with their thickness. Thus a
“ red liquor, in a conical glass, held be-
“ tween the light and the eye, looks of
“ a pale and dilute yellow at the bot-
“ tom where 'tis thin, and a little higher
“ where 'tis thicker, grows orange ; and
“ where 'tis still thicker, becomes red ;
“ and where 'tis thickest, the red is
“ deepest and darkest. For it is to be
“ conceived, that such a liquor stops the
“ indigo-making and violet-making rays
“ most

of the visible parts of the object; for I do not absolutely exclude all other ways of modifying the rays of light, between their parting from the lucid body, and their reception into the common organ of vision. Now there seem to be several ways whereby liquors may suddenly alter the colour of one another, but I shall only touch upon a few. And, first, the minute corpuscles that compose a liquor may easily insinuate themselves into those pores of bodies whereto their size and figure adapt them; and these pores they may either exactly or inadequately fill; in which latter case they will, for the most part, alter the number and figure, but always the

“ most easily, the blue-making rays more
“ difficultly, and the red-making most
“ difficultly; and that if the thickness
“ of the liquor be only so much as suffi-
“ ces to stop a competent number of the
“ violet-making and indigo-making rays,
“ without diminishing much the num-
“ ber of the rest, the rest must compound
“ a pale yellow. But if the liquor be
“ so much thicker, as to stop also a great
“ number of the blue-making rays, and
“ some of the green-making, the rest must
“ compound an orange; and where it is
“ so thick as to stop also a great number
“ of the green-making, and a considera-
“ ble number of the yellow-making, the
“ rest must begin to compound a red,
“ and this red must grow deeper and
“ darker, as the yellow-making and
“ orange-making rays are more and more
“ stopped by the increasing thickness of
“ the liquor; so that few rays besides
“ the red-making can get through.

“ Of this kind is an experiment lately
“ related to me by Dr. Halley, who, div-
“ ing deep into the sea in a diving ves-
“ sel, found, in a clear sunshine day,
“ that when he was sunk many fathoms
“ deep into the water, the upper part of
“ his hand, on which the sun shone di-
“ rectly thro’ the water, and thro’ a
“ small glass-window in the vessel, like
“ that of a damask rose, and the water
“ below and the under part of his hand
“ illuminated by light reflected from the
“ water below, looked green. From
“ thence it may be gathered, that the
“ sea-water reflects back the violet and
“ blue-making rays most easily, and lets
“ the red-making pass most freely and
“ copiously to great depths; for there-
“ by the sun’s direct light, at all great
“ depths, by reason of the predominating

“ red-making rays must appear red, and
“ the greater the depth is, the fuller
“ and intenser must that red be. And at
“ such depths as the violet-making rays
“ can scarce penetrate to, the blue-mak-
“ ing, green-making and yellow-mak-
“ ing rays being reflected from below
“ more copiously than the red-making
“ ones, must compound a green. Now
“ if there be two liquors of full co-
“ lours, suppose a red and a blue, and
“ both of them so thick as suffices to
“ make their colours sufficiently full,
“ tho’ either liquor be sufficiently trans-
“ parent a-part, yet will you not be able
“ to see thro’ both together; for if only
“ the red-making rays pass thro’ one li-
“ quor, and only the blue-making thro’
“ the other, no rays can pass thro’ both.
“ This Dr. Hook tried casually with glass
“ wedges, filled with red and blue li-
“ quors, and was surprized at the event.

“ Now whilst bodies become coloured,
“ by reflecting or transmitting this or
“ that sort of rays more copiously than
“ the rest, it is to be conceived that
“ they stop and stifle in themselves the
“ rays which they do not reflect or trans-
“ mit. For if gold be foliated, and held
“ between the eye and the light, the
“ light looks of a greenish blue; and
“ therefore massy gold lets into its body
“ the blue-making rays, to be reflected
“ to and fro within it, till they be stop-
“ ped and stifled, whilst it reflects the
“ yellow-making outwards, and thereby
“ looks yellow. And much after the
“ same manner that leaf-gold is yellow
“ by reflected, and blue by transmitted
“ light, and massy gold is yellow in all
“ positions of the eye; there are some
“ liquors, as the tincture of *Lignum Ne-*
“ *phriticum*, and some sorts of glass,
“ which

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the magnitude of the former pores. And in what capacity soever these corpuscles of a liquor come to be lodged or harboured in such pores, the surface of the body will commonly have its asperity altered, and the incident light, that meets with a grosser liquor in the little cavities that before contained nothing but air, or some more subtle fluid, will have its rays either refracted, absorbed, or reflected more or less irregularly than if the body had been dry. Thus we see that even fair water falling on white paper, linen, &c. will, for some such reason as those assigned, immediately alter the colour of them, and for the most part render it deeper than that of the unmoisten'd parts of the same bodies. In like manner, when, during the summer, the high-ways are dry and dusty, if there fall much rain, they quickly appear of a darker colour than before. Thus also if a drop of oil be let fall upon white paper, that part of the paper which by imbibing the liquor acquires a greater continuity, and some transparency, will appear much darker than the rest; many of the incident rays of light being now transmitted, that would, otherwise, be reflected to the eye.

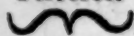
Secondly, a liquor may alter the colour of a body, by freeing it from those things that prevent its appearing of its genuine colour; and tho' this may be said to be rather a restoration of a body, or a restitution of

“ which transmit one sort of light most
 “ copiously, and reflect another sort,
 “ and thereby look of several colours,
 “ according to the position of the eye to
 “ the light. But if these liquors or
 “ glasses were so thick and massy, that
 “ no light could get thro them, I ques-
 “ tion not but they would, like all other
 “ opake bodies, appear of one and the
 “ same colour in all positions of the eye.
 “ For all coloured bodies, so far as my
 “ observation reaches, may be seen thro,
 “ if made sufficiently thin, and therefore
 “ are in some measure transparent, and dif-
 “ fer only in degree of transparency from
 “ tinged and transparent liquors; these li-
 “ quors, as well as those bodies, by a suffi-
 “ cient thickness becoming opake. A trans-
 “ parent body, that looks of any colour
 “ by transmitted light, may also look of
 “ the same colour by reflected light, the
 “ light of that colour being reflected by
 “ the farther surface of the body, or by
 “ the air beyond it. And then the re-
 “ flected colour will be diminished, and
 “ perhaps cease, by making the body ve-
 “ ry thick, and pitching it on the back-
 “ side, to diminish the reflexion of its
 “ farther surface, so that the light re-
 “ flected from the tinging particles may
 “ predominate. In such cases the colour

“ of the reflected light will be apt to va-
 “ ry from that of the light transmitted.”
Newton. Optic. p. 159—163.

This doctrine is farther confirmed, by mixing coloured light, so as to compound a beam of light of the same colour and nature with a beam of the sun's direct light, and making experiments therein. Thus Sir Isaac Newton found, that such light is endued with all the properties of a beam of the sun's light; natural bodies appearing in it of the same colours they have in day-light. Thus cinnabar appeared of the same red colour in it as it does by day, and if the green-making and blue-making rays were stopped, its redness became more full and lively; but if the red-making rays were intercepted, it became yellow or green, or of some other colour, according to the sorts of rays which were not intercepted. So gold in this light appears of the same yellow colour as in day-light; but by stopping a due quantity of the yellow-making rays, it appears white like silver; which shews, that its yellowness arises from the excess of the intercepted rays tinging that whiteness with their colours when they are let pass. See *Newton. Optic. p. 163—167.*

its



its native colour, than a change; yet there here happens a change of the colour which the body afforded before this operation. And such a change a liquor may work, either by dissolving or corroding, or by some such means carrying off the matter, which either veiled or disguised the colour that afterwards appears. Thus we restore old pieces of dirty gold to a clear shining yellow, by putting them into the fire, and into *Aqua fortis*, which takes off the acquired filth from the metal; and there is also an easy way to restore silver coins to their due lustre, by fetching off that which discoloured them. I have likewise a chymical liquor, which I employ to restore pieces of cloth spotted with grease, to their proper colour, by imbuing the spotted part therewith; which incorporating with the grease, and yet being of a very volatile nature, easily carries it away with itself. And I have sometimes try'd, that by rubbing upon a good touchstone a certain metalline mixture so compounded, that the impression it left upon the stone appeared of a very different colour from that of gold; yet a little *Aqua fortis* would immediately make the golden colour disclose itself, by dissolving only the other metalline corpuscles that concealed those of the gold.

Thirdly, a liquor may alter the colour of a body, by comminuting its parts; and principally after two ways: First, by disjoining and dissipating those clusters of particles which stick more loosely together, only by means of some cement that is easily dissoluble; and this seems to be the case in some of the following experiments, where the colour of many corpuscles brought to cohere, by being precipitated together, is destroyed by the affusion of very sharp and piercing liquors. Secondly, By dividing the grosser and more solid particles into minute ones, which will be, for the most part, otherwise shaped than the entire corpuscles so divided; as happens in a piece of wood reduced to splinters or chips, or when a piece of crystal heated red-hot, and quenched in cold water, cracks into a multitude of little fragments, which, tho' they do not fall asunder, alter the disposition of the body of it, as to its manner of reflecting the light.

There is a fourth way opposite to the third, whereby a liquor may change the colour of another body, especially a fluid, by procuring the coalition of several particles that before lay scattered, and too remote from each other to exhibit the colour that afterwards appears. Thus, when a solution of gold has been so diluted, that I questioned whether the liquor really imbibed any of the metal, I have been quickly satisfied it did, by pouring a little mercury into it; for the gold therein would soon clothe the surface of the quicksilver with a thin film of its own livery. And by this way of bringing the minute parts of bodies together, in such numbers as to make them a fit object for the eye, many of those colours seem to be generated which are produced by precipitation; especially if made with fair water; as when resinous gums, dissolved in spirit of wine, are let fall again, if the spirit be greatly diluted with that weakening liquor. And thus out of the rectified and transparent butter of antimony, there will, by the bare mixture of fair water, be plentifully precipitated

PHYSICS. that milk-white substance, which, by having its looser salts well washed off, is turned into *Mercurius vita*.

A fifth way wherein a liquor may change the colour of a body, is by dislocating its parts, and changing their order; at the same time, perhaps, also altering the position of the single corpuscles, as well as their situation in respect of one another. What share particular commotions or dislocations of the parts of a body may have in changing its colour, is evident from the mutations of that quality observable in quicksilver, and some other concretes, long kept by chymists in a convenient heat, tho' in close vessels; and also in the obvious degenerations thereof in bruised cherries, and other fruit: and that such liquors as we have been speaking of, may greatly discompose the textures of many bodies, and thereby alter the disposition of their superficial parts, the great commotion made in metals and other substances by *Aqua fortis*, oil of vitriol, &c. easily persuade us. And what such a varied situation of parts may do towards diversifying the manner wherein they reflect the light, may be guessed, in some measure, by the pulverization of transparent glass, which thereby becomes a white powder; but still better, by the experiments we shall hereafter set down, with relation to the production and destruction of colours by subtil, saline liquors. And in some chymical oils, as particularly in that of lemon-peel, the transposition of the parts, consequent upon bare concussion, will represent on the surfaces of the bubbles thereby occasioned, exceeding lively colours, which, when the bubbles relapse into the rest of the oil, immediately vanish. I know not whether I should mention, as a distinct method, that power whereby a liquor may alter the colour of another body, by putting the parts of it into motion; for tho', possibly, the motion so produced, seldom, of a sudden, changes the colour of the body whose parts are agitated; yet this seems to be one of the most general, tho' not immediate causes of the quick change of colours in bodies; for the parts being put into motion by the adventitious liquor, many of them, that were before united, may be thereby disjoined; and when that motion ceases or decreases, others of them may stick together in a new order: by which means the motion may sometimes produce permanent changes of colours; as in the experiment we shall hereafter mention, where a snow-white body is immediately turned into a yellow, by the bare affusion of fair water. And tho when you rub good blue vitriol upon the clean blade of a knife, it will not impart its latent colour thereto; yet if you moisten that substance with common water, the particles of the liquor disjoining those of the stone, and thereby giving them the various agitation requisite to fluid bodies, the metalline corpuscles of the vitriol thus dissolved will lodge themselves in throngs in the small pores of the iron, and give the surface of it the genuine colour of copper.

Lastly, The most important way, by which a liquor may alter the colour of another body, is by associating the saline, or other more rigid corpuscles of the liquor, with the particles of the body whereon it is employ'd to work. For these adventitious corpuscles uniting with the protuberant

tuberant particles of the surface of a coloured substance, must necessarily alter their bigness, and most commonly their shape. And that the colours of bodies greatly depend upon the bulk and figure of their superficial particles, seems probable, since many ancient and modern philosophers have thought, that all colours might, in the general, be accounted for from them alone. The diversification hereof will, in our case, be attended with these two circumstances; the one, that the protuberant particles being increased in bulk, must often be varied as to the closeness or laxity of their order; fewer of them being contained within the same sensible space than before; or else, by approaching one another, they must streighten the pores; and perhaps too by the manner of their associating themselves with the protuberant particles, intercept new pores. And thus, also, these adventitious corpuscles may produce a great change, as well in the little cavities, as in the protuberances of a coloured body; for they may likewise, by lodging themselves in those little cavities, fill them up; and it may easily happen, that they shall not only fill the pores they insinuate themselves into, but have their upper parts extant above them: and partly by such new protuberances, partly by increasing the bulk of the former, these extraneous corpuscles may much alter the number and bigness of the superficial pores, changing the old ones, and intercepting new; and very probably the order of the little extancies, and consequently that of the little depressions, in point of situation will be altered likewise. Thus if you dissolve quicksilver in some kinds of *Aqua fortis*, the saline particles of the menstruum associating themselves with the mercurial globules, will make a green solution, which afterwards easily degenerates; and red lead, dissolved in spirit of vinegar, yields a clear solution, the redness of the lead being destroyed by the liquor. But a better instance is afforded from copper; for I have try'd, that if upon a copper plate you let some drops of weak *Aqua fortis* rest for a while, the corpuscles of the menstruum joining with those of the metal, will produce a very sensible asperity upon the surface of the plate, and thence coagulate into very minute grains of a pale blue vitriol; tho', if upon another part of the same plate you suffer a little strong spirit of urine to remain for a competent time, the rough surface will be adorned with a deeper and richer blue. And the same *Aqua fortis*, that suddenly changes the redness of minium into a darker colour, will, being put upon crude lead, produce a whitish substance, as with copper it did a bluish, and as with iron it gives a reddish, and on white quills a yellowish colour: so much may the coalition of the parts of the same liquor, with the differently figured particles of solid bodies several ways turn the differinglly disposed surfaces rough, and diversify the colour of those bodies. And 'twill be easily believed, that in many changes of colour, which happen upon the dissolutions of metals, and precipitations made with oil of tartar, &c. a coalition of saline corpuscles may be made with the particles of the body dissolved or precipitated; if we consider how much the vitriol of a metal may be heavier than the pure metalline part thereof, upon account of the

PHYSICS. saline parts coagulated therewith; and that in several precipitations the weight of the calx, for the same reason, much exceeds that of the metal when it was first exposed to be dissolved.

But I am not aiming to establish a particular theory of colours, only design to offer some experiments and observations on the subject of them. I shall, therefore, add but two particulars more, with relation to the forms of superficial asperity in coloured bodies. And first, I say, that there are many other means whereby true and permanent colours may be produced in bodies, besides those practicable by the help of liquors; for proof whereof several examples might be alledged; but I need only refer to what we have already observed of the change of colours suddenly made on tempered steel and lead, by the operation of heat, without the interposition of a liquor. Secondly, I observe, what is of more importance to our present subject, that tho' nature and art may, in some cases, so change the asperity of the superficial parts of a body, as to alter its colour by any of the mentioned ways a-part, yet 'tis generally by two or three, or perhaps by more of them together, that the effect is produced; and if it be considered how variously those several ways, with others of the like nature, may be compounded and applied, it will not appear surprizing, that such fruitful principles should be fitted to change or generate many differing colours.

Whether all bodies are transparent?

Hitherto we have considered the little protuberances, or other superficial particles which constitute the roughness of bodies, as if we took it for granted that they must be perfectly opaque, and impenetrable to the rays of light, and so contribute to the variety of colours, as they stop more or less thereof, and reflect them to the eye particularly mixed with more or less of shade. I have often thought it worth a serious inquiry, whether or no particles of matter, each of them singly insensible, may not yet separately consist of many minuter particles, betwixt which we may conceive little junctures, where they adhere to one another, in some degree pervious to the prodigiously subtile corpuscles of the rays of light; whence consequently those particles would, in such a degree, be diaphanous. For as perfectly opaque bodies can only reflect the incident rays of light, those that are diaphanous refract* them too; and that refraction has a great hand

* Refrangibility of the rays of light is their disposition to be turned out of their way, in passing out of one transparent body into another; reflexibility, their disposition to be turned back into the same medium from the surface of any other. Sir Isaac Newton first shewed the precise difference between them, in the *Philosophical Transactions*, No. 80. A.D. 1671. and in several subsequent pieces which occur in the same *Transactions*, so far vindicated his discovery from several

objections made against it, that his opponents, and particularly F. Pardies, ingenuously acknowledged themselves satisfied with it. At length Sir Isaac published his doctrine more fully, in his excellent treatise of the *reflections, refractions, inflections, and colours of light*; wherein he confirms it by numerous experiments, made with the utmost simplicity and plainness. Herein he shews that the colours of light produced by the prism, and also those reflected from opaque bodies,

hand in the production of colours, appears from light passing thro' drops of water, which exhibits a rain-bow; thro' prismatic glasses, and many other transparent bodies. And I find, that in a darkned room, where the light is permitted to enter but at one hole, the little floating particles of dust, commonly called motes, being viewed by an eye placed on one side of the beam of light, appear in certain positions adorned with very vivid colours, like those of the rain-bow, or rather like those of very minute sparkling fragments of diamonds; and that as soon as their motion has brought them to an inconvenient position in regard to the light and the eye, they are only visible, without affording any lively colours: which seems to argue, that this dust, or minute fragments of several sorts of bodies reputed opake, and only crumbled into dust, do not barely reflect the rays that fall upon them, but reflect them to the eye refracted.

We may also observe, that several bodies, which pass for opake, appear in great measure transparent, when reduced into thin parts, and held against a strong light. This I have not only taken notice of in pieces of ivory reduced to thickish leaves, in many considerable thick shells of fish, and in shavings of wood; but have also found, that a piece of very thick deal purposely interpos'd betwixt my eye and the clear day-light, appear'd quite thorough of a lovely red. And in the darkned room above-mentioned, bodies held against the hole at which the light enter'd, appear'd far less opake than they would elsewhere have done: so that I could easily and plainly see, thro' the whole thickness of my hand, the motions of a body placed at a very small distance beyond it. And even in minerals, the opacity is not always so exceeding great, if the body be made thin; for white marble, tho' of a pretty thickness, being within a due distance placed betwixt the eye and a convenient light, will suffer the motions of one's finger to be well discern'd through it; and so will thick pieces of many common flints. But, above all, that instance afforded us by *Muscovy* glass is remarkable; for tho' plates of this mineral of a moderate thickness often appear opake, yet if one of these be dexterously split into the thinnest leaves 'tis made up of, it will yield such a number of them, as scarce any thing but experience could have induced me to believe. These leaves afford the most transparent sort of solid bodies that, for ought I have observed, are yet known: and a single plate of it will be so

bodies, have different degrees both of flexibility and refrangibility; and that the mixture of all the coloured rays, into which the sun's light is thrown by the prism, makes white light; and therefore, that all homogeneous light, that is, light whose rays are all alike refrangible, have a peculiar colour answering to their degree of refrangibility; that this is unalterable by any farther reflections or refractions whatever; that the sun's light is composed of all the primary colours to-

gether; and that compound colours proceed from a mixture of the primary. Hence it follows, that no colours can arise from any new modifications of light. But Sir Isaac is of opinion, that the permanent colours of bodies are owing to the disposition of them to reflect some one sort of rays, as red, blue, &c. more plentifully than others. Hence red bodies appear most beautiful in red light, blue bodies in blue light, &c.

far

PHYSICS. far from being opaque, that 'tis scarce so much as visible. And multitudes of bodies there are, whose fragments seem opaque to the naked eye, which yet, when included in good microscopes, appear transparent. On the other hand, there may, perhaps, be some bodies, whose minute particles, even in an excellent microscope, will not appear diaphanous. For upon viewing, by the help of a good glass, mercury precipitated *per se*, the little granules that made up the powder, appeared like little fragments of coral beheld at a distance, with the naked eye. Filings of steel likewise, and copper, tho' viewed in an excellent microscope, on a fair day, shew'd like pretty large fragments of those metals, and appear'd considerably bright on some of their surfaces; yet I was not satisfy'd that I perceived reflections from the inner parts of any of them. Nay, looking thro' my best microscope upon the red calx of lead, neither I, nor any to whom I shew'd it, could discern it to be other than opaque, tho' the day was clear, and the object strongly enlighten'd. And even calcined vitriol, tho' deeply red, appeared in the same microscope like coarse brick-dust. Nor would I be forward to determine how far, or in what cases the transparency or semi-diaphaneity of the superficial corpuscles of larger bodies may have an interest in the production of their colours; especially since even in several white substances, as beaten glass, snow, and froth, where it seems manifest that the superficial parts are singly diaphanous, we see no such variety of colours as is usual from the refraction of light in those bodies, when by their bigness, shape, &c. they are properly qualified to exhibit the colours of the rain-bow and prismatic glass †.

By

† For a clear account of opacity and transparency, let us hear the excellent Sir *Is. Newton*. The opacity of bodies proceeds (according to that great Philosopher) from the multitude of reflections caused in their internal parts, by a discontinuity of them; for “ between the parts of opaque and coloured bodies are many spaces, either empty, or replenished with mediums of other densities.—And that this discontinuity of parts is the principal cause of the opacity of bodies, appears from considering, that opaque substances become transparent by filling their pores with any substance of nearly equal density with their parts. Thus paper dipt in water or oil, the *Oculus mundi* stone steeped in water, linen cloth oil'd or varnish'd, and many other substances soaked in such liquors as will intimately pervade their little pores, become, by that means, more transparent than otherwise: so, on the contrary, the most transparent substances may,

“ by evacuating their pores, or separating their parts, be render'd sufficiently opaque; as Salts, or wet paper, or the *Oculus mundi* stone, by being dried; horn, by being scraped; glass, by being reduced to powder, or otherwise, flaw'd; turpentine, by being stirred about with water, till they mix imperfectly; and water, by being form'd into many small bubbles, either alone, in the form of froth, or by shaking it together with oil of turpentine, or oil of olive, or with some other convenient liquor, wherewith it will not perfectly incorporate. And to the increase of the opacity of these bodies, it conduces something, that the reflexions of very thin, transparent substances are considerably stronger than those made by the same substances of a greater thickness.” *Newton. Optic.* p. 223, 224.

Sir *Isaac Newton* farther observes, that the parts of bodies, and their interstices, must not be less than of some definite big-

By what has hitherto been delivered we may be assisted to judge of that famous controversy anciently held, betwixt the atomists on one side, and most other philosophers on the other side; the former denying bodies to be coloured in the dark, and the latter maintaining colour to be an inherent quality, as well as figure, hardness, weight, &c. For, if colour be only light modify'd, how can it subsist in the dark? But if colour be considered as a certain constant disposition of the superficial parts of the object, to disorder the light they reflect after a determinate manner; as this modifying disposition perseveres in the object, whether it be enlighten'd or not, there seems no just reason to deny, that, in this sense, bodies retain their colour as well in the night as in the day. Or it may, upon this supposition, be otherwise said, that bodies are potentially coloured in the dark, and actually coloured in the light.

But 'tis of greater moment in the inquiry into the nature of colours, to decide that controversy, whether those of the rain-bow; those often seen in clouds before the rising, or after the setting of the sun; and, in a word, whether those other colours, usually called emphatical, ought to be accounted true: for it being generally granted, and may easily be proved, that emphatical colours are light modify'd chiefly by refractions, with

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Whether objects
are coloured in
the dark?

Whether emphatical
colours be
real or imagina-
ry?

bigness to render them opaque and coloured; "for the opakest bodies, if their parts be subtilly divided, become perfectly transparent. Thus water, salt, glass, stones, and such like substances, are transparent; for, upon divers considerations, they seem to be as full of pores or interstices between their parts as other bodies are; but yet their parts and interstices seem to be too small to cause reflexions in their common surfaces." *Newton. Optic. p. 225.*

"The transparent part of bodies, says Sir Isaac Newton, according to their several sizes, reflect rays of one colour, and transmit those of another, on the same grounds that thin plates or bubbles do reflect or transmit those rays;" and this Sir Isaac takes to be the ground of all their colours: "for if, says he, a thinned or plated body, which being of an even thickness, appears all over of one uniform colour, should be slit into threads, or broken into fragments, I see no reason why every thread or fragment should not keep its colour; and, by consequence, why a heap of those threads or fragments, should not constitute a mass or powder of the same colour which the plate exhibited before it was

"broken. And the parts of all natural bodies being like so many fragments of a plate, must, on the same grounds, exhibit the same colour. Now that they do so, will appear by the affinity of their properties. The finely coloured feathers of some birds, and particularly those of peacocks tails, do, in the very same part of the feather, appear of several colours in several positions of the eye, after the very same manner that thin plates do. And therefore their colours arise from the thinness of the transparent parts of the feathers; that is, from the slenderness of the very fine hairs, or *Capillamenta*, which grow out of the sides of the grosser lateral branches or fibres of those feathers. And to the same purpose it is, that the webs of some spiders, by being spun very fine, have appeared coloured; and that the coloured fibres of some silks, by varying the position of the eye, do vary their colour. Also the colours of silks, cloths, and other substances which water or oil can intimately penetrate, become more faint and obscure, by being immersed in those liquors, and recover their vigor again, by being dried after the manner of thin bodies. Leaf-gold, some

PHYSICS. with the concurrence sometimes of reflections, and perhaps other accidents depending on them both; if these emphatical colours be resolved genuine, it will seem to follow, that colours, or at least many of them, are but diversify'd light, and not such real and inherent qualities as men commonly suppose.

Now since we allow echoes, and other sounds of bodies, to be true sounds, all their odours to be true odours, and in short, other sensible qualities to be true, because they are the proper objects of some of our senses; I see not why emphatical colours, being the proper and peculiar objects of the organ of sight, and capable to affect it as truly and as powerfully as other colours, should be reputed imaginary. And if we have proved, that colour, when taken in its more proper sense, is only modify'd light, there will be little reason to deny, that these are true colours, which more manifestly than others shew themselves to be produced by the diversifications of light. There is indeed a difference taken notice of betwixt these apparent colours, and those that are usually esteemed genuine, as to their duration; which has induced some learned men to call the former rather evanid than fantastical. But, as *Gassendus* observes, if this way of arguing were true, the greenness of a leaf ought to pass for apparent, because soon fading into a yellow, it cannot

“ some sorts of painted glass, the infusion
 “ of *Lignum Nephriticum*, and some other
 “ substances, reflect one colour, and trans-
 “ mit another, like thin bodies also. And
 “ some of those coloured powders which
 “ painters use, may have their colours a
 “ little changed, by being very elabo-
 “ rately and finely ground. Where I see
 “ not what can be justly pretended for
 “ those changes, besides the breaking
 “ of their parts into less parts, by that
 “ contrition, after the same manner that
 “ the colour of a thin plate is changed,
 “ by varying its thickness: for which
 “ reason also it is, that the coloured flow-
 “ ers of plants and vegetables, by being
 “ bruised, usually become more transpa-
 “ rent than before; or at least in some
 “ degree or other change their colours.
 “ And thus, by mixing various liquors,
 “ very odd and remarkable productions
 “ and changes of colours may be effected,
 “ of which no cause can be more obvious
 “ and rational, than that the saline cor-
 “ puscles of one liquor do variously act
 “ upon, or unite with, the tinging cor-
 “ puscles of another, so as to make them
 “ swell or shrink, (whereby not only
 “ their bulk, but their density also may
 “ be changed) or to divide them into

“ smaller corpuscles, (whereby a coloured
 “ liquor may become transparent) or to
 “ make many of them associate into one
 “ cluster, whereby two transparent li-
 “ quors may compose a coloured one.
 “ For we see how apt those saline men-
 “ strua are to penetrate and dissolve sub-
 “ stances to which they are applied; and
 “ some of them to precipitate what others
 “ dissolve. In like manner, if we consi-
 “ der the various phenomena of the at-
 “ mosphere, we may observe, that when
 “ vapours are first raised, they hinder not
 “ the transparency of the air; being di-
 “ vided into parts too small to cause any
 “ reflexion in their superficies. But
 “ when, in order to compose drops of
 “ rain, they begin to coalesce, and consti-
 “ tute globules of all intermediate sizes,
 “ those globules, when they become of a
 “ convenient size to reflect some colours,
 “ and transmit others, may constitute
 “ clouds of various colours, according to
 “ their sizes. And I see not what can be
 “ rationally conceived in so transparent
 “ a substance as water, for the production
 “ of these colours, besides the various sizes
 “ of its fluid and globular parcels.” *Newton*.
Optic. p. 226—228.

be compared, in point of duration, with the greenness of an emerald. But if the sun-beams pass in a convenient manner thro' a glass prism, so as to be thrown upon some well shaded object, within a room; the rainbow thereby painted on the surface of a body that terminates the rays, may often last longer than some colours I have produced in certain bodies: which colours would justly, and without scruple, be accounted genuine, tho' they suddenly degenerate, and lose their nature.

A greater disparity betwixt emphatical colours, and others, may, perhaps, be urged; because genuine colours seem to be produced in opaque bodies, by reflection; but apparent ones, in diaphanous bodies, principally, by refraction: for, in some cases, reflection, also, may concur. But still this seems not to prove, that these latter colours are false ones; nor must what we lately said of the differences of true and apparent colours, be understood in too unlimited a sense: for if water be agitated into froth, it exhibits a white colour, which it soon after loses upon the resolution of the bubbles into air and water; in which case, the whiteness of the froth is either a true colour, or not. If it be true, then true colours, supposing the water pure, may prove as short-lived as those of the rainbow; and the matter wherein the whiteness resided, may, in a few moments, be perfectly deprived of it. Besides, even diaphanous bodies may be capable of exhibiting true colours, by reflection; for that whiteness is so produced, will shortly appear. But if it be said, that the whiteness of froth is an emphatical colour; then it must no longer be said, that fantastical colours require a certain position of the illuminating body and the eye; and must be varied or destroyed by the change thereof: for froth appears white, whether the sun be rising or setting; in the meridian, or any where between that and the horizon; and in what part soever the spectator's eye is placed. And since, by making a liquor tenacious, without destroying its transparency, or staining it with any colour, we can give the little films whereof the bubbles consist, such a texture as will make the froth last for many hours, or days, together; it seems improper to assign duration for the distinguishing character of genuine from fantastic colours: for such froth may long out-last the true colours of some natural productions. Thus in that gaudy plant, the marvel of *Peru*, the flowers often fade on the very same day they are blown. And I have often seen a *Virginia* flower, which usually withers within the compass of a day. I am also credibly inform'd, that a curious neighbouring botanist has a plant whose flowers perish in about an hour's time.

But if the whiteness of water, turn'd into froth, must therefore be reputed emphatical, because it appears not that the nature of the body, but only that the disposition of its parts, with regard to the incident light, is changed; why may not that whiteness be accounted emphatical too, which I shall shortly shew to be producible, barely by such another change in black horn? And yet this whiteness, which is so easily acquir'd, seems to be as truly its colour as blackness was before; and is, at least, more permanent than the greenness of leaves, the redness of roses, or the ge-

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nuine colours of most natural productions. It may, indeed, be further objected, that, according as the sun or other luminous body changes place, these emphatical colours alter, or vanish. If a piece of cloth, in a draper's shop, where the light is seldom primary, be variously folded, it will appear of different colours, as the parts happen to be more illuminated or shaded; and if you stretch it flat, it will commonly exhibit some one uniform colour; yet these are not reputed emphatical: so that the difference seems to be chiefly this, that in the case of the rain-bow, and the like, the position of the illuminating body varies the colour; and, in cloth, the position of the object does it. Yet I am not forward to allow, that in all cases the apparition of emphatical colours requires a determinate position of the eye; for if men will have the whiteness of froth emphatical, we have already seen what may be inferred from thence. Besides, the sun-beams transmitted thro' a triangular prism of glass, after the manner lately mentioned, will, upon the body that terminates them, paint a rain-bow, visible to the eye, whether placed on the right hand of it, or on the left; above it, or beneath it; before it, or behind it: and tho' there may appear some little variation in the colours of the rain-bow, beheld from different parts of the room; yet the like may be observed by an attentive eye, in real colours, view'd under the like circumstances. Nor will it follow, that, because there remain no footsteps of the colour upon the object, when the prism is removed, that therefore the colour was not real; since the light was truly modify'd by the refraction * and reflection it suffered in passing thro' the prism: and the object, in our case, served as a speculum to reflect that colour to the eye. For a rough and coloured object may serve for a speculum to reflect the artificial rain-bow we speak of; since, in darkned rooms, a wall, conveniently situated within, will so reflect the colours of bodies external to the room, that they may very clearly be discern'd and distinguished: and yet it is taken for granted, that the colours seen in a darkned room, tho' they leave no traces upon the wall or body that receives them, are the true colours of the external objects; together with which the colours of the images are moved, or rest. And the error lies not in the eye, whose office is only to perceive the appearance of things, and truly does so; but in the judgment that, by mistake, concludes the colour belongs to the wall, which really belongs to the object; because the wall is that from whence the rays of light which carry the visible species, come in strait lines directly to the eye. And thus, for the same reason, we suppose at a certain distance from concave spherical glasses,

* We must again repeat, that Sir Isaac Newton demonstrates, all the colours in the universe, which are made of light, and depend not on the power of imagination, are either the colours of homogeneal lights, or compounded of them in a certain proportion. "For the changes

" of colour, made by refraction, do not
 " arise from any new modification of the
 " rays impressed by those refractions;
 " and, by the various terminations of
 " light and shadow; as has been the
 " constant and general opinion of philosophers." *Newton. Optic.* p. 99. & p. 138.
 that

that we see the image stand out to meet us, and hang in the air betwixt the glass and us; because the reflected rays, that compose the image, cross one another in that place where the image appears to be; and thence in direct lines take their course to the eye. I might here add what will more fully appear hereafter, that colours, called emphatical, because not inherent in the bodies where they appear, may be compounded with one another, as those that are confessedly genuine.

S E C T. II.

I Come now to inquire into the nature of whiteness and blackness. Whiteness, considered as a quality in the object, seems, in the general, chiefly to depend upon the roughness of the surface of the body, call'd white; which gives it innumerable small superficies, that acting like so many little specula in various positions, they reflect the rays of light that fall on them, not towards one another, but externally towards the spectator's eye. The sun, and other very lucid bodies, not only offend or dazzle our eyes; but if any colour is ascribed to them, it should be whiteness: * for the sun at noon-day, and in clear weather, when his face is serene,

The nature of whiteness.

* White, in strictness, we have already observed to be no simple colour; but a compound of all colours in a due proportion. "When the several sorts of rays are mixed, says Sir Isaac Newton, and, in crossing, pass thro' the same space, they act not upon one another, so as to change their colour-making qualities; but, by mixing their actions in the sensorium, beget a sensation, differing from what either would do a-part: that is, a sensation of a mean colour between their proper colours. And particularly, when, by the concurrence and mixtures of all sorts of rays, a white colour is produced; the white is a mixture of the colours which the rays would have a-part: the rays in that mixture do not alter their several colour-making qualities; but, by all their various kinds of actions, mixed in the sensorium, beget a sensation of a middling colour, between all their colours, which is whiteness. For whiteness is a mean between all colours; having itself indifferently to them all; so as with equal facility to be tinged with any of them. A red powder mixed with a little blue, or a blue with a little red, doth not presently

lose its colour; but a white powder, mixed with any colour, is presently tinged with that colour, and is equally capable of being tinged with any colour whatever." See Newton. Optic. p. 117, 118: 138, 139.

We farther learn from Sir Isaac Newton, that as a mixture of all the prismatic colours makes perfect white light; so if soapy water be agitated into a froth, various colours will appear therein, when viewed near; and, at a distance, when those colours can no longer be distinguished, the froth will seem perfectly white. But, by mixing coloured powders, he tells us, we are not to expect a strong and full white, but some dusky obscure one; because they suppress and stop in them a very considerable part of the light by which they are illuminated. Thus he produced a dark white, or a dun colour, by mixing one part of red lead with five parts of verdigrease; for these two colours were severally so compounded of others, that, in both together, there was a mixture of all colours.

Again, one part of red lead, and four parts of blue bise, composed a dun colour, varying a little to purple; and, by adding to it a certain mixture of orpiment

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ferene, and his rays pass thro' a much less part of the atmosphere to our eyes, appears of a colour more approaching to white than when he is nearer the horizon; in which case the interposition of certain fumes and vapours make him oftentimes appear either red or yellow. And when the sun shines upon smooth water, that part of it which appears most illumined, seems far whiter than the rest. And I have sometimes found, that when the sun was veil'd with a thin white cloud, tho' still too bright to be look'd upon directly; that by casting my eyes upon still water, his body, being not far from the meridian, appeared to me exceedingly white. And tho' we vulgarly say, in *English*, a thing is red-hot, to express a superlative degree of heat; yet, at the forges and furnaces of artificers, by a white heat they understand a further degree of ignition, than by a red one.

2. Common experience informs us, that as too much light overpowers the eye; so when the ground is covered with snow, those who have a weak sight, complain that this prospect is offensive to them. And even those who have good eyes, are from hence generally sensible of an extraordinary light in the air; and, if obliged to look very long upon the snow, find their sight injured by it. Thus *Xenophon* relates, that *Cyrus* marching his army, for many days, over mountains covered with snow, the dazzling splendour of its whiteness prejudiced the sight of a great number of his soldiers, and blinded some of them; and other accounts of the same nature may be met with in writers of good note. The like has also been affirmed to me by credible persons of my own acquaintance, and especially by one, who, during

ment and verdigrease, in due proportion, it lost its purple tincture, and became perfectly dun. But the experiment succeeded best without red lead, thus: "To orpiment, says Sir *Isaac*, I added, by little and little, a certain full bright purple, which painters use, till the orpiment ceased to be yellow, and became of a pale red: then I diluted that red, by adding a little verdigrease and a little more blue bise than verdigrease, till it became of such a grey or pale white, as varied to no one of the colours more than to another. For thus it became of a colour equal in whiteness to that of ashes, or of wood newly cut, or of a man's skin. The orpiment reflected more light than did any other of the powders; and therefore conducted more to the whiteness of the compounded colour than they. To assign the proportions accurately, may be difficult, by reason of the different goodness of powders of the same kind. As the co-

"lour of any powder is more or less full and luminous, it ought to be used in a less or greater proportion. Now considering that these grey and dun colours may be also produced, by mixing whites and blacks; and, by consequence, differ from perfect whites, not in species of colours, but only in degree of luminousness; it is manifest that there is nothing more requisite to make them perfectly white, than to increase their light sufficiently; and, on the contrary, if, by increasing their light, they can be brought to perfect whiteness; it will thence also follow, that they are of the same species of colour with the best whites; and differ from them only in the quantity of light." And accordingly, by placing some powder, composed of orpiment, purple, bise, and verdigrease, in the sun's rays; and, viewing it at a distance, it appeared intensely white. *Newton. Optic.* p. 129—134.

his

his stay in *Muscovy*, found his eyes much impaired, by being frequently obliged to travel in the snow: and this weakness of sight did not leave him when he left that country, but still continues, tho' he be a young man. I myself also, as well as others, have observed, that upon travelling by night, when the ground was all covered with snow, tho' it would otherwise have been dark, yet we could very well see to chuse our way. But much more to my present purpose is that account given us by *Olaus Magnus* of a way of travelling in the *Northern* regions during the winter, where the days of that season are very short. "In the day-time," says he, they travel twelve *Italian* miles, but twice or thrice as far in the night, and that with ease; for the light of the moon, reflected by the snow, renders both hills and vales conspicuous; so that then they can see not only precipices afar off, but the wild beasts they would avoid."

This testimony I the less scruple to alledge, because it agrees very well with what has been affirmed to me by a physician of *Moscow*; who informed me, that he could see things at a far greater distance, and with more clearness, when he travelled by night on the snow there, tho' without the assistance of the moon-shine, than we, in these parts, would easily believe. 'Tis true, indeed, the intenseness of the cold might contribute something to the considerableness of the effect, by clearing the air of dark steams, which, in these more temperate climates, are usually thick in snowy weather: for this physician, and the ingenious navigator Captain *James* agree, that in dark frosty nights, in frozen climates, they could discover more stars, and have a clearer prospect of the heavens, than we in *England*. I know, indeed, many learned men suppose snow thus strongly affects our eyes, not by a borrowed, but a native light. I venture, however, to give it as a proof, that white bodies reflect more light than others, because having once placed a parcel of snow in a room, carefully darkened, that no celestial light might fall upon it, neither I, nor an ingenious person skilled in optics, could find it had any light besides what it received; and 'tis usual, among such as travel in dark nights, to make their guides wear something of white to be discern'd by; for there is scarce any night so dark, but that, in the free air, some light remains, tho' broken and debilitated, perhaps, by a thousand reflections from the opaque corpuscles, that swim in the atmosphere, and send it to one another before it arrives at the eye.

3. And the better to shew that white bodies reflect much more light than others, I held in the darkened room, formerly mentioned, not far from the hole at which the light entered, a sheet of white paper; from whence casting the sun-beams upon a white wall, it manifestly appeared both to me, and to a person I took to be witness of the experiment, to reflect a far greater light than any of the other colours; for the wall itself was not only thus notably enlightened, but also a considerable part of the room. And, further, to shew that white bodies reflect the rays outwards, let me add, that ordinary burning-glasses will not, in a great while, burn or discolour white paper; so that when I was a boy, and delighted to make

PHYSICS. make trials with such glasses, I could not but wonder at this odd phenomenon, which set me very early upon guessing at the nature of whiteness; and the more, because I took notice that the image of the sun, upon white paper, was not so well defined as upon black; and because that, upon inking over the paper, the moisture would be quickly dried up, and the paper, that I could not burn before, would now presently take fire. I have also try'd, that by exposing my hand, with a thin black glove on it, to the warm sun, it would thereby very suddenly be more considerably heated, than if I took off the glove, and held my naked hand to its rays, or put on another glove of thin white leather.

4. And to shew that white bodies are apt, like specula, to reflect the light that falls on them, we have found, in a darken'd room, that the sun-beams being cast from a coloured body upon a white wall, the determinate colour of the body was from the wall reflected to the eye; whilst we could, in many cases, manifestly alter the colour arriving at the eye, by substituting, at a convenient distance, a colour'd glossy body instead of the white wall; thus by throwing the rays from a yellow body upon a blue, there would be exhibited a kind of green. I might also here take notice, that when looking upon the calm and smooth surface of a river, lying betwixt my eye and the sun, it appeared to be a natural speculum, wherein that part, which reflected to my eye the entire and defined image of the sun and rays adjacent thereto, appeared of a great whitish brightness, but the rest comparatively dark; when, if afterwards the superficies chanced to be a little ruffled by a gentle breath of wind, and thereby reduced into a multitude of small, smooth specula, the surface of the river would, at a distance, appear almost white; tho' it lost that appearance upon a return of the surface to a calm and uniform level. And I have sometimes, as an experiment, brought in a lenticular glass, the image of a river illumined by the sun, into a darkened room, at about the distance of a quarter of a mile; by which means the numerous declining surfaces of the water were so contracted, that, upon the body which received the image, the whole river appeared a very white object at two or three paces distance. But if we drew near it, this whiteness seemed to proceed from innumerable lucid reflections, made by the superficies of the water gently waved, which looked, when closely viewed, like a multitude of very little shining scales of fish; many whereof every moment disappeared, while as many were by the sun, wind, and river, generated anew. But tho' this observation seemed sufficiently to discover how the apparent whiteness, in that case, was produced; yet in some other cases, water may have the same, tho' not so vivid a colour, upon other accounts; for it often happens that the smooth surface of the water appears bright or whitish, by reason of the reflection, not immediately of the image of the sun, but of the brightness of the sky; and in such cases a convenient wind may, where it passes along, make the surface look black, by causing many such furrows and cavities, as make the inflected superficies of the water reflect the brightness of the sky, rather internally

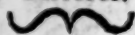
ly than externally. And again, if the wind increase into a storm, the water may appear white, especially near the shore, because the rude agitation breaks it into foam or froth; so much do whiteness and blackness depend upon the disposition of the superficial parts of a body, to reflect the rays of light inward or outward. But that as white bodies reflect the most light of any; so their superficial particles are of a specular nature, I shall further endeavour to shew, by making specular bodies white, and a white body specular.

5. Upon distilling quicksilver in a cucurbit, fitted with a capacious glass-head, I have observed, that when the operation was performed by proper degrees of fire, there would stick to the inside of the alembic a multitude of little round drops of mercury. And as mercury is a specular body, each of these little drops was a small spherical looking-glass; and a number of them lying near one another, made the glass they were fastened to, manifestly appear a white body. And as many parts of the sky, especially the milky way, appear white to the naked eye; yet the galaxy, viewed thro' a telescope, does not appear white, but to be made up of a vast multitude of little stars; so, many lucid bodies, if too small to be singly discerned by the eye, and set sufficiently thick by one another, may, by their united rays, appear to the eye as one white body: and why may not the like happen, when a multitude of bright, little corpuscles, crowded together, are made jointly to reflect vivid rays to the eye, tho' they shine by a borrowed light?

But to return to our experiments; we may take notice, that the white of an egg, which tho' in part transparent, yet, by its power of reflecting some incident rays of light, is, in some measure, a natural speculum, being long agitated with a whisk or spoon loses its transparency, and becomes a very white froth; that is, an aggregate of numerous small bubbles, whose convex superficies fit them to reflect the light every way outwards. And 'tis worth observing that when water, for instance, is agitated into froth, if the bubbles be great and few, the whiteness will be but faint, because the number of specula within a narrow compass is but small; and they are not thick enough set to reflect so many little images or rays of the lucid body, as go to produce a vigorous sensation of whiteness. And lest it should be said, that the whiteness of such globular particles proceeds from the air included in the froth, (tho' who can prove that the air itself is white?) and at the same time to illustrate our doctrine of whiteness, I shall add this experiment. I put to some fair water, contained in a glass vial, a convenient quantity of spirit of turpentine, which will not incorporate with water, yet is almost as clear and colourless as that; and these being well shaken together, I found the agitation broke the oil into a multitude of little globes, which each of them reflecting outwards a lucid image, made the imperfect mixture of the two liquors, appear whitish; but if by vehemently shaking the glass for a competent time, a further comminution of the oil be made into far more numerous and smaller globules, whilst it is also thereby more thoroughly confounded

PHYSICS. confounded with the water, the mixture will appear of a much greater whiteness, and almost like milk; tho' if the glass be let alone awhile, the colour will gradually fade as the oily globules grow fewer and larger, and at length quite vanish; leaving both the liquors distinct and diaphanous as before. And such a trial hath succeeded, when instead of oil, or spirit of turpentine, I took a yellow mixture, made with a large proportion of crude turpentine, dissolved in that liquor; and it also succeeded better than one would expect, when I employ'd an oil brought to a deep green, by infusing copper filings therein. Thus aromatics, distilled with water, often yield a white liquor, which may long continue of that colour; because if the fire be made too strong, the subtle oil is thereby much agitated, broken, and blended with the water, in such numerous and minute globules, as cannot easily, in a short time, emerge to the top of the water, and, whilst they remain therein, make it look whitish. And hence, perhaps, it is, that we find hot water usually more opaque and whitish than the same when cold; the agitation turning the more volatile particles of the water into vapours, and thereby producing, in the body of the liquor, a multitude of small bubbles, which interrupt the free passage of the rays of light, and from the innermost parts of the water, reflect many of them outwards. These, and the like examples, have induced me to suspect, that the superficial particles of white bodies, may, for the most part, be as well convex as smooth; tho' it seems not easy to prove, that when diaphanous bodies are reduced into white powders, each corpuscle must needs be of a convex superficies; and, perhaps, it may here suffice that the specular surfaces look several ways. We have seen that when a diaphanous body is reduced to very minute parts, it thereby acquires a multitude of little surfaces within a narrow compass; and tho' each of those should not be of a convenient figure to reflect a round image of the sun, they may reflect some physical line of light, wherein some refraction of that which falls upon the body, whereon it depends, may often contribute to its whiteness. Thus if a slender wire, or solid cylinder of glass, be exposed to luminous rays, you shall see, in some part thereof, a vivid line of light; and if we were able to draw out and lay together a multitude of these little wires or threads of glass, so slender, that the eye could not discern a distance between the luminous lines, there is no doubt, as far as I can guess by a trial of this kind purposely made, that the whole physical superficies, composed of them, would appear white to the eye; and if so, it is not always necessary that the figure of those corpuscles, that make a body appear white, should be spherical: and snow itself commonly appears both to the naked eye, and when viewed thro' a microscope, to consist principally of little slender icicles of several shapes, which afford such numerous lines of light as we speak of.

6. If you take a diaphanous body, as, for instance, a piece of glass, and reduce it to powder, the same body which, when entire, freely transmitted the rays of light, acquiring, by contusion, a multitude of minute surfaces, each of which is, as it were, a little speculum, becomes there-
by



by qualified to reflect, in a confused manner, so many rays, or little and singly unobservable images of the lucid body, that from a diaphanous, it degenerates into a white substance. And heating lumps of crystal red-hot, in a crucible, I have found, that, upon quenching 'em in fair water, even those which remained seemingly entire, exchanged their transparency for whiteness; the ignition and extinction having cracked each lump into a multitude of minute bodies, and thereby given it a multitude of new surfaces. And even with coloured diaphanous bodies, there may, by this way, be a great degree of whiteness produced. I have, by confusion, obtained whitish powders from granats, glass of antimony, and emeralds; but the experiment is more easily made, by comparing deep blue pulverized vitriol of copper, with some of the entire crystals of the same, for this will, comparatively, exhibit a considerable degree of whiteness.

7. And as by a change of position in the parts of differently coloured bodies, they may be rendered white; so by a slight change, in the texture of its surface, a white body may be deprived of that property. A piece of silver newly boiled, with salt and tartar, after the goldsmith's fashion, is of a lovely white; but if, with a piece of smooth steel, a part of it be burnish'd, that part presently loses its whiteness, and turns to a speculum, almost every where dark, like other mirrors; which adds a great confirmation to our doctrine. For hence we see what it chiefly is that made the body white before; since all that was done to deprive it of that whiteness, was only to depress the little irregular protuberancies, that stood out on the surface of the silver, into one continued regular plain.

8. What we have said of whiteness may assist us to form a notion of blackness; those two qualities being sufficiently opposite to illustrate each other. And as that which makes a body white, is chiefly such a disposition of its parts, as disposes it to reflect more of the light that falls on it, than bodies of different colours; so that which renders a body black, is principally a peculiar kind of texture of its superficial particles; whereby it damps the light that falls on it, so that very little is reflected to the eye.

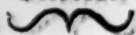
The nature of blackness.

9. This texture is explicable two several ways; and first, by supposing, in the superficies of the black body, a particular kind of asperity; whence the superficial particles reflect few of the incident rays outwards, and the rest inwards, upon the body itself: as if, for instance, the surface of a black body should rise up in numberless little cylinders, pyramids, cones, &c. which, by being thick set and erect, throw the rays of light from one to another inwards, so often, that, at length, they are lost before they can come out again to the eye. The other way supposes the textures of black bodies either to yield to the rays of light, or, upon some other account, to stifle and keep them from being reflected in any number, or with any considerable vigour outwards. According to this notion it may be said, that the corpuscles, which compose the rays of light, thrusting one another from the lucid body, and falling on black substances, meet with such a texture, that they receive into themselves, and retain almost all the

motion

motion communicated to them by those corpuscles, and consequently reflect but few of them, or those but languidly, towards the eye; as when a ball, thrown against a floor, rebounds a great way upwards, but very little or not at all when thrown against mud or water; because the parts yield, and receive into themselves the motion which should reflect the ball back. But this last manner of accounting for blackness I barely propose, without either adopting, or absolutely rejecting it; for the hardness of touchstones, black marble, and of other bodies that are black, and solid, seems to render it somewhat improbable that they should be of so yielding a nature; unless we say that some bodies may be more disposed to yield to the impulses of the corpuscles of light, by reason of a peculiar texture, than others, which, by particular trials, appear to be softer than they. Both the solutions, however, agree in this, that black bodies reflect but little of the light which falls on them. And it is not impossible that, in some cases, both the disposition of the superficial particles as to figure and position, and the yielding of the body, or some of its parts, may jointly contribute to render a body black. The considerations which induced me to propose this notion of blackness, are principally these.

(1.) Whiteness and blackness being generally reputed contrary qualities, whiteness depending, as I said, upon the disposition of the parts of the body, to reflect light plentifully, it seem'd probable, that blackness might depend upon a contrary disposition of surface; but upon this I shall not insist. However, if a body, of an uniform colour, be placed, part in the sun-beams, and part in the shade, that part, which is not illumined, will appear nearer allied to blackness than the other, from which more light is reflected to the eye; dark colours also seem the blacker, the less is the light they are viewed in: and all things seem black in the dark, when they yield no rays to make impressions upon our organs of sight; so that shadow and darkness are near a-kin: and shadow, we know, is but a privation of light; blackness, accordingly, seems to proceed from the want of rays reflected from the black body to the eye; tho' the bodies we call black, as marble, jet, &c. are not perfectly so, for if they were, we should not see them at all. But notwithstanding the rays which fall on the sides of those erect particles we mentioned, do few of them return outwards; yet such as fall upon the points of those cylinders, cones, or pyramids, may be thence reflected to the eye, tho' they make but a faint impression there; because they are mixed with a great proportion of little shades. Thus, having procured a large piece of black marble to be well polished, and brought to the form of a large spherical concave speculum, the inside thereof was a kind of dark looking-glass, wherein I could plainly see a little image of the sun, when it shone thereon. But this image was very far from offending or dazzling my eyes, as it would have done from another speculum; and tho' this were large, I could not, in a long time, set a piece of wood on fire with it; tho' a far less speculum of the same form, and of a more reflecting material, would presently have made it flame. And having exposed to the sun a pretty large mortar



of white marble, polish'd on the inside, we found that it reflected a great quantity of glaring light, but so scatter'd, that we could not make the reflected rays meet in any such conspicuous focus as that we observ'd in the black marble; tho' by holding a candle, in the night-time, at a convenient distance, we were able to procure a concourse of a few reflected rays at about two inches distance from the bottom of the mortar. But we found the heat of the sun-beams so disperfedly reflected, to be very languid, even compared to the focus of the black marble: and the little picture of the sun that appear'd upon the white marble as a speculum, was very faint, and exceedingly ill defined.

(2.) Taking two pieces, the one of black, and the other of white marble, whose surfaces were plain and polish'd; and casting on them successively the rays of the same candle, in such a manner that the adjacent superficies being shaded by an opaque and perforated body, the incident rays pass'd thro' a round hole of about half an inch in diameter, the circle of light that appear'd on the white marble was, in comparison, very bright, but very ill defined; whilst that on the black marble was far less luminous, but much better defined.

(3.) When we look upon a piece of linen that has small holes in it, they appear very black; so that men are often deceiv'd in taking holes for spots of ink: and painters, to represent holes, make use of black; the reason whereof seems to be, that the rays which fall on those holes, penetrate so deep, that none are reflected back to the eye. And in a narrow well, part of the mouth seems black, because the incident rays are reflected downwards, from one side to another, till they can no longer rebound to the eye. We may consider too, that if different parts of the same piece of black velvet be stroak'd opposite ways, there will appear two distinct kinds of blackness, the one far darker than the other; probably because in the less obscure part of the velvet, the little silken piles, whereof 'tis compos'd, being inclined, there is a greater part of each of them turn'd to the eye; whilst in the other part the piles of silk being more erect, there are by far fewer rays sent outwards from the lateral parts of each pile: so that most of those reflected to the eye, come from the tops of the piles, which make but a small part of the whole superficies of the velvet. This explanation I propose, not that I think the blackness of the velvet proceeds from the cause assign'd; since each single pile of silk is black by reason of its texture, in what position soever it be view'd; but because the greater blackness of a single tuft seems to proceed from the greater defect of rays reflected thence, and from the want of those parts of a surface that reflect rays, and the multitude of those shaded parts that reflect none. And I have often observ'd, that the position of particular bodies, far greater than piles of silk, may, notwithstanding each of them hath a colour of its own, make one part of their aggregate appear far darker than another. Thus a heap of carrots appear of a much darker colour when view'd with their points, than with their sides obverted to the eye.

(4.) I have observ'd in a darken'd room, that if the sun-beams which came in at the hole were receiv'd upon white, or any other colour, and directed to a convenient part of the room, they would manifestly increase the light of that part; but if we substituted either a piece of black cloth or black velvet, it would so damp the incident rays, that the said place would be less illumin'd than before, when it receiv'd its light only from the weak and oblique reflections of the floor and walls of a pretty large room; over which the beams that came in at the hole, were confusely and in a broken manner dispers'd.

(5.) And to shew that the rays which fall on black bodies, as they do not rebound outwards to the eye, so they are reflected towards the body itself, as the nature of those erect particles to which we have imputed blackness requires; we shall add an experiment, that will at the same time confirm our doctrine of whiteness. We took, then, a broad and large tile, and having whited over one half of its superficies, and black'd the other, we expos'd it to the summer sun. And having let it lie there a convenient time, we found that whilst the whited part of the tile remain'd cool, the black'd part of it was grown very hot. And for further satisfaction, we have sometimes left upon the surface of the tile a part that retain'd its native red; and exposing all to the sun, we observ'd the latter to have contracted a heat in comparison of the white part, but inferior to that of the black. 'Tis also remarkable, that rooms hung with black are not only darker than they would be otherwise, but warmer too. I have known a great lady, of a tender constitution, complain that she commonly took cold upon going into the air, after having made any long visit to persons whose rooms were hung with black. And this is not the only lady I have heard complain of the warmth of such rooms; which, tho' perhaps it may partly be imputed to the effluvia of those materials where-with the hangings were dyed, yet probably the warmth in this case depends chiefly upon the same cause with darkness; for upon exposing two pieces of silk, the one white, the other black, in the same window to the sun, I have often found the former considerably heated, when the latter has remain'd cool.

(6.) A virtuoso of unsuspected credit acquainted me, that in a hot climate he had, by carefully blacking the shells of eggs, and exposing them to the sun, seen them thereby well roasted, in no long time. But in *England*, the sun's rays seem not to be sufficiently strong to produce such an effect; for having expos'd eggs in the summer season thereto, they acquired indeed a considerable degree of heat, but not enough to roast them.

(7.) Lastly, our conjectures about the nature of blackness, may be somewhat confirmed by the observation of the blind man, formerly mention'd, who discerns colours with his fingers; for he says, that he feels a greater roughness upon the surfaces of black bodies, than upon those of red, yellow, or green. And *Bartholine* tells us, that a blind earl of *Mansfield* could distinguish white from black only by the touch; which might sufficiently argue a great difference in the asperities, or superficial textures of

of the bodies of those two colours; if the learned relator had affirm'd the matter upon his own knowledge. Let us next take in the assistance of our experiments, purposely made to bring us farther acquainted with the nature of white and black.

1. Take any quantity of fair water, heat it, and add thereto as much good common sublimite as it will dissolve, or till some of it lie untouched at the bottom of the liquor; then filter this solution thro' cap-caper, and, to a spoonful or two of the clear, add four or five drops of good limpid spirit of urine: shake them together, and immediately the whole mixture will appear white, like milk. After this, if you presently add a convenient proportion of rectify'd *Aqua fortis*, the whiteness will immediately disappear, and the whole mixture become transparent; which you may, if you please, again reduce to a considerable degree of whiteness, by pouring therein more fresh spirit of urine. It is not necessary to employ either *Aqua fortis*, or spirit of urine, about this experiment; for we have made it with other liquors.

The nature of whiteness and blackness shewn by experiments.

2. Make a strong infusion of bruised galls in fair water; and having filtered it into a clean vial; add more of the same fluid to it, till you have made it somewhat transparent, and sufficiently diluted the colour for the credit of the experiment. In this infusion, shake a convenient quantity of a clear, but very strong solution of vitriol; and you shall immediately see the mixture turn black, almost like ink: and if, presently after, you drop into this mixture a small quantity of good oil of vitriol, and, by shaking the vial, suddenly disperse it thro' the two other liquors; you will see the dark colour of the whole presently begin to dissipate, grow clear, transparent, and lose its inky blackness; which may be again restored by the affusion of a small quantity of a strong solution of salt of tartar. And tho' both these atramentous liquors will seem very pale, if you write with a clean pen dipt in them; yet that is common to them, with some sorts of ink, which prove very good when dry; as I have found, that when these were carefully made, what I wrote with either, especially with the former, would, after a while, turn sufficiently black. This experiment of destroying and restoring blackness, we have likewise try'd in common ink; tho' with this it succeeds not so well, and but very slowly; because the gum usually employ'd in making it, opposes the operations of the saline liquors. And tho' it be taken for granted, that bodies will not precipitate with alkalizate salts, which have not been first dissolved in some acid menstruum; yet I have found, upon trial, that many vegetables, barely infused, or but slightly boiled in common water, afford, upon the bare affusion of a strong and clear lixivium of pot-ashes, a large quantity of coagulated matter; such as I have had in the precipitations of vegetable substances, by means of acids; and that this matter was easily separable from the rest of the liquor; being left behind by it in the filtre. And, from the first ink mentioned in this experiment, I could, by filtration, separate a considerable quantity of a very black pulverable substance. And when the ink was made clear again, by the oil of vitriol, the affusion of

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of dissolved salt of tartar seem'd but to precipitate, and thereby unite, and render conspicuous, the corpuscles of the black mixture, that had been before disperfed, into very minute and singly invifible particles, by the refolving power of the highly corrofive oil of vitriol. And that galls are not abfolutely neceffary to make atramentous liquors, appears from the following experiment. We boil'd dry'd rofe-leaves for a while in fair water, and into two or three fpoons-ful of the decoction, fhook a few drops of a ftrong and well filtred folution of vitriol; whereupon the mixture immediately turn'd black: and prefently fhaking herein a juft proportion of *Aqua fortis*, we changed it from a black to a deep red ink; which, by the affufion of a little fpirit of urine, may be reduced immediately to an opake blackifh colour.

3. In thefe experiments, the infufion of galls, the decoction of rofes, and the folution of vitriol, have each their own colour; but we may fuddenly produce a blacknefs, by mixing an infufion of orpiment, and a folution of minium, both whereof fhall be limpid and colourlefs. And with thefe liquors may be exhibited a curious and furprizing phenomenon, if made and applied in the following manner: 1. Take of the ftrongeft unflaked lime about two parts, of yellow orpiment one part, of fair water fifteen or fixteen parts; beat the lime grofsly, and powder the orpiment, with care to avoid the noxious duft: and having put thefe two ingredients into the water, let them remain there for two or three hours; ftirring the mixture from time to time. Thus you'll obtain a fetid liquor; the clear part whereof muft be poured off from the reft, or gain'd by the filtre. 2. In the mean time burn a piece of cork, and quench it, whilft fired, for feveral times fucceffively in fair water; and having, by this means, reduced it to a coal, you may eafily, by grinding it with a folution of gum-arabic in water, bring it to the colour and confiftence of a good black ink. 3. Take any quantity of red lead, and two or three times its weight of vinegar, or rather the weak fpirit of it; and, putting the powder and that into a glafs vial, let them infufe in fome confiderably warm place for two or three hours, till the liquor has acquired a fweet tafte. Matters being thus prepared, write what you please with a clean pen dipt in the folution of the red lead; which, if filtred, will prove fo clear, as to be invifible upon the paper. Over what is thus written, you may draw any characters or letters you please, with a pen dipt in the black ink made with cork. And, laftly, to fhew the experiment, dip a linen rag in the fetid folution of the lime and orpiment, which is alfo limpid, and draw it over the written paper; and this will at once both wipe out the ftrokes of the black ink, and render all that was wrote with the invifible ink conspicuoufly black.

4. If pieces of white hart's-horn be, with a moderate degree of fire, diftilled in a glafs retort, they will, after the feparation of the phlegm, fpirit, volatile falt, and the loofer and lighter parts of the oleaginous fubftance, remain one behind of a coal-black colour. And even ivory itfelf, when fkilfully burnt, affords painters one of the beft and deepeft blacks they

they have. Yet, in the instance of distill'd hart's-horn, the operation being made in glass vessels, carefully closed, no extraneous black substance insinuates itself into the white horn; but the whiteness is destroyed, and the blackness generated only by a change of texture made in the burnt body by the recess of some parts, and the transposition of others. And tho' I remember not to have ever found the *Caput mortuum* of distilled hart's-horn to pass from a black to a true whiteness, whilst it continued in close vessels; yet, having taken out the coal-black fragments, and calcined them in open vessels, I could, in few hours, quite destroy that blackness, and, without sensibly changing their bulk or figure, reduce them to a degree of whiteness: so much do these two colours depend upon the disposition of the little parts that the bodies wherein they are to be met with, consist of. And we find, that if white-wine tartar, or the white crystals of such tartar, are burnt, without being truly calcined, the *Caput mortuum* will be black. But if the calcination be continued till the tartar is perfectly reduced to ashes, and kept long enough in a strong fire, the remaining calx will be white. And so we see, that not only other vegetable substances, but even white woods, as the hazel, will yield a black charcoal, and afterwards whitish ashes. Thus also animal substances, naturally white, as bones and egg-shells, grow black, upon being burnt, and white again, when perfectly calcined.

5. Yet I much question whether the rule, *adusta nigra, perusta alba*, will hold as universally as is presumed; for I have several examples to allege against it. By burning alabaster so as to make it appear to boil almost like milk, and to reduce it to a very fine powder, it would not grow black at all, but retain its pure and native whiteness: and tho', by keeping it longer than usual in the fire, I produced a faint yellow in that part of the powder which lay nearest the top of the crucible; yet a curious and experienced stone-cutter told me he had found, that if alabaster, or plaister of *Paris*, be very long kept in a strong fire, the whole heap of burnt powder would exchange its whiteness for a much deeper colour than the yellow I observed. Lead calcined with a strong fire, turns at length, to minium, whose colour we know is a deep red; and if this minium be again urged with a strong fire, you will sooner find a glassy brittle body, darker than minium, than any white calx or glass. 'Tis known among chymists, that the white calx of antimony, by a more vehement operation of the fire, may be melted into a glass; which we have obtain'd of a red colour far deeper than that of the calx of burnt antimony: and tho' common glass of antimony, being usually adulterated with borax, have its colour thereby diluted, often to a very pale yellow; yet not only ours, made more genuinely, was, as we said, of a colour less remote from black than the calx; but, by melting it once or twice more, we found the colour heighten'd. And if you burn blue unsophisticated vitriol very slowly, and with a gentle degree of heat, you may observe, that when 'tis burnt only so far as to rub to powder betwixt your fingers, it will be of a white, or whitish colour: but if you pro-

ceed

PHYSICS. cute the calcination, this body will pass thro' other colours, as a grey, a yellowish, and a red: and if you continue it in a long and vehement fire, by that time it comes to be thoroughly calcined, it will be of a dark purple, nearer to black, not only than the first calx, but than the vitriol, before it at all felt the fire. I might add, that *Crocus Martis*, made by the lasting violence of the reverberated flames, is not so near to white as the iron or steel that afforded it, before its calcination.

These instances may suffice to shew, that minerals are to be excepted from the foremention'd rule; which, tho' it seldom fails in substances belonging to the animal or vegetable kingdom, may yet be suspected even in some of these, if *Bellonius* say true, that charcoal, made of the wood of oxycedar, is white. And I could not find, tho' hart's-horn, and other white bodies, will turn black in retorts, by heat, that camphire would at all lose its whiteness; tho' I have purposely kept it in such a heat as made it melt and boil.

6. And tho' I could not, in closed glasses, blacken camphire by heat, but it would sublime to the sides and top of the vessel in its natural form; yet being set on fire in the free air, it sends out a great smoke: and having, purposely, upon some of it, whilst flaming, clapt a large glass, almost in the form of a hive, with a hole at the top, it continued burning, so as to line all the inside of the glass with a foot as black as ink; and in so great a quantity, that the closeness of the vessel consider'd, almost all that part of the camphire which took fire, seem'd to have been changed into that deep black substance.

7. I took rectified oil of vitriol, and, by degrees, mixed with it a convenient proportion of the essential oil of wormwood; and, warily distilling the mixture in a retort, there remain'd a scarce credible quantity of dry matter, black as a coal. And because the oil of wormwood, tho' a chymical oil, and drawn by a virtuoso, seem'd to have somewhat in it of the colour of the plant, I substituted, in its room, the pure and subtile oil of winter-savory; and gradually mixing it with an equal weight of the same oil of vitriol, and distilling them, as before, in a retort, even these two clear liquors left me a considerable proportion of a substance black as pitch; which I keep by me as a rarity.

8. Take a little yellow wax, scraped, or thinly sliced, and putting it into a convenient glass, pour to it a considerable quantity of spirit of wine; and, placing the vessel in warm sand, increase the heat by degrees, till the spirit of wine just begins to boil; and by continuing that degree of heat, you will quickly find the wax dissolv'd: then taking it off, you may either suffer it to cool as hastily as with safety to the glass it can, or pour it, whilst yet hot, into a filtre of paper; and either in the glass where it cools, or in the filtre, you will soon find the wax and menstruum together reduced into a white substance almost like butter; which, by letting the spirit exhale, will shrink into a much less bulk, but still retain its whiteness. 'Tis a pretty phenomenon in working of this magistry of wax, that the yellowness vanishes, and neither appears in the
spirit

spirit of wine that passes limpid thro' the filtre, nor in the butter, if I may so call it, which is wax well and suddenly blanched.

9. We took two parts of common sublimate, and one of tin-glass, both finely powdered, and exactly mixed; these we sublimed together, by proper degrees of fire, and there ascended a matter of a very peculiar texture, for the most part made up of very thin, smooth, soft, slippery plates, almost like the finest sort of fish-scales; but of so lovely a white, inclining to pearl-colour, and of so curious and shining a gloss, that they appeared, in some respects, little inferior to orient pearls, and, in others seem'd to surpass them. They were applauded for a sort of the prettiest trifles we had ever prepared to amuse the eye. I will not undertake, that tho' a man can hardly miss changing the colour of the tin-glass, that he shall at the first, or perhaps the second trial, hit upon the right way of making this glittering sublimate.

10. When, in *Aqua fortis*, we dissolve a mixture of gold and silver, melted into one mass, it usually happens, that the powder of gold which falls to the bottom, as not being dissoluble by that menstruum, will not retain its own yellow, but appear of a black colour; tho' neither the gold, the silver, nor the *Aqua fortis* did before manifest any blackness. And many chymists, when they make solutions of minerals, are very glad to see a black powder precipitated to the bottom; taking it for a hopeful sign, that those particles are of a golden nature, which appear in a colour so common to gold, when separated from other metals by *Aqua fortis*, tho' so obstinate, that it is hard for the refiner to reduce the precipitated calx to its native colour. For tho' that may quickly be done by fire, which will make this gold look very charmingly, and is indeed one of the best ways in practice for the refining of gold; yet it requires a watchful eye, and competent skill, to give it such a degree of fire, as will serve to restore its lustre, without bringing it to fusion. I remember, that having taken a flat bright piece of gold refined by a curious and skilful person, on purpose to try to what height of purity that metal could be brought by art; I found this very piece, as glorious as it look'd, being rubb'd a little upon fine clean linen, sullied it with a kind of black. And the like I have observed in refined silver; which I therefore mention, because I formerly suspected, that the impurity of the metal might have been the only cause of what I have frequently observed in wearing silver-hilted swords, that where they rubb'd upon my clothes, when made of light-coloured cloth, they would quickly black them. And thus also I have found pens blacked nearly all over, upon carrying them about me in a silver ink-case. And as in these several instances of acquired blackness, the metals are worn off, or otherwise reduced to very minute parts, 'tis a circumstance that deserves to be remark'd.

11. That a solution of silver will dye hair of a black colour, is a known experiment, wherewith some persons of greater curiosity than skill have unluckily made their friends very merry. And I lately diverted myself by an improvement of this observation; for having dissolved some

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pure silver in *Aqua fortis*, and wholly evaporated the menstruum, I caus'd a quantity of fair water to be poured upon the calx, two or three several times; and to be each time evaporated, till the calx was very dry, and all the greenish blue, that usually appears in common crystals of silver, quite vanished: after this, I made those I had a design upon, to moisten some part of their skin with their own spittle, and slightly rub the moistened parts with a little of the silver thus prepared; upon which they were surprized, to see that a snow-white body should presently produce a deep blackness there, as if the stains had been made with ink. This blackness could not, like that produced by ink, be readily washed away; but required a long time to be got off. And with the same calx, and a little fair water, we likewise stain'd the white hafts of knives with a lasting black, in those parts where the calx was plentifully laid on; but where it was very thinly spread, the stain was not of quite so deep a colour.

An inquiry into
the cause of
blackness in the
Negroes.

12. The cause of blackness, in whole nations of *Negroes*, has been long disputed by learned men; who possibly had done well to have considered why some whole races of other animals, as foxes and hares, are distinguished by a blackness unusual to the generality of the same species. However, I shall freely acknowledge, that this inquiry seems more abstruse to me than it does to many others; because, consulting many authors, the accounts of voyages, and travellers, to satisfy myself in matters of fact relating to it; I have met with some things among them, which seem not to agree with the notion of the most classic writers upon this head. As it is, therefore, my present business to deliver rather matters historical, than theoretical, I shall annex some few of my collections, instead of a formal disputation.

It is commonly presumed, that the heat of the climates, inhabited by *Negroes*, is the cause of their colour; and this, principally, because we plainly see, that mowers, reapers, and other country people, who spend the most part of the summer-days in the heat of the sun, have the skin of their hands and faces, which are the parts immediately exposed to his rays, of a dark colour, and tending to blackness. On the contrary, we observe, that the *Danes*, and other inhabitants of cold climates, and even the *English*, who feel not so severe a degree of cold, have usually whiter faces than the *Spaniards*, *Portuguese*, and other *Europeans* of hotter countries. But this argument seems far more specious than convincing; for tho' the heat of the sun may darken the colour of the skin, yet experience doth not shew, that heat alone is sufficient to produce a discolouration, which shall amount to a true blackness, like that of the *Negroes*. Besides, in many parts of *Asia*, under the same parallel, or in the same degree of latitude with the *African* regions, inhabited by blacks, the people are but tawny. And in *Africa* itself, many nations in the empire of *Ethiopia* are not *Negroes*, tho' situate in the torrid zone, and as near the equinoctial, as other nations, that are black. Again, I find not, by the best accounts, I could any where obtain of the *West-Indies*, that, excepting, perhaps, one place, or two, of small extent, there are any blacks, originally natives of any part

part of *America*; for the blacks, now there, were, by the *Europeans*, long ago transplanted thither: tho' it contains so great a variety of climates, and particularly reaches quite cross the torrid zone, from one tropic to the other. 'Tis true, the *Danes* are a whiter people than the *Spaniards*; yet this may proceed rather from other causes, than the coldness of the climate; since the *Swedes*, and other inhabitants of those cold countries, are not usually so white as the *Danes*, nor whiter than other nations, in proportion to their distance from the pole. And, in the numerous train of an ambassador extraordinary from the *Russian* emperor, I observed, tho' it were then winter, the colour of their hair and skins was far less whitish than that of the *Danes*, who inhabit a milder region; and rather, for the most part, of a darkish brown. And the physician to this ambassador told me, that, in *Muscovy* itself, the generality of the people were more inclined to have dark-coloured hair, than flaxen; but seem'd to suspect, that the true and ancient *Russians*, some whereof he had met with in one of the provinces of that vast empire, were rather white, like the *Danes*, than any thing near so brown, as the present *Muscovites*, whom he guesses to be descended from the *Tartars*, and to have inherited their colour from them.

But further, eminent authors inform us, that there are *Negroes* in *Africa*, not far from the cape of *Good-Hope*, and consequently beyond the southern tropic; and out of the torrid zone, about the same latitude to the north, there are many *American* nations, that are not *Negroes*; and wherein the inhabitants of *Candia*, some parts of *Sicily*, and even of *Spain*, are not so much as tawny. Nay, I find, by our latest accounts of *Greenland*, that the inhabitants there are olive-colour'd, or rather of a darker hue. But if the case were the same with men, and those other kinds of animals, before mentioned; I should offer a remark; as a considerable proof, that cold may do much towards making men white, or black. However it be, I shall set down the observation, as deserving a place in the history of whiteness and blackness. It is affirmed by *Olaus Magnus*, and others, that in some parts of *Russia* and *Livonia*, hares, foxes, and partridges, which were black before, or red, or grey, become white in the depth of winter, by reason of the great coldness thereof. And a virtuoso, who lately travelled thro' *Livonia* to *Moscow*, confirms this relation; adding, that himself had seen such animals there, whilst they were white, which the inhabitants assured him had been black, or of other colours, before the winter began, and that they would be so again, when that season was over. But, for further satisfaction, I also consulted one, who had, for some years, been an eminent physician in *Russia*, and who, tho' he rejected some other traditions, that are generally believed concerning that country, told me, he saw no cause to doubt of this relation, as to foxes and hares; not only because 'tis the common and uncontested assertion of the natives, but also, because he himself, in the winter, could never, that he remember'd, see foxes and hares of any other colour than white. And I myself, having seen a small white fox brought out of

PHYSICS. *Russia* into *England* towards the latter end of winter, foretold those who shew'd him me, that he would change colour in the summer; and accordingly, coming to look upon him in *July*, I found that his back and sides, with the upper part of his head and tail, were already grown of a dark colour; the lower part of the head and belly still continuing white.

Let me add, that were it not for some scruple, I should think more than what *Olaus* relates confirm'd by *Olearius*, who, in his Account of *Muscovy*, has this passage: "The hares are there grey, but in some provinces they grow white in the winter." And soon after—"It is not very difficult to find the cause of this change, which certainly proceeds from the outward cold; since I know that even in summer, hares will change their colour, if they be kept for a competent time in a cellar." But in the same page, this author affirms the like change of colour that happens to hares in some provinces of *Muscovy*, happens to them also in *Livonia*; and yet immediately subjoins, that in *Curland* the hares vary not their colour in winter, tho' the two latter countries be divided only by the river *Dugna*: for it is scarce conceivable, how cold alone should have, in countries so near, so very differing an effect; tho' not more strange, than what is believed by those who ascribe the complexion of negroes to the heat of the sun, when they would have the river *Cenega* so to affect the *Moors*, that tho' on the north side they are but tawny, on the other side they are black. There is another opinion as to the complexion of the negroes, not only embrac'd by many of the more vulgar writers, but by men of eminence and learning; who would have their blackness an effect of *Noah's* curse upon *Cham*. But tho a naturalist may safely believe all the miracles attested by the holy scriptures, yet in this case to fly to a supernatural cause, will, I fear, look like shifting off the difficulty, instead of solving it; for we here enquire not into the first and universal, but the proper, immediate, and physical cause of the blackness in negroes. Besides, 'tis not express'd in scripture, that the curse meant by *Noah* to *Cham*, was the blackness of his posterity; but 'tis plain this curse was quite another thing, clearly expressing that he should be a servant of servants, that is, a very abject servant to his brethren: which accordingly, in some measure, came to pass, when the *Israelites*, the posterity of *Sem*, subdued the *Canaanites* that descended from *Cham*, and kept them in great subjection. But how is blackness a curse? for navigators tell us of black nations, who think so differently of their own condition, that they paint the devil white.

Blackness is not inconsistent with beauty, which depends not so much upon colour, as an advantageous stature, a comely symmetry of the parts of the body, and just features in the face; so that I see not why it should be thought such a curse to the negroes, unless, perhaps, they going naked in those hot climates, the colour of their skin probably makes the sunbeams more scorching to them, than they would prove to people of a white complexion. 'Tis very probable that the principal cause of blackness in negroes, is some peculiar and seminal impression; for black children,

dren, brought over into these colder climates, lose not their colour. And credible authors inform us, that the offspring of negroes, transplanted out of *Africa* above a hundred years ago, still retain the complexion of their progenitors; tho' possibly, in tract of time, it will decay: on the other hand, white people removing into very hot climates, have their skins, by the heat of the sun, scorch'd into dark colours; tho' neither they nor their children are observ'd, even in the countries of negroes, to descend to a true black. Yet *Piso* tells us, that betwixt the *Americans* and negroes, are generated a distinct sort of men, which they call *Cabocles*; and that betwixt *Portuguese* men and *Ethiopian* women, he has sometimes seen twins, one whereof had a white skin, and the other a black one, with other the like effects of seminal impressions, from whence they seem to proceed; since even organical parts may receive great differences from such peculiar impressions, upon what account soever they came to be settled in the first individuals, from whom they are propagated to posterity; as we see in the thick lips and flat noses of most nations of negroes. And if we may credit what learned men deliver concerning the little feet of the *Chinese*, the *Macrocephali*, taken notice of by *Hippocrates*, will not be the only instance we might apply to our present purpose. And on this occasion it may not be impertinent to add what I have observed in other animals. Thus there is a sort of hens which want rumps; and I have seen a perfectly white raven, both in bill and feathers; which I attentively consider'd, for fear of being impos'd upon. A very ingenious physician has often told me of a young lady, a patient of his, who greatly complain'd of want of health, tho' there appear'd so little cause thereof in any respect, that he concluded it wholly imaginary, and advis'd only little journeys of pleasure; when going once to visit *St. Winifrid's* well, the lady, who was a catholic, and devout in her religion, having continued a pretty while in the water, to perform some devotions, fix'd her eyes very attentively upon the red pebble stones, which, in a scatter'd order, made a large part of those that appear'd thro' the water. A while after this, she grew big, went her time, and was deliver'd of a child, whose skin was plentifully speckled with spots of the colour and magnitude of those stones: and tho' this child is many years old, yet she still retains them.

I have but two things here to add concerning the blackness of negroes; the one is, that the seat of that colour, seems to be only the thin outward skin; for I knew a young black, who, having been lightly sick of the small-pox or measles, had, in the places of the pustules, whitish specks left behind. And *Piso* assures us, that upon dissecting many negroes in *Brazil*, he found their blackness went no deeper than the cuticle, which being removed, the *cutis* appear'd as white as that of *European* bodies. And the like has been affirm'd to me by a physician, who dissected a negro in *England*. The other thing I shall here take notice of concerning negroes, is, that an intelligent acquaintance of mine, who keeps about three hundred, women and men, to work in his plantations in the *Indies*, told me their children came into the world almost of the like reddish colour.

PARSIES. flour with our *European* children; and that in a few days after they appear'd black. Agreeable hereto is the account lately publish'd by a Jesuit, one of the missionaries into *South-America*, who there baptiz'd several infants; which, when newly born, he says, were much of the same colour with *European* children, but within about a week begin to put on the hue of their parents. More full is the testimony of our countryman, *Andrew Battel*; who being sent prisoner to *Angola*, lived there, and in the adjoining regions, near eighteen years; for he mentioning the *African* kingdom of *Longo* peopled with blacks, says, according to *Purchas*, "The children in this country are born white, and change their colour in two days to a perfect black. The *Portuguese*, in the kingdom of *Longo*, have sometimes children by the negroe women; and the fathers are often deceiv'd, thinking that when the child is born, it is theirs, and within two days it proves itself the child of a negroe." And the same person has elsewhere a relation, which, if we may credit it, is very well worth our notice; since this, together with what we have formerly mention'd of seminal impressions, shews it possible that a race of blacks might be begun, tho' none of the sons of *Adam* were for many precedent generations of that complexion. 'Tis surely as possible, that white parents may sometimes have black children, as that *African* negroes should sometimes have lastingly white ones; especially since concurrent causes may more easily favour the production of the former, than, in the scorching heat of *Africa*, that of the latter. And I remember, what may a little countenance this assertion, that the possessor of the white raven I formerly mention'd, affirm'd to me, that in the nest, out of which he was taken white, they found with him another young one, of as perfect a black as any common raven. But to come to the passage itself: "There are, says our author, speaking of the regions formerly mention'd, born in this country white children, which is very rare among them, for their parents are negroes; and when any of them are born, they are presented to the king, and are call'd *Dondos*. These, who are as white as any white men, are the king's witches, and are brought up in witchcraft, and always wait on the king. There is no man that dares meddle with these *Dondos*; if they go to the market, they may take what they list; for all men stand in awe of them. The King of *Longo* hath four of them." Yet this country in our globes is placed almost in the midst of the torrid zone; and our author elsewhere tells us, that the inhabitants here are so fond of their blackness, that they will not suffer the whites to be buried in their land; of which he annexes a particular example, that may be seen in his voyage, preserv'd by the industrious *Purchas*. But it is high time to return to our experiments.

Whiteness produced in chymical preparations.

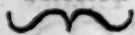
13. The way of producing whiteness by chymical precipitations, is well worth our observing; for thereby bodies of very different colours and natures, tho' dissolved in various liquors, are all reduced to white powders. Thus we find, that not only crabs eyes, and pearls, but red coral, and minium, being dissolved in spirit of vinegar, may be uniformly

formly precipitated by oil of tartar into a white calx. Thus silver, and tin, separately dissolved in *Aqua fortis*, will the one precipitate itself, and the other be precipitated, by common salt-water into white powders; and so will crude lead, tin-glass, and quick-silver, dissolved in *Aqua fortis*, and precipitated; and many of these powders may be made, at least, as fair and white, if, instead of oil of tartar, they were precipitated with oil of vitriol. Nay, that black mineral antimony being reduced, by the salts that concur to compose common sublimate, into the clear unctuous liquor, called rectify'd butter of antimony, will, as was before observed, by the bare plentiful affusion of fair water, be struck down into a snow-white powder; which, when well washed from its saltiness, is term'd *Mercurius vitæ*; tho' the like powder may be made of antimony, without the addition of any mercury at all. And this whiteness commonly ensues, when spirit of wine, impregnated with those parts of gums, or other vegetable concretions, supposed to abound with sulphureous corpuscles, is suddenly diluted with fair water. And on a tincture of benjamin, drawn with spirit of wine, and brought to be as red as blood, having poured some fair water, it presently united with the tincture, and turn'd the whole mixture white. But if such milky fluids be suffered to stand at rest for a convenient time, they usually let fall to the bottom a resinous substance; which the spirit of wine, diluted, and weakned by the water, was unable any longer to sustain. And something of kin to this change of colour in vegetables, is that which chymists observe, in pouring acid spirits to the red solution of sulphur, made with an infusion of pot-ashes, or some sharp lixivium; when the precipitated sulphur, before it subsides, immediately turns the red liquor white. But tho' most precipitated bodies are white, yet some are not. Gold, dissolved in *Aqua regis*, whether precipitated with oil of tartar, or with spirit of sal-armoniac, will not afford a white, but a yellow calx. Mercury also, tho' reduced to sublimate, and precipitated with liquors, abounding in volatile salts, as the spirits drawn from urine, hart's-horn, and other animal substances; yet affords, as we formerly noted, a white precipitate: but, with some solutions, hereafter to be mentioned, it will let fall an orange-tawny powder. And so will crude antimony, if, being dissolved in a strong lixivium, you pour any acid liquor upon the solution, newly filtered, whilst it is yet warm. And if, upon the filtered solution of vitriol, you pour a solution of lixivate salts, there will subside a large substance, very far from white, which the chymists call sulphur of vitriol; so that the greatest part of dissolved bodies being, by precipitation, brought to white powders, and yet some affording precipitates of other colours, the reason of both the phenomena may deserve to be inquired into.

14. Some learned moderns are of opinion, that the reason why whiteness and blackness ought to be called the two extreme colours, is, that black receives no other colour, but white very easily receives them all. And, not to dispute about words, or expressions, the thing itself, that is affirmed

Whether black receives no other colour, and white all colours?

PHYSICS. affirmed as matter of fact, seems to be true, in most cases, but not in all. For tho' it be a common observation among dyers, that cloth, which has once been thoroughly imbued with black, cannot well afterwards be dyed of a lighter colour; yet the experiments, lately delivered, may shew, that, were the change of colour in black bodies attempted, not by mixing those of lighter colours with them, but by adding such things, as are proper to alter the texture of those corpuscles, which contain the black; 'tis no difficult matter to effect. For inks of several kinds, may immediately be deprived of all their blackness; and those made with log-wood, and red roses, be changed, the one into a red, the other into a reddish liquor. With oil of vitriol, I have sometimes turn'd black pieces of silk into a kind of yellow; and tho' the taffaty were thereby made rotten, yet that no way prejudices the experiment; the change of black silk, into yellow, being never the less true, because the yellow silk is not good. And as for whiteness, I think the general affirmation of its being so easily destroyed, or transmuted, by any other colour, ought not to be received, without some cautions, and restrictions. For tho', according to what I formerly noted, lead is, by calcination, turn'd into that red powder, called minium; and tin, by the same means, reduced to a white calx; yet the common putty, instead of being, as it is pretended, and ought to be, only the calx of tin, is, by the artificers who make it, to save the charge of that metal, composed but of half tin, and half lead, if not far more lead than tin; and yet the putty, in spite of so much lead, is a very white powder, without disclosing any mixture of minium. And so, if you take two parts of copper, which is a high-coloured metal, and but one of tin, you may, by fusion, bring them into a mass, wherein the whiteness of the tin is much more conspicuous, and predominant, than the reddishness of the copper. And a very honest man assures me upon his own experience, that if arsenic and copper be melted together in a due proportion, the arsenic will blanch the copper both within and without; and that this white mixture, skilfully kept upon the cupel, would presently let go its arsenic, which made whiteness its predominant colour, and return to the reddishness of copper: so that some white mineral bodies may be very capable of eclipsing others, and of making their colour predominant in mixtures. And, as a further confirmation of this, I remember, that I had a lump of silver and gold, melted together, wherein, by the estimate of a very experienced refiner, there might be about a third part of gold; yet the yellow colour of that metal was so hid, that the whole mass appeared to be silver; and, when rubbed upon the touch-stone, 'twas not easily distinguished from the touch of common silver: tho' if I put a little *Aqua fortis*, upon any part of the white surface it had given the touch-stone, the silver, in the moistened part, being immediately taken up, and conceal'd by the liquor, the golden particles would presently disclose themselves, and look as if pure gold alone had been rubbed upon the stone.



15. Having scraped a piece of black horn, with a piece of glass, into many thin curled shavings, and laid a quantity of them together; I found, that the heap they compos'd, was white: and tho' if I laid it on a clean piece of white paper, its colour seem'd somewhat eclips'd by the greater whiteness of that body; yet if I laid it upon any thing very black, it appeared to be of a good white. This easy experiment seems very opposite to their doctrine, who would have colours flow from the substantial forms of bodies; and that of the chymists also, who ascribe them to one, or other, of the three hypostatical principles: for tho', in our case, there was so great a change made, that the same body, without being, substantially, either increas'd, or lessen'd, passes immediately from one extreme colour to another, even from black to white; yet this so great and sudden change, is effected by a slight mechanical transposition of parts; there being no salt, sulphur, or mercury, added, or taken away; nor any substantial form generated, and destroyed; the effect proceeding only from a local motion of the parts: which so vary'd their position, as to multiply their distinct surfaces, and qualify them to reflect far more light to the eye, than they could before they were scraped off from the entire body of the black horn.

Colours depend not on the substantial forms of bodies.

16. The chymists, we know, usually assign, for the cause of blackness, the fuliginous steams of adust sulphur; but many of the preceding experiments will abundantly confute this doctrine. If they here mean the fumes of common sulphur, 'tis proper to remind them, that fusion, or sublimation, does not turn that substance black; but it thereby becomes rather more than less white; and, when fired, it affords so little soot, as scarce, in any degree, to blacken white paper; but the smoke of it rather blanches linen, and turns red roses pale. Besides, I have seen a fixed kind of sulphur kept, for many weeks together, in a very violent fire, that did not, when cool'd, appear black, but of a true red. If, by sulphur, they mean the sulphureous principle; upon this supposition, torrify'd sulphur should afford more blackness, than most other concretes; wherein that principle is supposed more deficient. Yet spirit of wine, tho' totally inflammable, will not, by burning, discolour white paper, held over it; and the smoke of our *Tinby* coals has been found, rather to blanch, than blacken linen. To these particulars, many others, of the same kind, might be added; but we need only look back, for further satisfaction, to our way of making black inks, from substances of different colours; for how can it be said, that, when the component liquors thereof are put together, actually cold, and continue so, after their mixture, there intervenes any new aduotion of sulphur, to produce the emergent blackness? And when, by instilling a few drops of oil of vitriol, &c. the blackness, produced in those experiments, is presently destroy'd; if the colour proceeded only from the sulphureous parts, torrify'd, in the black bodies, I demand, what becomes of them, when the colour so suddenly disappears? for it cannot reasonably be said, that all those which sufficed to make so great a quantity of black matter, should

The chymical doctrine of blackness confuted.

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resort to so very small a proportion of the clarifying liquor, and be diluted by it, without giving it any blackness. And if it be said, that the intill'd liquor disperfed those black corpuscles; I demand how that dispersion comes to destroy their blackness, but by such a local motion of their parts, as destroys their former texture? This may be a matter of such moment, in cases like ours, that I remember I have, in a few hours, without addition, from foot itself, obtain'd a large quantity of crystalline salt, and of a transparent liquor; yet this black substance had its colour so altered, by the change of its texture from the fire wherewith it was distilled, that it did, for a great while, afford such plenty of very white exhalations, that the receiver, tho' large, seem'd to be almost fill'd with milk. Secondly, But were it granted, as it is in some cases not improbable, that bodies may receive a blackness from a footy exhalation, occasion'd by the aduſtion of their sulphur, or oily parts; yet this is applicable only to some particular bodies, and will afford us no general theory of blackness. For if, for example, white hart's-horn, included in vessels well luted to each other, and exposed to the fire, be said to turn black, by the infection of its own smoke; I think I may justly demand, what makes the smoke, or foot itself, black, since no such colour appeared before in the hart's-horn? And, with the same reason, when we are told, that torrify'd sulphur makes bodies black; I desire to be told also, why torrification makes sulphur itself black? Nor will there be any satisfactory reason assigned of these queries, without taking in those intelligible mechanical principles of the position and texture of the minute parts of the body, with regard to the light and the eye; which may serve the turn, in many cases, where the aduſtion of sulphur cannot be pretended; as in the apparent blackness of an open window, view'd at a distance, &c. in which, and many other cases, formerly alledged, there appears nothing requisite to the production of blackness, but a prevention of the incident rays of light, from being plentifully enough reflected to the eye*. In short, the doctrine I here oppose, is pleaded for, as chymists commonly argue about qualities; who content themselves to say in

* For the production of black, Sir Isaac Newton observes, that "the corpuscles of the body must be much less than any of those which exhibit colours. For at all greater sizes, there is too much light reflected, to constitute blackness. And from hence, says he, may be understood, why fire, and the more subtile dissolver, putrefaction, by dividing the particles of substances, turn them to black; why small quantities of black substances impart their colour very freely, and intensely, to other substances, to which they are applied; the minute particles of these, by reason of their very great number, easily overspreading the gross particles of others; why glass, ground very elaborately with sand, on a copper-plate, till it be well polished, makes the sand, together with what is worn off from the glass and copper, become very black; why black substances do soonest, of all others, become hot in the sun's light, and burn; considering the multitude of refractions in a little room, and the easy commotion of such very small corpuscles, &c." See Newton. Optic. p. 235.

what

what ingredient of a mixed body a particular quality resides, instead of explaining its nature.

S E C T. III.

I Here propose to throw together the miscellaneous experiments I have made, with relation to colours.

I know of no way more likely to convince the generality of men, how great a share the variable texture of bodies may have, in making them appear of differing colours, than by shewing how the addition of a single ingredient, that either is colourless, or at least has not any of the colours to be produced, is immediately able, by introducing a secret change of texture, to make the body 'tis put to, appear sometimes for one colour, sometimes of another; according as the parts of the body wrought upon, are disposed to receive such a change, as modifies the incident rays of light, after the manner requisite to make them exhibit a blue, a green, a red, or some other particular colour.

Many changes of colour produced by one simple ingredient.

For this end, I made choice of the spirit of salt, as that which is very simple; and which, if it be not too highly rectified, may be had clear, and colourless. With this spirit, I proceeded to make the following experiments, upon several bodies, whose differing textures seem'd to fit them for my purpose.

1. Some drops of well-coloured syrup of violets, being let fall together upon a piece of white paper; if a third, or fourth part so much spirit of salt be mixed with them, the syrup will presently become of a red colour, usually inclining to purple.

2. But if the liquor to be acted on, be otherwise disposed, 'tis possible, with spirit of salt, to turn it from a blue colour, not to a red, but to a green; as I have sometimes done, by letting fall into a deep solution of filings of copper, made with an urinous spirit, as that of sal-armoniac, just as many drops of spirit of salt, as were requisite to produce the change intended. A very small error, either in excess, or defect, may leave the mixture still blue, or bring it to be colourless.

3. Upon a few drops of good syrup of violets, let fall two or three drops of good spirit of urine, hart's-horn, or the like; and when, by mixing them well, the syrup has acquired a fine green colour; by putting to it a little of the spirit of salt, and stirring it, you may turn the green syrup into a red.

4. If you put a quantity of red rose-leaves, well dried, into a glass vial, almost full of fair water; and, soon after, put to them as much spirit of salt as will make the water pretty sharp; you will quickly see, both that liquor, and the contained leaves, brought to a fine lovely red, which they will long retain. The like effect spirit of salt will have on some other vegetables of a stiptic, or of an astringent nature.

5. But if, by infusing brazil-wood in fair water, you make a tincture of it, which you may much deepen, by dropping into it a little spirit of hart's-horn, or of urine; and you then put to it a little spirit of salt, it will pre-

sently

PHYSICS. sently change from a deep reddish colour, to a colour far more pale, or rather yellow; so that the same spirit acting upon two vegetable tinctures, differently disposed, draws out, and heightens redness in the one, and destroys it in the other.

6. If you make an infusion of true *Lignum Nephriticum*, in spring water, it will appear of a deep colour, like that of an orange, when you place the vial between the window and your eye; and of a fine deep blue, when you look on it with your eye placed between it and the window: but if you shake into this liquor a few drops of spirit of salt, the blue colour will presently vanish, and appear no more, in what light soever you view the vial; tho' the liquor will still retain the orange-colour.

7. We took common writing-ink, and having let fall several drops of it upon a piece of white paper; so that, when it grew dry in the air, some parts of the ink lay thick, and some thinner, upon the paper, whereon it spread itself: we then put a few drops of strong spirit of salt, some on one part of the black'd paper, and some on another; and observed, that in those places, where the spirit had been put, or to which it reached, the blackness was quite destroyed, and was succeeded by an unpleasant kind of colour, that seem'd, for the most part, to participate of yellow and blue, neither of them good in kind.

8. If in spirit of salt, you dissolve filings of steel, and slowly evaporate the filtered solution, it will shoot into a kind of *Vitriolum Martis*, which appears green, as well as that which chymists vulgarly make with oil of vitriol. And if you take these crystals, made with spirit of salt, and, when they are dry, keep them in a crucible; you will find, that even a moderate fire, if duly apply'd, will make them, in a short time, exchange their green colour for a red, like that of the finer sort of *Crocus Martis*: as, indeed, this operation makes them referable to that sort of medicine.

9. We took some mercury, precipitated *per se*; and tho' crude mercury is not, as far as I have tried, soluble in our *English* spirit of salt; yet this red precipitate readily dissolved in that liquor, without at all imparting its own colour to it. I also found, that red lead, being boil'd a while in good spirit of salt, the redness totally disappear'd. So that the same agent which produces redness in several bodies, in our two cases quite abolish'd it. Thus also, the reddest coral being dissolved in our menstruum, the redness vanishes, and the solution appears colourless.

10. Take small filings of copper, and having poured thereon good spirit of salt, till it swim, about two fingers breadth over them; keep the vial in a pretty strong sand-heat, till you perceive the menstruum has dissolved a competent part of the metal: then warily take out the vial, and holding it between your eye and the light, you will perceive the solution of copper to be of a dark and troubled colour, often inclining to a deep, but muddy red.

11. But if you pour this solution into a wide-mouth'd glass, and let it stand for a competent time, the exposed liquor will appear of a green, much finer than that of the crystals of iron. Take

12. Take the clear limpid solution of silver, or of mercury, made in *Aqua fortis*, and drop upon it some spirit of salt; and you will find the clear liquor turn'd white as milk, which, after a while, will let fall a presipitate of the same colour.

13. And if, instead of a solution of silver, or quick-silver, you take a red solution, or tincture, of benjamin, or of the resinous part of jalap root; you'll also have, upon the affusion of spirit of salt, a white liquor, and a precipitate of the same colour.

14. Being desirous to produce two differing colours at once, by the same affusion of spirit of salt; I infused some dry'd red rose-leaves in fair water, till it had acquired a deep colour from them: to this infusion, poured off warily, that it might be clear, I added a considerable proportion of the sweet liquor, made by digesting spirit of vinegar upon red lead; by which I knew 'twould be turn'd of a bluish green. Upon this almost opake liquor, I pour'd spirit of salt, which precipitated the lead that had been dissolved in the sweet liquor, into a very white powder, and gave the remaining liquor, well impregnated with particles of the rose-leaves, a very fine and durable scarlet colour. And if the experiment be well made, you may barely, by shaking together, and confounding the white powder with the red liquor, make a carnation-colour, which will appear very fine and lovely, whilst it lasts; but, in no long time, the two substances that compose it, separate, by degrees, and appear each in its former place and colour.

15. We took some spirit of salt, that, having laid long upon filings of copper, had lost the muddy tincture it first acquired, by being almost boil'd upon them. This liquor, that look'd like common water, we pour'd into a small wide-mouth'd crystal glass, and leaving it in a window, it appeared, in forty minutes, to have acquired a colour, much like that of a *German* amethyst; and seem'd to have no tendency to greenness. But in about three hours time, it appear'd of a lovely green.

16. Precipitate a strong solution of sublimate made in fair water, with a sufficient quantity of oil of tartar, *per deliquium*; put the liquor and powder into a filtre of cap-paper, and when the water is run thorough, the precipitate will remain in the filtre; which is to be slowly dry'd. Then take it out of the filtre, in the form of a gross powder, and having put it into a clear glass, let fall on it, warily, some drops of strong spirit of salt; and, during the conflict that will be made, the little lumps of the precipitate will lose all their former brick-dust colour, and turn white; tho' afterwards they will appear dissolved into a transparent liquor, wherein the orange-colour is quite abolished.

17. Having calcined copper, without any addition, but of fire and water; we took a quantity of it, and having poured thereon about three or four times its quantity of good spirit of salt, we obtain'd a muddy reddish liquor, and a white powder, whose quantity bore a considerable proportion to the part that was dissolved; in which part itself, by the affusion of common water, and the action of the air, we afterwards produced more than one change of colour.

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18. We sometimes took a spoonful of the dark brown, or somewhat reddish solution of copper; and having put it into a cylindrical vial, that the change of colour might appear the better, we poured on it two or three spoons-ful of highly rectify'd vinous spirit; and giving the glass a shake, to mingle them, we presently had a lovely green liquor; which, when well settled, was very fair, and look'd as if it were a liquid emerald.

19. Having moisten'd a small part of some green taffaty-ribband, twice or thrice, with good spirit of salt, we suffer'd it to dry of itself, which it did in a short time; and then found, that the part wetted was no longer of a green, but a blue colour. The same spirit presently turn'd that part of a piece of black ribband, upon which we put two or three drops of it, to a colour not unlike what is call'd *Fueille morte*.

20. 'Tis usual to see books covered with paper, that looks sometimes of a greenish blue colour, bordering upon purple; and sometimes upon that of violets. The deeper coloured paper of this sort, I have several times held in one hand, and with the other lightly touched it here and there with the end of a feather, dipped in spirit of salt; which almost in the twinkling of an eye, dy'd the touched parts of the paper of a lovely red, that would sometimes long continue very vivid. And if, instead of a feather, I used a brush, dipt in the saline spirit, and made many drops at once fall upon the paper; 'twas pleasant to see how suddenly it would be speckled.

21. Upon well-powdered antimony, we pour'd three or four times its weight of good spirit of salt; and caused it to be boiled in this liquor, in a glass vessel, wherein a part of it was dissolved, and taken up into the menstruum; when the antimony quite lost its blackness. And this spirit of salt, thus impregnated, being dropped into fair water, the black mineral immediately subsided, in the form of a very white powder or precipitate.

Hence, by the way, it were not amiss, if physicians, chymists, &c. who compound drugs, or other ingredients, would be less forward than they usually are, to jumble several of them together either unnecessarily, or without a due regard to their qualities, in respect of one another. For most of us are but too liable to be mistaken, when we presume before-hand to say what changes the coalition, or other associations of differing bodies shall produce; especially if they be either saline, or plentifully partake of a saline nature: since experience frequently shews, that by the action and re-action consequent upon untry'd ways of composition, there arise in the mixture new and unlook'd-for consistences and other qualities or accidents. And tho' it may sometimes happen, that these new qualities shall prove advantageous, yet this may well be look'd upon but as a lucky chance; and it may still be justly fear'd, that, ordinarily, such accidental qualities of a medicine will prove to be either worse than were expected, or at least different from what was design'd, and consequently less fit for the physicians or the artists purpose.

I. Octob.

1. *Octob. 11.* About ten in the morning, the weather being sun-shiny, we took several sorts of stain'd paper, some of one colour, and some of another, and in a darkned room, whose window look'd southward, we with them cast the beams that enter'd at a hole about three inches and a half in diameter, upon a white wall that stood on one side, about five foot distance from them. The white gave much the brightest reflection. The green, red, and blue being compared together, the red gave much the strongest reflection, and manifestly threw its colour upon the wall; the green and blue were scarce discernible by their colours, and seem'd to reflect an almost equal light. The yellow, compared with the two last, reflected somewhat more light. The red and purple being compared together, the former manifestly reflected much more light than the latter. The blue and purple compared together, the former seem'd to reflect somewhat the more light, tho' the purple colour was most visible. A sheet of very well sleek'd marbled paper being apply'd as the others, did not cast any of its distinct colours upon the wall, nor throw its light with an equal diffusion thereon; but sent the beams unstain'd and bright to particular parts thereof, as if its polish had given it the nature of a speculum. But comparing it with a sheet of white paper, we found the reflection of the latter to be much the stronger; this diffusing almost as much light to a considerable extent, as the marble paper did to one part of the wall. The green and purple left us somewhat in suspense which reflected the most light; only the purple seem'd to have some little advantage over the green, which was dark in its kind.

2. Though a darken'd room be generally thought requisite to make the colour of one body appear by reflection from another that is not supposed to act as a speculum; yet I have often observ'd, that when I wore waistcoats lined with silk that was very glossy and vividly colour'd, but especially if red, I could in an enlighten'd room plainly discern the colour upon the pure linen that came out at my sleeve; as if that fine white body were more of a mirror than colour'd and unpolish'd bodies are usually supposed.

3. Holding stain'd sheets of paper, sometimes one, and sometimes another, before the hole of the window in the darken'd room, betwixt the sun and the eye, with the colour'd sides obverted to the sun, we found them singly to be somewhat transparent, and to appear of the same colour as before, only a little alter'd by the great light they were placed in: but applying two of them one over another to the hole, the colours were compounded as follows. The blue and yellow scarce exhibited any thing but a darker yellow, which we ascribed to the coarseness of the blue paper, and its darkness in its kind. For applying the blue parts of the marbled paper with the yellow paper after the same manner, they exhibited a good green. The yellow and red look'd upon together, gave us but a dark red, a little inclining to an orange-colour. The purple and red view'd together, appear'd more deeply scarlet. The purple and yellow made an orange. The green and red made a dark orange-tawny. The
green

PHYSICS. green and purple made the purple appear more dirty. The blue and purple made the purple more delightful, and far more deep. The red parts of the marbled paper view'd with the yellow, appear'd of a red, far more like scarlet, than without it. But the fineness or coarseness of the paper, its being carefully or slightly colour'd, with many other circumstances, may so vary the events of such experiments, that they ought to be carefully repeated, before any conclusion is drawn from them.

4. The triangular prismatic glass being a very commodious instrument to shew the nature of colours, we thought proper to observe the several reflections and refractions which the rays of light undergo in falling upon or passing through it. And this we imagined might be best done, not in an ordinary enlighten'd room, where even the curious have left particulars unregarded; but in a darken'd one, where, by placing the glass in a convenient posture, the various reflections and refractions may be distinctly observ'd; and where it may appear what rays are untinged, and which they are that paint upon the bodies whereby they are stopt, either the primary or secondary iris. In pursuance hereof, we observ'd in a darken'd room four reflections and three refractions, that were afforded us by the same prism. And notwithstanding the rules of catoptrics and dioptrics, we thought it would not be amiss to find, by covering sometimes one part of the prism, and sometimes another, and observing where the light or colour vanished thereon, by which reflection and by which refraction each of the several places where the light falling, or passing through the prism, and appearing either pure or ting'd, gave the phenomena. But because these and other particulars which we observ'd, would be tedious, and not so intelligible to deliver in words, I refer to the scheme, where all of them may be taken in at one view.

Fig. 4.

P P P an equilatero-triangular crystalline prism, one of whose edges **P** is placed next the sun.

A B and $\alpha \beta$, two rays from the sun falling on the prism, at **B** β ; and thence partly reflected towards **C** and γ , and partly refracted towards **D** and δ .

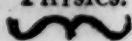
B C and $\beta \gamma$, those reflected rays.

B D and $\beta \delta$, those refracted rays, which are again partly refracted towards **E** and ϵ , and there represent an iris 12345, denoting the five succeeding colours, red, yellow, green, blue, and purple, and partly reflected towards **F** and ζ .

D and **F**, and $\delta \zeta$, those reflected rays, which are partly refracted towards **G** and η colourless, and partly reflected towards **H** and θ .

F H and $\zeta \theta$, those reflected rays, which are refracted towards **I** and ι , and there represent another fainter iris; the colours of which are contrary to the former 54321, signifying purple, blue, green, yellow, red; so that the prism in this posture represents four rain-bows.

5. We observ'd in a room not darken'd, that the prismatic iris might be reflected, without losing any of its several colours, not only from a plain looking-



looking-glass, and from the smooth surface of fair water, but also from a concave looking-glass; and that refraction did as little destroy those colours, as reflection. For by the help of a large double-convex glass, thro' which we refracted the sun's rays, we found that tho' one part of the iris might be made to appear either beyond or on this side of the other parts thereof; yet the same vivid colours would appear in the disorder'd part as in the other. And by covering that side of the prism obverted to the sun, with an opaque body, wherein only one small hole was left for the light to pass thro', having reduced the prismatic iris, cast upon white paper, into a very narrow compass, and view'd it thro' a microscope, the colours appear'd the same, as to kind, they did to the naked eye*.

6. It may afford matter of speculation, that as the colours of outward objects brought into a darken'd room, so greatly depend, in their visibility, upon the dimness of the light they are there beheld by, that the ordinary light of the day being freely let in upon them, they immediately disappear; so our experiments have shewn, that as to the prismatic iris, painted on the floor by the beams of the sun passing thro' a triangular glass, tho' the colours of it appear very vivid, even at noon-day and in sun-shiny weather, yet by a more powerful light they may be made to disappear. For having sometimes taken a large metalline concave speculum, and therewith cast the converging beams of the sun upon a prismatic iris, which I had caused to be projected upon the floor; I found that the over-powerful light made the colours of the iris vanish: and if the light were so reflected as to cross but the middle of the iris, the colours in that part only

* The more general phenomena of the prism, as observ'd by the illustrious Sir *I. Newton*, are, in short, as follow.

1. The rays of light passing thro' the prism, paint an image of various colours on the opposite wall; the principal whereof are red, yellow, green, blue, and violet.

2. This image is not round, but the angle of the prism being of 60 or 65 degrees, about five times longer than 'tis broad; the reason whereof was unknown, till Sir *I. Newton* found it owing to the different refrangibility of the rays. See *Philos. Transact.* N° 80.

3. The rays which exhibit the yellow colour, are turn'd more out of their strait course, than those which make the red; those which make the green, more than those which make the yellow, &c. and those which make the violet, most of all.

4. If the prism thro' which the rays are transmitted, be turn'd about its axis, so that the red rays, the yellow, the green, &c. may fall thro' a small hole, in order,

upon another prism, placed about twelve feet from the former, and be thrown to another part of the room, the yellow rays, for instance, tho' they fall in the same manner upon the second prism with the red, yet will not be thrown to the same place, but beyond them. And if at the place of the second prism, they be collected by a lens, the yellow rays, the green, &c. will each in their order, be thrown to a focus at a shorter distance than the red.

5. The colours of the coloured rays, when well separated, can neither be destroyed, nor in any manner changed by new refractions.

6. The colours of the coloured rays remain unchanged in passing thro' an enlighten'd space, in mutually crossing each other, in the confine of shadow, and in being reflected from any natural bodies in a place otherwise dark.

7. All the coloured rays collected together by several prisms, by a glass lens,

PHYSICS. only became invisible; those parts of the iris that were on the right hand and on the left of the reflected light, continuing to exhibit the same colours as before.

*Objects view'd
in different
kinds of light.*

7. I have sometimes thought it worth while to take notice, whether the colours of opaque bodies might not appear to the eye somewhat diversify'd, as well by the nature of the lucid body that shines upon them, as by the disposition of the superficial parts of the bodies themselves, and the position of the eye with regard to the object and the light: I have observ'd a manifest difference in some kinds of colour'd bodies view'd by day-light, and afterwards by the light of the moon. Several sheets of the colour'd paper that had been view'd in the sun-shine, being look'd upon at night by the light of a pretty large candle, the changes observ'd therein were these. The yellow seem'd much fainter than in the day, and inclinable to a pale straw-colour. The red seem'd little chang'd, but reflected light more strongly than the other colours. A fair deep green look'd upon by itself, seem'd to be a dark blue; but view'd together with a dark blue, appear'd greenish; and compared with a yellow, appear'd more blue than at first. The blue look'd more like a deep purple or murrey, than it had done in the day-light. The purple seem'd very little alter'd. The red compared with the yellow, made the latter look almost like brown cap-paper.

8. But to satisfy ourselves whether the light of the candle were not unsincere, or ting'd with a yellow colour by its fuel; we took a pretty thick cylinder of deep blue glass, and viewing the flame of the candle at a convenient distance thro' it, we perceiv'd it to look green, which is the colour that emerges from the composition of opaque bodies, one of them separately blue, and the other yellow. And this perhaps may

or a concave speculum, or by any other way whatever, make white: and being again separated after this union, every one exhibits its own colour.

8. If the sun's rays fall upon the internal surface of the prism in the most oblique manner that any rays can possibly be transmitted, those reflected will be violet, and those transmitted, red.

9. All natural bodies, especially white ones, view'd thro' a prism, appear on the one hand fringed with red and yellow; but with violet and blue on the other.

10. Two prisms being placed together, so that the red of the one and purple of the other may unite upon a convenient paper in the dark, the image will appear pale; and being view'd thro' a third prism, applied at a due distance from the eye, it will appear as two objects, red and purple.

11. In like manner, if two powders, a perfect red and a perfect blue, be mix'd

together, and laid thick upon some minute body, this body will appear double thro' a prism, and of two colours, red and blue. See *Clark. Annot. in Robault. Ed. 3. p. 199.*

We are farther to consider, that by other means, light is found to be a body propagated in right lines; and that it spends about seven or eight minutes in passing from the sun to the earth. "This was first observed by Mr. Roemer, by means of the eclipses of the satellites of Jupiter. For these eclipses, when the earth is between the sun and Jupiter, happen about seven or eight minutes sooner than they ought to do by the tables; and when the earth is beyond the sun, they happen about seven or eight minutes later than they ought to do: the light of the satellites having farther to go in the latter case than in the former, by the diameter of the earth's orbit." *Newton. Optic. p. 252.*

be

be the chief reason why a sheet of very white paper, view'd by candle-light, is not easily, at first, distinguished from a light yellow, or lemmon-colour; white bodies, as we formerly observed, being more of a specular nature, because tho', when unpolished, they exhibit not the shape of the luminary that shines on them; yet they reflect its light more sincere and undisturbed, by either shades or refractions, than bodies of other colours do.

9. We took a piece of leaf-gold, and with the edge of a knife, lightly moisten'd, laid upon the edge of the gold, we so fasten'd it thereto, that it continued extended, like a little flag; which, being held very near the eye, and obverted to the light, appear'd so full of pores, that it seem'd to have such a kind of transparency, as that of a sieve; but the light which pass'd thro' these pores was in its passage so temper'd with shade, and modify'd, that the eye discerned no more a golden colour, but a greenish blue. And, for further satisfaction, we, in the night, look'd upon a candle, thro' such a leaf of gold; and, by trying the effect of several proportions of distance betwixt the leaf, the eye, and the light, we quickly hit upon such a position for the gold, wherein the flame, view'd thro' the leaf, appeared of a greenish blue, as in the day-time. But the like experiment, try'd with a leaf of silver, did not succeed.

10. Druggists have a wood they call *Lignum Nephriticum*; because the inhabitants of the country where it grows, use the infusion of it, made in fair water, against the stone in the kidneys: and, indeed, an eminent physician of my acquaintance, who has very particularly enquired into that disease, assures me, he found such an infusion one of the most effectual remedies he ever try'd, against that formidable disease. The most ancient account I have met with of this simple, is given us by *Monardes*, who says, "We have a thick, smooth kind of wood, brought us from *New Spain*, which has long been used in the diseases of the kidneys and urinary passages. To prepare a medicine from it, we steep its shavings in clear water, and there suffer them to remain, till the whole liquor is drank up; but when it has thus stood for half an hour, the water appears of a blue colour, which gradually increases, the longer the infusion is continu'd; tho' the wood itself be white." This wood will afford us an experiment, which, besides the singularity of it, may greatly assist to discover the nature of colours. The experiment, as we made it, is this. Take a handful of the thin slices of *Lignum Nephriticum*, and put them into two, three, or four pound of the purest spring-water; let them infuse there for a night, tho' a much shorter time may suffice; decant the impregnated water into a clear glass vial; and if you then hold it directly between the light, and your eye, you will see it tinged, (excepting the very top of the liquor, where a sky-coloured circle sometimes appears) of an almost golden colour, if the infusion be not made too strong of the wood; for, in that case, it will, against the light, appear somewhat dark and reddish, and requires to be diluted by the addition of fair water. But if you hold it from the

Experiments
with the tincture
of *Lignum Ne-
phriticum*.

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And thus far we have try'd the experiment, and found it to succeed, even by candle-light. But if you hold the vial over-against your eye, so that it may have a window on one side, and a dark part of the room before it, and on the other side; you will find the liquor, partly of a bluish, and partly of a golden colour. And if, turning your back towards the window, you pour out some of the liquor towards the light, and towards your eye; it will seem, upon coming out of the glass, to be perfectly blue; but when it is fallen down a little way, the drops may appear party-colour'd, according as the rays of light more or less fully penetrate, and illumine them. If you take a basin, about half full of water, and having placed it so in the sun-beams, that one part of the water may be illumined thereby, and the other part of it darken'd by the shadow of the brims of the basin; and then drop of our tincture, made somewhat strong, both into the shaded and illumined parts of the water, you may, by viewing it from several places, and by a little agitation of the water, observe many pleasing phenomena. If a little of this tincture be poured upon a sheet of white paper, so that the liquor may remain of some depth upon it, you will perceive the adjacent drops to be partly of one colour, and partly of another, according to the position of your eye, with regard to the light: but if you pour off all the liquor, the paper will seem dy'd almost of a yellow colour. And if a sheet of paper, with some of this liquor on it, be placed in a window, where the sun-beams may come freely to it; then, if you turn your back to the sun, and take a pen, or some such slender body, and hold it a-thwart, betwixt the sun and the liquor, you may perceive, that the shadow projected by the pen, upon the liquor, will not, all of it, be a common dark shadow, but, in part, curiously coloured; that edge of it next the body, which makes it, being almost of a lively golden colour; and the remoter verge of a blue one. These, and other phenomena, which I have observed in this delightful experiment, many of my friends have beheld with wonder. And I remember an excellent oculist, finding, by accident, a vial full of this liquor, and having never heard of the experiment, continued apprehensive, upon viewing it for a long time, that some strange new distemper had invaded his eyes. And, I confess, the oddness of the phenomena made me very solicitous to find out the cause thereof. But tho' I am far from pretending to have discovered it; yet my inquiries have enabled me to give some considerable hints about it. In the first place, then, observing that this tincture, if it were too deep, kept the colours from being so lively, and their change from being so discernible; and finding also, that the impregnating virtue of this wood did, by being frequently infused in fresh water, gradually decay, I conjectured, that the tincture afforded by the wood, must proceed from some subtle parts, drawn out by the water; which, swimming about therein, so modify'd the

the light, as to exhibit particular colours. And because these subtile parts were so easily soluble, even in cold water, I concluded, that they must abound with salts, and perhaps contain much of the essential salt of the wood. And, to try whether these subtile parts were volatile enough to be distilled, without dissolving their texture, I carefully submitted some of the tinged liquor, in very low vessels, to the gentle heat of a lamp-furnace; but found all that came over, to be as limpid and colourless as rock-water: whilst the liquor remaining behind, was of such a deep blue, that it must be opposed to a very strong light, to appear of any other colour. I took, likewise, a vial, fill'd with spirit of wine, and a little salt of hart's-horn, and found, that there was a certain proportion to be met with, betwixt that liquor, and the salt, which render'd the mixture fit to exhibit some little variety of colours, not observable in ordinary liquors, as it was variously situated, with regard to the light and the eye: but this change of colour came very far short of that we had admired in our tincture. However, I suspected that the tinging particles abounded with salts, whose texture, and the colour thence arising, would probably be altered by piercing acids; and pouring into a small vial of the impregnated water a very little spirit of vinegar, I found, that the blue colour immediately vanished, while the golden one remained: and which way soever I turn'd the vial, either to, or from the light, I found the liquor constantly appear'd of a yellowish colour, and no other. Upon this, I imagin'd, that the acid salts of the vinegar, having been able to deprive the liquor of its blue colour, a sulphureous salt, which is of a contrary nature, would mortify the saline parts of the vinegar, and destroy their effects. And, accordingly, having placed myself betwixt the window and the vial, and let fall into the same liquor a few drops of oil of tartar, *per deliquium*; I found, that immediately upon the diffusion of this liquor, the impregnated water was restored to its former blue colour. And this oil of tartar, being very ponderous, and falling directly to the bottom of the vial; it was easy to observe, that for a little while the lower part of the liquor appeared of a deep blue, whilst all the upper part retained its former yellowness; which it immediately lost, as soon as the oil of tartar was diffused thro' the whole: and the liquor thus restored, being view'd, either against, or from the light, exhibited the same phenomena with the tinged water, before the adventitious fluids were poured into it.

Kircher speaks of this nephritic wood, in the following manner:
 " 'Tis a white *Mexican* wood, and tho', generally, supposed to communicate only a blue colour to water; yet we have found, by repeated experiments, that 'twill turn it into all kinds of colours. The tree itself, 'tis said, commonly grows to a large size, with a thick even trunk, like a pear-tree, leaves resembling those of rue, and small oblong yellow flowers, growing in clusters. The plant is cold and moist, and nearly of a middle temper. The wood of this tree, made into a cup, turns the water put into it, first of a perfect blue colour, like the bugloss-flower; and the longer it stands herein, the deeper the colour grows.

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“ grows. But if this colour’d water be put into a glass globe, and exposed to the light, there will not appear the least sign of blueness therein; but the liquor will be clear and limpid, like pure water. Yet if this globe of glass be turn’d to a more shady place, the whole liquor will appear of a pleasant green; and if to a place still more shaded, of a reddish colour. And thus ’twill change its hue, according to the objects whereto ’tis exposed. But, after being placed in the dark, ’twill regain its own blue cast.” In this account, I observe the following particulars: First, the author calls it a white *Mexican* wood; tho’, not to mention that *Monardes* says, ’tis brought from *New Spain*, the wood that we have met with, and employ’d as *Lignum Nephriticum*, was not white, but, for the most part, of a dark colour, not unlike that of the sadder-colour’d wood of juniper. ’Tis true, *Monardes* says, that the wood is white; and it is also affirmed, that what is of a sadder colour, is adulterated, by being imbued with the tincture of a vegetable. But having enquired of the most eminent of our *English* druggists, he peremptorily deny’d it. And, indeed, having considered some of the fairest round pieces of this wood, that I could procure, I took notice, in one or two of them, that ’twas the external part only that appeared white, whilst the more internal part was of the other colour; the contrary whereto would, probably, have appeared, if the wood had been adulterated after the manner just mentioned. And I have, at present, by me, a piece of such wood, which, for about an inch next the bark, is white, and then suddenly passes to the above-said colour: and yet this wood, by the tincture it affords in water, seems to have its colour’d part genuine; for the white part appears, upon trial, to be much less endow’d with the tinging property.

Next, our author tells us, that the infusion of this wood expos’d in a spherical vial to the light, looks like spring-water; and adds, that there is no tincture to be seen in it: but herein our observation and his do not agree; for the liquor, which oppos’d to the darker part of a room exhibits a sky-colour, did constantly with us, when held against the light, appear yellowish or reddish, according as its tincture was more or less deep. So that if there be no mistake in the case, his white nephritic wood, and the sadder-colour’d one, which we employ’d, were not of the same nature. What he mentions of the cup made of this wood, we have not try’d; but as for what he says, that this wood tinges the water with all sorts of colours, that is much more than any of those pieces we have hitherto employ’d was able to do: the change of colours discernible in a vial full of water impregnated by any of them, and directed towards a place more light or obscure, being far from affording any such variety. And as for what he tells us, that in the dark this infusion will resume a blue colour, I wish he had inform’d us how he try’d it. But having sometimes brought a round long-neck’d vial fill’d with the tincture of this wood into the darken’d room already often mention’d, and holding it sometimes near the sun-beams that enter’d at the hole, and sometimes partly in them

and

and partly out of them; varying also the position of the glass, and viewing it from several parts of the room, it disclos'd a much greater variety of colours, than it does in an ordinary enlighten'd room: for besides the usual ones, it exhibited a red in some parts, and a green in others; with intermediate colours produced by the different degrees and odd mixtures of light and shade.

'Tis surprizing in this experiment, that the blue tincture may be destroy'd or restor'd, whilst the yellowish or reddish one continues as it was. That salts are of a considerable use in striking colours, the many experiments which the dyers trade affords us, will shew: and as far as we have hitherto try'd, those liquors, in general, which greatly abound in acid salts, have the power of destroying the blue colour in this infusion; as those that abound in sulphureous ones, have the virtue to restore it. And by the way, this observation hints to us a new and easy method of discovering in many liquors whether it be an acid or a sulphureous salt that is predominant. That such a discovery is often very difficult, and may frequently be of great use, he who is no stranger to the various properties and effects of salts, and knows of how great moment it is to be able to distinguish their tribes, will readily conceive.

Now suppose I would try whether alum, tho' it be plainly a mix'd body, To find whether an acid or a sulphureous salt predominates in a liquor. abunds rather with an acid than a sulphureous salt, I turn my back to the light, and holding a small vial full of the tincture of *Lignum Nephriticum* in my hand, which view'd in that position appears blue; then dropping into it a little of a strong solution of alum made in fair water, and finding upon the affusion and shaking of the whole, that the blueness formerly conspicuous in our tincture presently vanishes, I am thereby induced to suppose, that the salt predominant in alum is acid. But if, on the other hand, I would examine, whether salt of urine, or of hart's-horn, is rather of a salino-sulphureous, than of an acid nature; I drop a little of the saline spirit of either, into the nephritic tincture; and finding that the blue colour is thereby rather deepened than destroyed, I collect, that the salts which constitute these spirits, are rather sulphureous than acid. And, to satisfy myself yet farther in this particular, I take a small vial of fresh tincture, and placing both it and myself, with regard to the light, as before; I drop into the infusion just as much distilled vinegar, or other acid liquor, as will serve to deprive it of its blueness; then, without changing my posture, I drop, and shake into the same vial, a small proportion of spirit of hart's-horn, or urine: and finding, upon this, the tincture immediately to recover its blue colour, I am thereby confirmed in my opinion of the sulphureous nature of these salts.

It is much doubted to what sort of salt that which is predominant in quick-lime belongs; but we have been persuaded to refer it rather to the lixivate, than acid kind, by observing, that tho' an evaporated infusion of it will scarce yield such a salt as ashes, and other alkaline bodies do; yet if we deprive our nephritic tincture of its blueness, by just so much distilled vinegar as is requisite to make that colour vanish; the lixivium of

PHYSICS. of quick-lime will, immediately, upon its affusion, recall the banished colour, tho' not so powerfully as either of the sulphureous liquors, formerly mentioned. And I guess at the strength of the liquors, thus examined, by the quantity of them; which is sufficient to destroy, or restore the blue colour of our tincture. But whether as to liquors, wherein neither acid nor alkaline salts are eminently predominant, our tincture will enable us to conjecture any thing more, than that such salts do not abound in them, I leave to further trial; for I find not, that spirit of wine, spirit of tartar, freed from acidity, or chymical oil of turpentine, have any remarkable power to deprive our tincture of its blue colour, or to restore it, when, upon the affusion of vinegar, it has disappeared.

*Different colours
observed in the
same piece of
glass.*

11. We may also see in a mineral body, something very near of kin to the changeable quality of the tincture of *Lignum Nephriticum*; for I have several flat pieces of glass of the thickness of ordinary panes for windows, one of which being interposed betwixt the eye, and a clear light, appears of a golden colour, not much unlike that of the moderate tincture of our wood: but being viewed, when the beams of light pass not so much thro' it, as they are reflected from it to the eye, the yellow seems to degenerate into a pale blue, somewhat like that of a turquoise. And, what is strange, if in a certain position, you hold one of these plates perpendicular to the horizon, so that the sun may shine upon one half of it, whilst the other half is shaded, you may see the part illumin'd, of a much fainter yellow than the shaded part, which will appear more richly coloured. And if the glass be not held perpendicular, but parallel to the horizon, you may see the shaded part of a golden colour, whilst the other appears considerably blue: and as you remove any part of the glass, thus held horizontally, into the sun-beams, or the shade, it will, in the twinkling of an eye, seem to pass from one of those colours to the other; and the rays passing thro' it, and received by a sheet of white paper held near, colour it with a yellow, somewhat bordering upon a red: yet the glass may be so opposed to the sun, as to throw a mixed colour upon paper, in some parts more inclined to yellow, and in others to a blue.

In making these experiments with glass, you must take notice, that as one of the sides has its superficial parts disposed to reflect the blue colour, that side must be held next to the eye. I have myself made glasses proper to exhibit an experiment, not unlike the last mentioned, by laying some silver, very finely foliated, upon glass, and giving it, by degrees, a much stronger fire than is requisite to tinge glass of other colours. And this experiment, tho' made without a furnace, is the more considerable; because a skilful painter, who allow'd 'twas with silver he coloured his glasses yellow, told me, that, when to burn them, he lays on the plates nothing but a calx of silver, calcined without corrosive liquors, and temper'd with fair water, the plates are tinged of a fine yellow, that looks of a golden colour, which part soever be turn'd to, or from the light; whilst we have found, more than once, that some pieces of

of glass, prepared after our manner, tho', when held against the light, they appeared of a transparent yellow; yet viewed with one's back turn'd to the light, exhibited an opaque blue. PHYSICS.

12. There are but few simple, and primary colours; from the various compositions whereof, all the rest result: for tho' painters imitate the hues of those numerous different colours, to be met with in the works of nature, and of art; yet I have not found, that to exhibit this strange variety, they need employ any more than white, black, red, blue, and yellow; these five, variously compounded, and re-compounded, being sufficient to exhibit such a variety, as those, who are altogether strangers to the painter's pallet, can hardly imagine. Thus black and white, differently mixed, make a vast number of lighter and darker greys; blue and yellow make a great variety of greens; red and yellow make orange-tawny; red, with a little white, makes a carnation; red, with an eye of blue, makes a purple: and, by these simple compositions again compounded, the skilful painter can produce what kind of colour he pleases; and a great many more than we have names for. But, to render the rules about the production of colours fit to be relied on, the corpuscles, whereof the several pigments consist, must be such as do not destroy one another's texture; for, in case they do, the emerging colour may be very different from what would result from the mixture of other agreeing pigments of the same colours. *The simple and primary colours but few.*

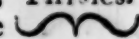
13. It may also help to discover the nature of colours, to know, that the light of the sun, passing thro' diaphanous bodies of different hues, may be tinged of the same compound colour, as if it proceeded from painters colours of the same denomination; tho' the latter be exhibited by reflection, and manifestly compounded of material pigments. Wherefore, to try the composition of colours, we provided several plates of tinged glass, which being laid, two at a time, upon one another; the object, view'd thro' them both, appeared of a compound colour: which agrees with what we deliver'd of looking against the light thro' paper of different colours. But we thought the experiment would be more satisfactory, if we procured the sun-beams to be so tinged in their passage thro' plates of glass, as to exhibit the compound colour upon white paper. And tho', by reason of the thickness of the glasses, the effect was but faint, even when the sun shone strong; yet we easily remedy'd that, by collecting his rays, with a convex glass; which increased the light, at the point where they met, sufficiently to make the compound colour very manifest upon the paper. By this means we observed, that the rays passing thro' blue and yellow, compos'd a green; that an intense and moderate red, did, with yellow, make different degrees of saffron, and orange-tawny; that green and blue made a colour partaking of both, like what some Latin writers call *Pavonaceus*; and that red and blue made a purple. To which we might add other colours that we produced by the combinations of glasses differently tinged, did I not want proper words to express them in our language. And having exposed four or five sorts of coloured glass, and other transparent bodies to the sun, and cast the reflected light upon white paper, held near them; the light

PHYSICS. appeared not manifestly tinged, but as if it had been reflected from the impervious parts of a colourless glass; only that reflected from the yellow, was here and there stained with the same colour; as if those rays were not all reflected from the superficial, but some from the internal parts of the glass. And a skilful tradesman, who makes such coloured glass, told me, that whereas the red pigment was but superficial, the yellow penetrated to the very midst of the plate. But, for further satisfaction, we foliated a plate of *Muscovy* glass, and laying on it a little transparent varnish of a gold-colour, we exposed it to the sun-beams, so as to cast them upon a proper body; on which the reflected light appearing yellow, manifested, that, being reflected from the specular part of the selenites, it was tinged, in its return, with the colour of the transparent varnish, thro' which it passed.

Apparent colours
compound as the
genuine.

14. We now proceed to some experiments made in favour of those colours, that are taught in the schools, not to be real, but only apparent, and fantastical. And, upon trial, we found, that these colours might be compounded both with true and stable colours; and, with one another, as well as those which are unquestionably genuine and lasting; and that the colours resulting from such compositions, would respectively deserve the same denominations. For, first, having, by means of a glass prism, thrown an iris on the floor; I found, that, by placing a blue glass at a convenient distance, betwixt the prism and the iris, that part of the iris, which before was yellow, might be made to appear green; tho' not of a grass-green, but more diluted and yellowish. And it seems not improbable, that the narrow greenish list, usually seen between the yellow and blue parts of the iris, is made by the confusion of those two bordering colours. And tho' the want of a sufficient liveliness in either of the compounding colours, or a small error in the manner of making the following trials, was enough to render some of them unsuccessful; yet when all necessary circumstances were duly considered, the event was answerable to our expectation. As a red and blue compound a purple, so I could produce the latter of the three colours, by casting, at some distance from the glass, the blue part of the prismatic iris, upon a lively red. And sometimes, when I try'd this upon a piece of red cloth, that part of the iris, which would have been blue, and, if compounded with the red of the cloth, appeared of a fair purple; did, when I came to view it near, look very oddly, as if there were some strange reflection, or refraction, or both, made in the hairs of which that cloth was composed. Casting, likewise, the prismatic iris upon a very vivid blue, I found that part of it, which would otherwise have been the yellow, appear green. But it may seem more strange, that tho' this iris, being made by the refraction of light thro' a body that has no colour, must, according to the doctrine of the schools, consist of the purest emphatical colours; yet even these may be compounded of one another, as well as real colours in the grossest pigments. For I took, at once, two triangular glasses, and one of them being kept fixed in the same posture, that the iris it projected on the floor might not waver, I cast on the same floor another iris with the other prism, and moving it to and fro, to bring what part of the

the second iris I pleas'd, to fall upon what part of the first I thought fit; we sometimes, by this means, obtain'd a green colour in that part of the more stable iris that before was yellow or blue; and, frequently, by casting those rays, that in one iris made the blue, upon the red parts of the other, we produced a lovely purple; which might be destroyed, or re-compos'd at pleasure, by separating and re-joining the edges of the two.



15. Considering the prism as the most useful instrument hitherto employ'd about colours, and that those generally used, are made of glass, transparent and colourless; I thought proper to try what change the superinduction of a colour, without destroying the transparency, would produce in the phenomena exhibited by it. But, being unable to procure one of coloured glass, and fearing also, that if it were not carefully made, the thickness of it would render it too opaque; I endeavour'd to supply its place by one of clarify'd rosin, or of turpentine brought to the consistence of a transparent gum. Our attempts this way were not wholly lost indeed, yet we found it so difficult to give these materials their true shape, that we chose rather to varnish over an ordinary prism with some transparent pigment; and this we did, first with a yellow, and then with a red, or crimson one, made of lac, temper'd with a convenient oil: the event was, that, for want of good transparent colours, of which there are but very few, both the yellow and the red rendered the glass so opaque, tho' prudently laid on, that, unless I look'd upon some very vivid object, I could scarce discern any colours at all, especially when the glass was covered with red. And, upon viewing such objects, I found, that the colours of the pigment had vitiated, or drown'd, some of those which the prism would otherwise have exhibited; and, mixing with others, altered them. Thus, when covered with yellow, it made those parts of bright objects, where the blue should have been conspicuous, appear of a light green. But the nature of the colours, the degree of transparency, and darkness in the pigment, with many other circumstances, so vary'd the phenomena of these trials, that, till I can procure small coloured prisms, or hollow ones, that may be filled with tinged liquors, or obtain some better pigments, than those I was reduced to employ; I shall forbear to build any thing upon what I have done in this way: only desire the inquiry may be further prosecuted.

Experiments made with a coloured prism.

16. There are some liquors which, tho' colourless themselves, yet, when they come to be elevated, and dispersed into exhalations, exhibit a conspicuous colour; which they lose again, when re-united into a liquor. Thus, good spirit of nitre, or strong *Aqua fortis*, tho' they have no appearance of redness, whilst in the form of a liquor; if a little heat chance to turn the minute parts of them into vapours, the steam will appear of a reddish, or deep yellow colour; which vanishes, when those exhalations resume the form of a liquor. And if you look upon a glass, half full of *Aqua fortis*, or spirit of nitre, and the other half full of steams, proceeding from either; you will see the upper part of the glass, of the colour just mentioned, if turn'd to the light. But, what is much more considerable, I have try'd,

Limpid liquors may afford coloured vapours.

PHYSICS. that putting *Aqua fortis* in a long clear glass, and adding thereto a little copper, or some such open metal, to excite heat and fumes; the light passing thro' those fumes being cast upon a sheet of white paper, appear thereon of the colour of the fumes, when directly view'd; as if the light were as well ringed in its passage thro' these fumes, as it would have been by passing thro' some glass, or liquor, in which the same colour was inherent. Having, also, sometimes observed, whether the beams of the sun, near the horizon, passing thro' a very red sky, would not exhibit the like colour; I found, that the rays falling in a room, upon a very white object, placed directly opposite to the sun, disclosed a manifest redness, as if they had pass'd thro' a coloured medium.

17. The resulting of colours, upon the coalition of the particles of such bodies, as were neither of them coloured like that mixture whereof they are the ingredients, is very well worth our nicest observation; being of great service, both in speculation and practice. For the mechanical use of colours, among painters and dyers, very much depends upon knowing which of them are producible by mixing pigments of different hues. And 'tis of advantage to the naturalist to know how many, and which colours are primitive and simple; because it both eases his labour, by confining his inquiry to a small number of those upon which the rest depend; and assists him to judge of the nature of particular compound colours, by shewing, from the mixture of what more simple ones, and in what proportions to one another, the particular colour to be considered, results. But because to insist on the proportions, the manner, and the effects of such mixtures, would oblige me to consider a great part of the painter's art, and dyer's trade; I shall confine myself to mention several ways of producing green, by the composition of blue and yellow.

Several ways of
producing a
green, with blue
and yellow.

First, then, as painters make a green, by tempering blue and yellow, reduced to a soft consistence, with water, oil, or some liquor of kin to these; I found, that by chusing fit ingredients, and mixing them in the form of dry powders, I could do more this way, than if the ingredients were tempered with a liquor. But the blue and yellow powders, besides being finely ground, must be such, that the corpuscles of the one may not be too unequal to those of the other; lest, by their disproportionate minuteness, the smaller cover and hide the greater. We used, with good success, a slight mixture of the fine powder of bise, with that of orpiment, or good yellow oker, tho' an exquisite mixture did not succeed so well; but, by lightly mixing the two in several little parcels, those of them in which the proportion and manner of mixture was more luckily hit, afforded us a good green.

(2.) I have also learned, at the dye-house, that cloth, dy'd blue with woad, is afterwards, by the yellow decoction of *Luteola*, turn'd to a green.

(3.) We formerly said, that having in a darkned room taken two bodies, a blue and a yellow, and cast the light reflected from the one upon the other; we thereby obtained a green.

(4.) We

(4.) We also formerly observ'd a green to be produced, when in the same darken'd room we look'd at the hole where the light enter'd, thro' the green and yellow parts of a sheet of marbled paper laid one upon the other.

(5.) We found too, that the beams of the sun passing thro' two pieces of glass, the one blue, and the other yellow, laid upon one another, did upon a sheet of white paper, on which they were made to fall, exhibit a lovely green: and other experiments have been before deliver'd to the same purpose.

(6.) We have likewise contriv'd a way of trying whether metalline solutions, tho' one of them had its colour adventitious, might not, by the mixture of the menstrua employ'd, be made to compound a green, after the manner of other bodies. To a high yellow solution of pure gold in *Aqua regia*, I put a due proportion of a fine deep blue solution of erude copper, made in spirit of urine; and these two liquors, tho' at first they seem'd to coagulate each other a little, yet being thoroughly mix'd by shaking, they presently united into a transparent green liquor; and so continued for several days; only letting fall a little blackish powder to the bottom. This experiment we more than once repeated with success.

(7.) And lastly, to try whether this way of compounding colours would hold even in ingredients actually melted by the violence of the fire, provided their texture were capable of enduring fusion; we caused some blue and yellow ammel to be long and well wrought together in the flame of a lamp, which being strongly and incessantly blown, kept them in some degree of fusion; whence at length we obtain'd an ammel of a green colour.

I have sometimes conjectured, that the mixture of the bise and the orpiment, lately mentioned, produced a green, by altering the superficial asperity which each of those ingredients had a-part; so that the light falling on the mixture, was reflected with different shades, as to quantity or order, or both, from those of either of the ingredients; and such wherewith the light is modify'd, when it reflects from grass or leaves, &c. that we call green. And sometimes too I have doubted whether the green thus produced, might not be partly derived from hence; that the rays reflected from the corpuscles of the orpiment giving one kind of stroke upon the retina, whose perception we call yellow, and those reflected from the corpuscles of the bise giving another stroke upon the same retina, like objects that are blue; the contiguity and minuteness of these corpuscles may make the appulse of the reflected light fall upon the retina within so narrow a compass, that the part they strike being as it were a physical point, they might give a compounded stroke, and consequently exhibit a new compound kind of sensation; as we see that two strings of a musical instrument being struck together, give two sounds, which arrive at the ear in the same time; and cause a differing sensation from either of them, and as it were one compounded of both. I shall only here farther observe, that the first of these ways of compounding a green, agrees much better with our conjectures about colours, than either with the doctrine

of

PHYSICS. of the schools, or with that of the chymists. For first, in the mixture of the two powders I could, by the help of an excellent microscope, discover that what seem'd to the naked eye a green body, was but a heap of distinct, tho' very small grains of yellow orpiment and blue bise confusedly blended together; whence it appears, that the colour'd corpuscles of either kind did each retain its own nature and colour: so much may mere transposition and juxta-position of minute and singly unchanged particles of matter, contribute to produce a new colour. Secondly, the green thus made, being mechanically produced, there is no pretence to derive it from any unintelligible substantial form; nor does this green, tho' a real and permanent colour, seem to be a very inherent quality, as by that doctrine it ought to be; since each part of the mixture remains unalter'd in colour, and consequently of a different one from the heap they compose: for, if the eye be assisted by a microscope, it no longer sees a green body, but a heap of blue and yellow corpuscles. And thirdly, I demand what sulphur, salt, or mercury have to do in the production of this green? for neither the bise nor the orpiment were of that colour before; and the bare juxta-position of the corpuscles of the two powders that operate not upon each other, but might, if we had convenient instruments, be separated unalter'd, cannot with any probability be imagin'd either to increase or diminish any of the three hypostatical principles.

The mixture of every yellow and every blue will not afford a green.

18. But it is not every yellow and every blue that will by their mixture afford a green. For in case either of the ingredients has a power to alter the texture of the other, so as to indispose it to reflect the light, as bodies that exhibit a blue or a yellow, reflect it; the emerging colour may not be green, but such as the change of texture in the corpuscles of one or both of the ingredients qualifies them to give. Thus, for instance, if you let fall a few drops of syrup of violets upon a piece of white paper, tho' the syrup will appear blue thereon, yet by mixing with it two or three drops of the above-mention'd solution of gold, I obtain'd a reddish mixture from the power of the acid salts in the solution. And having made a very strong and high-colour'd solution of copper-filings with spirit of urine, tho' the menstruum seem'd fully saturated with the metal; yet having let three or four drops of syrup of violets fall upon white paper, I found that the deep blue solution proportionably mix'd with this other blue liquor, made not a blue, but, upon account of the urinous salt in the menstruum, a fair green.

The colours of the rain-bow exhibited in very thin substances.

19. To shew the chymists that colours may be made to appear or vanish where there happens no accession or change either of the sulphureous, the saline, or the mercurial principle of bodies, I shall not make use of the iris afforded by the glass prism, nor of the colours to be seen in a fair morning in those drops of dew, that in a convenient manner reflect and refract the rays of light to the eye; but remind them of what they may observe in their own laboratories. For all chymical essential oils, as also good spirit of wine, being shaken till they rise in bubbles, those bubbles appear with various fine colours, which all immediately vanish upon the relapsing

relapsing of the liquor that affords them their films, into the rest of the oil or spirit of wine; so that a colourless liquor may immediately be made to exhibit a variety of colours, and lose them in a moment, without the accession or diminution of any of its hypostatical principles. And, by the way, 'tis worthy our notice, that some bodies, as well colourless as colour'd, by being brought to a great thinness of parts, acquire colours they had not before. For not to insist on the variety of colours that water, render'd glutinous by soap, acquires, when blown into spherical bubbles; turpentine will, by being blown into after a certain manner, afford bubbles adorn'd with various colours; which tho' they soon vanish after the bubbles break, yet these would, probably, always variously exhibit colours upon their superficies, if their texture were durable enough: for I have seen a person skill'd at fashioning glasses, by the help of a lamp; blow some so vehemently, as to burst them; whereupon we found the tenacity of the metal such, that before it broke, it suffer'd itself to be reduced into films so extremely thin, that being kept clean, they constantly shew'd on their surfaces the varying colours of the rain-bow, which were exceedingly vivid. Taking, also, a feather of a convenient shape and bigness, and holding it at a due distance betwixt my eye and the sun, when near the horizon, there appear'd to me a variety of little rain-bows; with different and very vivid colours, none of which were constantly to be seen in the feather. The like phenomenon I have at other times produced, by interposing, at a proper distance, a piece of black ribband betwixt the setting sun and my eye.

20. Drop a little good syrup of violets upon white paper, and on this liquor let fall two or three drops of the spirit either of salt, vinegar, or other eminently acid liquor; and upon the mixture of these, you shall find the syrup immediately turn'd red. But if instead of spirit of salt, or that of vinegar, you drop upon the syrup a little oil of tartar *per deliquium*, or of the solution of pot-ashes, and stir them together with your finger, the blue colour of the syrup will in a moment be turn'd into a perfect green. And this syrup may be substituted for the infusion of *Lignum Nephriticum*, when we would examine whether the salt predominant in a liquor, or other body, wherein 'tis loose and in plenty, belongs to the tribe of acid salts. For if such a body turns the syrup of a red or reddish purple colour, it for the most part argues that body to abound with acid salt. But if the syrup be turn'd green thereby, the predominant salt seems to be of a nature contrary to acid. For as spirit of salt, oil of vitriol, *Aqua fortis*, spirit of vinegar, juice of lemons, &c. will turn syrup of violets red or reddish; so I have found that not only all the volatile salts of animal substances which I have used, as spirit of hart's-horn, of urine, of sal-armoniac, of blood, &c. but all the alkaline salts I have employ'd, as the solution of salt of tartar, of pot-ashes, of common wood-ashes, lime-water, &c. will immediately change that blue syrup into a perfect green. And by the same way, the changes that nature and time produce in the more saline parts of some bodies, may be discover'd; and

Syrup of violets, and the juice of blue-bottles, by a change of colour, distinguish an acid from an alkali.

PHYSICS. and such bodies chymically prepared, as belong neither to the animal kingdom, nor to the tribe of alkalies, have their new nature successfully examined. But to change the colour of this syrup, it requires that the body be more strong of the acid or other salt predominant in it, than is necessary to work upon the tincture of *Lignum Nephriticum*: and tho' the actions of these contrary salts will destroy each other, yet neither of them will restore the syrup to its native blue, as in the nephritic tincture; but each of them changes it into the colour which itself affects.

21. By dropping on the fresh juice of blue-bottles, or the *Cyanus vulgaris minor*, a little spirit of salt, it immediately turn'd to a red. And if instead of the acid spirit, I mix'd with it a little strong solution of an alkaline salt, it presently disclosed a lovely green; the same changes being, by those different sorts of saline liquors, producible in this juice, as in syrup of violets. And finding this blue juice, when fresh, to be capable of serving for an ink of that colour; I attempted, by moistening one part of a piece of white paper with the spirit of salt, and another with some alkaline or volatile liquor, to draw a line on the paper after it was dry, that should appear partly blue, partly red, and partly green: but the latter part of the experiment did not succeed well, tho' the blue and red were conspicuous enough to surprize those who were unacquainted with the trick. But lest it should be thought that volatile or alkaline salts change blue into green rather upon the score of the easy transition of the former colour into the latter, than upon account of the texture, wherein most vegetables, that afford a blue, seem to be allied; I shall add, that having dissolv'd blue vitriol in fair water, and put a lixiviate liquor and an urinous salt to distinct parcels of it, each of 'em turn'd the liquor not green, but of a deep yellowish colour, almost like that of yellow oker; which colour, the corpuscles thereby precipitated, retain'd.

22. The hint of the following experiment was afforded us by the practice of some *Italian* painters, who counterfeit ultra-marine azure* by grinding

* The *Prussian* blue is allow'd to excel the ultra-marine. And the preparation of it being very curious and useful, we shall here give an extract of that receipt for it which was lately communicated to the Royal Society by Dr. Woodward, as sent him from Germany. Take of crude nitre and crude tartar, each four ounces; powder them fine, mix them together; and after decrepitation, there will remain four ounces of a salt of tartar: whilst this is hot, pulverize it, and add thereto four ounces of well-dry'd ox-blood, in fine powder. Calcine the whole in a cover'd crucible, whereof it may fill two thirds. After this operation, lightly grind the matter in a mortar, and put it hot into

four pints of boiling water; and let it continue boiling for half an hour. Strain the decoction, dilute the black remaining substance with water, and boil and strain as before, till the water pour'd on becomes insipid. Add the several strainings together, and evaporate them to four pints. Then dissolve an ounce of *English* vitriol, calcined white, in six ounces of rain-water, and filtre the solution. Dissolve, likewise, half a pound of crude alum in two quarts of boiling water; and add this to the solution of the vitriol taken hot from the fire; pouring to them also the first lixivium, whilst thoroughly hot, in a large vessel. A great ebullition, and a green colour, will immediately ensue.

grinding verdigrease with sal-armoniac and some other saline ingredients, and suffering them to lie for a good while together in a dunghill; for we supposed, that the change of colour wrought in the verdigrease by this way of preparation, must proceed from the action of certain volatile and alkaline salts, abounding in some of the mixed concretes, and brought to make a further dissolution of the copper contained in the verdigrease; and therefore conjectured, that if both the verdigrease, and such salts, were dissolved in fair water, the small parts of both being therein more subdivided, and set at liberty, would have better access to each other, and thence incorporate much the sooner. And, accordingly, we found, that if, upon a strong solution of good *French* verdigrease, we poured a just quantity of oil of tartar, and shook them well together, a notable change of colour immediately succeeded; the mixture growing thick, and not transparent; but if you stay till the grosser part be precipitated, and settled at the bottom, you may obtain a clear liquor of an exceeding delightful colour. You must drop in a competent quantity of oil of tartar, otherwise the colour will not be so deep and rich; but if, instead of this oil, you employ a clear lixivium of pot-ashes, you may have an azure, somewhat lighter or paler than the former. And if, instead of either of these liquors, you make use of spirit of urine, or of hart's-horn, you will, according to the quantity and quality of the spirit poured in, obtain some further variety of blue liquors; and, by the help of this urinous spirit, we have made a surprizing blue liquor. But these azure-coloured liquors should be freed from the subsiding matter, which the salts of tartar, or urine, precipitate out of them, rather by being decanted, than by filtration; for, in the latter method, we have sometimes found the colour of them very much impaired.

The production of a blue colour.

23. That roses, held over the fume of sulphur, may quickly be thereby deprived of their colour, and have as much of their leaves, as the fume works upon, turned pale, we have already hinted; and it is a known experiment. But it may seem strange to one who has never considered the compound nature of brimstone, that tho' the fume of it, as we

Of a red.

ensue. Whilst this ebullition continues, pour the mixture out of one vessel into another, and afterwards let it rest. Then run the liquor thro' a linen, and let the pigment remain in the strainer; and when 'tis thus freed from its moisture, put it, by means of a wooden spatula, into a small new pot; pour upon it two or three ounces of spirit of salt, and there will immediately arise a most beautiful blue. Let the matter be well stirr'd, and set to rest for a night; and afterwards thoroughly edulcorate it with repeated affusions of rain-water; allowing a competent time for the precipitate to subside. Thus, at length, it will become ex-

quisitely blue. Lastly, let it drain upon the linen strainer, and dry it gently for use. The success of this process greatly depends upon the calcination. The crucible is first to be surrounded with coals, that it may grow gradually hot, and the matter leisurely flame and glow. This degree of heat must be continued till the flame and glowing decrease: then the fire should again be raised, that the matter may glow with an exceeding white heat; and but little flame appear above the crucible. The lixivia must be vehemently hot; and ought to be mix'd together in an instant. See *Philos. Transact.* N^o 381. p. 15—24.

PHYSICS.

have said, whitens the leaves of roses, yet the oil of sulphur *per campanam*, powerfully heightens the tincture of red roses, and makes it more red and vivid; as we have easily try'd, by putting some red rose-leaves, that having been long dry'd, had lost much of their colour, into a vial of fair water: for a while after the affusion of a convenient quantity of the oil, both the leaves themselves, and the water they were steep'd in, discover'd a very fresh and lovely red colour.

What quantity
of limpid liquor
a pigment may
tinge

24. It may serve to illustrate not only the doctrine of pigments, and of colours, but many other parts of mechanical philosophy, as odors, and other qualities are explain'd by the assistance of bodies extremely minute, to examine to how much of a colourless liquor, a very small parcel of a pigment may impart a discernible colour. And tho' scarce any thing of exactness can be expected from such trials, yet I presum'd I shou'd hence be able to shew a much further subdivision of the parts of matter into visible particles, than seems hitherto taken notice of, or imagin'd.

The most promising bodies, for such a purpose, might seem to be metals, especially gold, because of the multitude and minuteness of its parts, which might be argued from the great closeness of its texture. But tho' we try'd a solution of gold, made in *Aqua regia* first, and then in fair water, yet because we were to determine the pigment we employ'd, not by bulk, but weight; and because also the colour of gold is but weak, in comparison of that of cochineal, we rather chose this to make our experiments with. But, from a number of these, it may suffice to select one which was carefully made in vessels conveniently shap'd; to which I shall only premise, that the cochineal will be better dissolv'd, and have its colour far more heighten'd by spirit of urine, than by common water, or even rectified spirit of wine. One grain of cochineal, dissolved in a considerable quantity of spirit of urine, and then further diluted by degrees with fair water, imparted a discernible colour to six glasses of water, each containing forty-three ounces and a half; which amounts to above one hundred twenty-five thousand times its weight.

Acid, alkaline,
and urinous
salts, change the
colours of many
vegetable pro-
ductions

25. It may afford a considerable hint towards improving the art of dying, to know what change of colours are producible by the three several sorts of salts we have mention'd, in the juices, decoctions, infusions, and the more soluble parts of vegetables. The blue liquors lately made use of in our experiments, are far from being the only vegetable substances upon which acid, urinous, and alkaline salts, have the like operations to those above recited. Ripe privet-berries, for instance, being crush'd upon white paper, tho' they stain it with a purplish colour, yet if we let fall on one part of it, two or three drops of spirit of salt, and on the other, some strong solution of pot-ashes, the former liquor immediately turns that part of the thick juice, or pulp, on which it fell, into a lovely red; and the latter changes the part whereon it falls, into a delightful green. This experiment is, perhaps, very extensive, and serviceable to those who wou'd know how dying stuffs may be wrought upon by saline liquors. For I have found it to succeed in so many various berries, flowers, blossoms, and other finer parts

parts of vegetables, that my memory will not serve me to enumerate them; and it is surprizing to see, by what differently colour'd flowers or blossoms; for example, paper being stain'd, may, by an acid spirit, be immediately turn'd red, and by any alkali, or urinous spirit, green; so that even the bruise'd blossoms of *Mezereon*, gather'd in frosty weather, and those of pease crush'd upon white paper, how remote soever their colours are from green, wou'd in a moment pass into a deep degree of that colour, upon the touch of an alkaline liquor. And either of these new pigments may, by a sufficient affusion of a contrary liquor, be presently chang'd from red to green, and from green to red; which observation holds also in syrup of violets, the juice of blue-bottles, &c.

26. There are, however, some cases wherein these experiments will not uniformly succeed. And first, I try'd the operation of acid salts upon such vegetable substances as are in their own nature red; as syrup of clove-july-flowers, the clear express'd juice of buckthorn-berries, red-roses, infusion of brazil, and many others; on some of which, spirit of salt either made no considerable change, or only alter'd the colour from a darker to a lighter red. And as to the operation of the other sorts of salts, upon these red substances, I found it not very uniform; some red or reddish infusions, as of roses, being thereby turn'd into a dirty colour, inclining to green. Nor was the syrup of clove-july-flowers turn'd by the solution of pot-ashes, to a much better, tho' a somewhat greener colour. Another sort of red infusion was by an alkali not turn'd into a green, but advanc'd to a crimson; tho' there were other kinds of them, particularly the juice of buckthorn-berries, that readily pass'd into a lovely green.

27. Among other vegetables, which seem'd likely to afford exceptions to the general observation about the different changes of colours, produced by acid and sulphureous salts, we made trial upon the flowers of jessamin; they being both white, as to colour, and esteem'd of a more oily nature than other flowers. Taking, therefore, only the white parts of the flowers, and rubbing them somewhat hard with my finger, upon a piece of clean paper, it appear'd very little discolour'd thereby; nor had spirit of salt, wherewith I moisten'd one part of it, any considerable operation thereon: tho' spirit of urine, and, particularly, a strong alkaline solution, immediately turn'd the paper, tho' it had remain'd almost colourless, of a deep greenish yellow; which experiment I several times repeated with the like success. But a great degree of unctuousness seems unnecessary to the production of these effects; for when we try'd the experiment with the leaves of those pure white flowers that appear about the end of winter, and are commonly call'd snow-drops, the event was much the same with the last mention'd.

28. Another sort of instances, to shew how much the changes of colours effected by salts, depend upon the particular texture of the colour'd bodies, we have from several yellow flowers, and other vegetables, as marygold-leaves, primroses, fresh madder, &c. For these being rubb'd upon white paper, till they imbued it with their colour, I cou'd not find,

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by the addition of alkaline liquors, or of an urinous spirit, that they would turn either green or red; even the spirit of salt would not considerably alter their colour, only dilute it a little: tho', in some early primroses, it destroy'd the greatest part of the colour, and made the paper almost white again. Madder, also, afforded something peculiar; for having gather'd some roots of it, whilst its yellow juice was fresh express'd upon white paper, an alkaline solution being dropp'd upon it, turn'd it neither green nor white, but red; and the bruised madder it self, drench'd with the like alkaline solution, exchange'd its yellowness for a redness.

29. It may be of use, towards discovering the nature of the changes which the alimental juices undergo in different vegetables, in their different degrees of maturity, to observe what operation acid, urinous, and alkaline salts will have upon those juices. To shew my meaning by an example, I took from the same cluster one black-berry full ripe, and another that had not yet out-grown its redness; and rubbing a piece of white paper with the former, I observed, that the juice adhering to it, was of a dark reddish colour, full of little black specks; and that this juice, by a drop of a strong lixivium, would immediately turn into a deep greenish colour; by as much urinous spirit, into a colour near allied to the former, tho' fainter; and by a drop of spirit of salt, into a fine light red: but the red berry, rubbed in like manner upon paper, left on it a red colour, which was very little altered by the same acid spirit; and from the urinous and lixivate salts, received changes of colour, different from those produced in the dark juice of the ripe black-berry.

I remember also, that tho' the infusion of damask roses would be heighten'd by acid spirits, to an intense degree of redness, and, by lixiviate salts, be brought to a darkish green; yet putting a rose, whose leaves were perfectly yellow, in a solution of salt of tartar, it afforded a green bluish tincture; but, by means of an acid liquor, I could not obtain a red one; the saline spirit I employ'd, only a little diluting the yellowness of the leaves. And if I were in the islands of *Banda*, where cloves so greatly prosper, I should try what operation our three differing kinds of salts would have upon the juice of this spice; which eminent authors inform us is at first white, afterwards green, and then reddish, before 'tis beaten off the tree; after which, being dry'd, it grows blackish, as we see it. And one of the latest botanic writers informs us, that the flower grows upon the top of the clove itself, consisting of small leaves, like a cherry blossom, but of an excellent blue.

'Tis very proper to take notice of the particular seasons wherein the vegetables, designed for the nicer experiments, are gathered. That diligent botanist, Mr. *Parkinson*, tells us, that "of buckthorn-berries are made three several sorts of colours; being gathered green, and kept dry, they are call'd sap-berries; which being steep'd in alum-water, give a fair yellow colour, used by painters, book-binders, and leather-dressers, who also make a green colour, called sap-green, taken from the berries, when they are black; that being bruised, and put into a brass kettle, and
" there

“ there suffered to remain for three or four days, with some beaten alum put to them, they are afterwards pressed, and the liquor usually put into bladders, and hung up till it be dry: this, he says, is afterwards dissolved in water, or wine, but canary is the best to preserve the colour from starving. The third is a purplish colour, made of the berries, suffered to grow upon the bushes, till the middle, or end of *November*, when they are ready to fall of themselves.” And I try’d, with success, to make such a kind of pigment as painters call sap-green, by a way not unlike that here delivered by our author. Much after the same manner, also, they make sap-green in the colour-shops.

30. Many bodies, digested in well-closed vessels, change their colour in tract of time, as rectify’d spirit of hart’s-horn; and the same is evident in the precipitations of amalgams of gold and mercury, without addition; where, by the continuance of a due heat, the silver-coloured amalgam is reduced into a shining red powder. And many other instances of the like kind, might be produced. Now, in these operations, there appears no reason why we should attribute the new colours to the action of a new substantial form; nor to any increase, or decrease of the salt, sulphur, or mercury of the matter that acquires them. For the vessels are closed, and these principles, according to the chymists, are ingenerable, and incorruptible. So that the effect seems to proceed from the heat, agitating and discomposing the corpuscles of the body exposed to it; which, in process of time, so changes its texture, that the transposed parts modify the incident light, otherwise than when the matter appeared of another colour.

31. Among the several changes of colour which bodies acquire, or disclose, by digestion, it is very remarkable, that chymists find a redness, rather than any other colour, in most of the tinctures they draw; and even in the more gross solutions they make, of almost all concretes that abound either with mineral or vegetable sulphur; tho’ the menstruum employ’d about these solutions, or tinctures, be never so limpid. This we have observed in abundance of tinctures, drawn with spirit of wine from jalap, guaiacum, and many other vegetables; and not only in the solutions of amber, benjamin, &c. made with the same menstruum; but also in several mineral tinctures. And, not to urge that familiar instance of the ruby of sulphur, as chymists call the solution of flowers of brimstone, made with the spirit of turpentine; nor to take notice of other more known examples of the aptness of chymical oils to produce a red colour with the sulphur they extract or dissolve; ’tis remarkable, that both acid and alkaline salts, tho’ in most other cases, of contrary operations, will, with many bodies that abound in sulphureous, or oily parts, produce a red; as is manifest in the vulgar instances of the tinctures, or solutions of sulphur, made with lixiviums, either of calcined tartar, or pot-ashes, and other obvious examples; and in that the true glass of antimony, extracted with some acid spirits, yields a red tincture. And a certain acid liquor will, in a moment, turn oil of turpentine into a deep red.

Changes of colour by digestion, &c. particularly a redness.

PHYSICS. red*. Among the many instances I could produce of the easy production of redness, by the operation of a saline spirit, as well as of spirit of wine, two or three of them deserve to be particularly mentioned.

32. But, before I set them down, 'tis proper to premise, that there seems to be a manifest disparity betwixt red liquors; for some of them may be said to have a genuine redness, in comparison of others that have it yellowish. Thus if a good tincture of cochineal be diluted never so much with fair water, it will not become a yellow liquor. But balsam of sulphur, tho' in a large quantity, it appear to be of a deep red; yet if you shake the containing glass, or pour a few drops on a sheet of white paper, spreading them on it with your finger, what falls back along the sides of the glass, or stains the paper, will appear yellow. And there are many tinctures, such as that of amber, made with spirit of wine, which will appear either yellow or red, according as the vessels that contain them are slender or broad.

33. To come now to the experiments we designed. First, oil or spirit of turpentine, tho' clear as fair water, being digested upon the pure white sugar of lead, has, in a short time, afforded us a high red tincture; probably a good medicine.

34. Secondly, take common brimstone, and sal-armoniac, of each, finely powdered, five ounces; of beaten quick-lime, six ounces: mix these powders exquisitely, and distil them in a retort, placed in sand, by degrees of fire; giving, at length, as intense an heat as you can in sand; and there will come over a volatile tincture of sulphur, which also may prove an excellent medicine. Now, tho' none of the ingredients here be red, the distilled liquor is of that colour; and, if it be well drawn, will, upon a little agitation of the vial, first unstopped, send out a great white fume, which sometimes spreads wide, and is very offensive: and tho' the liquor itself be red, and its fumes white; yet it will dye the fingers black.

35. The last experiment, I shall now produce, to shew how apt bodies, abounding in sulphureous parts, are to afford a red colour, is one wherein the operation of a clear saline spirit upon a white, or whitish body, may produce a redness in the twinkling of an eye. We took then, a little essential oil of aniseeds congealed, and spread it with a knife upon a piece of white paper; when mixing with it a drop or two of well rectified oil of vitriol, there immediately emerged, with some heat and smoke, a blood-red colour.

* Mr. Coles found, that a certain sulphureous spirit, mixed with a volatile alkali, gives a red colour in a moment. This spirit he made, by distilling two or three pound of benjamin with a little sand, in a retort, *ad siccitatem*; and putting the oil, spirit, and flowers together in a filtre of paper, when the spirit came first thorough. Put two parts of this spirit to one of spirit of sal-armoniac, and shake

the containing glass, and the liquor will instantly turn red; tho' they both were clear before. The more the vessel is shook, the deeper will be the red. And this effect is produced without any effervescence; whence the inventor conceives the experiment may be of use in accounting for sanguification. *Philos. Transf.* N° 228. p. 542.

36. Let it be here observed, once for all, that, in many of these experiments the colour produced is often very subject to degenerate. However, since the changes we have set down happen presently upon the operation of the bodies on each other, or at the times specified; that is sufficient to shew what we intend. For it is not essential to the genuineness of a colour to be durable; a fading leaf, that is ready to rot and moulder into dust, may have as true a yellow as a wedge of gold. And I have several times observed, that the mixture made by the oils of vitriol, and of aniseeds, tho' it acquire a thicker consistence than either of the ingredients, quickly loses its colour, and turns to a dark grey; at least in the superficial parts, where 'tis exposed to the air. This degeneration of colours may, in many cases, indeed, proceed from the further action of the saline corpuscles, and other ingredients upon one another; yet much of the sudden change may often be ascribed to the air. Thus we have sometimes observed window-curtains of a light colour, to have that part of them which was exposed to the air, when the window stood open, of one hue; and the lower part, that was screen'd from the air by the wall, of another colour. And *Parkinson* says, of the plant turnsol, that "its berries, when full ripe, have within them, between the outward skin and the inward kernel, a certain juice, which rubbed upon paper or cloth, at the first appears of a fresh lovely green, but presently changes into a kind of bluish purple. He adds, that the same cloth, afterwards wet in water, and wrung out, will turn the water into a claret-colour. And these rags of cloth, says he, are those usually call'd turnsol in the drug-gifts shops." I also remember, that letting some of the deep red juice of buckthorn-berries drop upon a piece of white paper, and leaving it there for many hours, till the paper was grown dry again; I found the juice degenerated to a dirty kind of greyish colour; which, in a great part of the stain'd paper, seem'd not to have so much as an eye of red: tho' a little spirit of salt, or dissolved alkali, would turn this unpleasant colour into a red or green. And, to satisfy myself that this degeneration of colour did not proceed from the paper, I dropp'd some of the same juice upon a white glazed tile; and, permitting it to dry thereon, I found it then also lost its colour.

Having dissolved good silver in *Aqua fortis*, and precipitated it with spirit of salt, upon first decanting the liquor, the remaining matter was purely white; but, after it had lain a while uncovered, the part of it that lay contiguous to the air, not only lost its whiteness, but appeared of a very dark, and almost blackish colour; but if the part that was contiguous to the air, were gently taken off, the subjacent part of the same mass would appear very white; till that also, having continued a while exposed to the air, would likewise degenerate. Whether the air produce this effect by the means of a subtiler salt, by a penetrating moisture, by soliciting the avolation of certain parts of the bodies to which 'tis contiguous, or by some other way, I leave to be further considered. 'Twere easy here to add many other instances of redness, resulting from the digestion of bodies.

PHYSICS. I have often seen, upon the borders of *France*, a sort of pear, which, digested with a little wine in a vessel exactly closed, will soon appear throughout of a deep red colour. Nay, even pure white salt of tartar, and limpid spirit of wine, will, by long digestion, acquire a redness. Some other obvious changes of colours frequently happen; such as is the blackness of bodies burned in the open air, &c. But I shall not at present examine into the causes of these changes; tho' certainly the reason why the roots of different bodies are almost all of them black; why so much the greater part of vegetables should be rather green than of any other colour; and, particularly, why gentle heat, so frequently, in chymical operations, produces rather a redness, than another colour in digested menstrea, may very well deserve a serious inquiry.

Different effects of an acid, in the production of colours, reconciled.

37. It may seem strange, that if the crimson solution of cochineal, the juice of black-cherries, or of some other vegetables that afford the like colour, be let fall upon a piece of paper; a drop or two of an acid spirit, such as spirit of salt, or *Aqua fortis*, will immediately turn it into a fair red; whilst an infusion of brazil, in fair water, will have its redness destroy'd by a little spirit of salt, or *Aqua fortis*; and be turn'd either yellow or pale. But if we consider the case attentively, the action of the acid spirit seems, in both these experiments, only to weaken the colour of the liquor wherein it falls; and so tho' it destroys redness in the tincture of brazil, but produce it in the tincture of cochineal, its operations may be still uniform: since as crimson is little else than a very deep red, with perhaps an eye of blue; so some kind of reds seem to be little else than heightned yellow; and consequently in such bodies the yellow seems to be but a diluted red. And accordingly, alkaline solutions, and urinous spirits, which seem dispos'd to deepen the colours of most vegetable liquors, will not only restore the solution of cochineal, and the infusion of brazil to the crimson, from which the spirit of salt had changed them into a truer red; but will also heighten the yellow juice of madder into red, and advance the red infusion of brazil to a crimson. But perhaps it will be much safer to derive these changes from the vary'd texture, than from the peculiar kinds of bodies.

The colours of the fumes of bodies, and of the substances they form, observed in distillations, &c.

38. It might greatly contribute to the history of colours, if chymists would give us a faithful account of those to be observed in the steams of bodies, sublimed, or distilled, and of the productions made by the coalition of those steams. Thus, for instance, we find in distilling pure salt-peter, that at a certain juncture of the operation, the body, tho' it seem either crystalline, or white, affords very red fumes; whilst tho' vitriol be either green or blue, its spirit comes over in whitish vapours. The like colour I have found in the fumes of several other concretes, of different colours and natures; especially when distill'd with strong fires. And even soot, as black as it is, has fill'd our receivers with white fumes. No less observable may the distill'd liquors be, into which such fumes convene: for tho', by skill and care, a reddish liquor is attainable from nitre, yet the common spirit of it, in the preparation whereof abundance, of these red fumes pass over

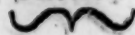
over into the receiver, has no appearance of red. Neither the spirit of vitriol, nor of foot, is white; and the empyreumatical oils of woods, and other concretes, are either of a deep red, or of a colour between red and black. But 'tis very remarkable, that notwithstanding the great variety of colours, to be met with in herbs, flowers, and other bodies usually distill'd in *Balneo*; yet all the waters and spirits that first come over by that way of distillation, leave the colours of the concretes behind them; tho', indeed, there are one or two vegetables, not commonly taken notice of, whose distill'd liquors carry over the tincture of the concrete with them. And as in distillation, so in sublimation, it were worth while to take notice of what happens to our purpose, by performing that operation in conveniently shap'd glasses, wherein the colour of the ascending fumes may be discern'd; for it might afford a naturalist good information, to observe the agreements, or differences betwixt the colours of the ascending fumes, and those of the flowers they compose by their convention. It is evident, that these flowers do, many of them, greatly differ in point of colour, not only from one another, but often from the concretes that afford them. Thus, tho' camphire and brimstone afford flowers much of their respective colours, except that those of brimstone are a little paler than the lumps that yielded them; yet the flowers of red benjamin are either white, or whitish. And, to omit other instances, even that black mineral, antimony, may be made to afford flowers, some of them red, others grey, and which is more strange, some of them purely white. And glassmen, by exquisitely mixing a convenient proportion of brimstone, sal-armoniac, and quick-silver, and subliming them together, make a sublimate of an excellent blue. And tho', upon making this experiment, we found the sublimate to be far from a lovely colour, yet, in some parts, it seem'd bluish, and was, at least, of a colour very different from either of the ingredients; which is sufficient for our present purpose. But a much finer colour is promis'd by some empirics, who tell us, that orpiment sublim'd, will afford, among the parts of it that fly upwards, some little masses, which, tho' the mineral it self be of a good yellow, will be red enough to emulate rubies, both in colour and transparency. This experiment may, for ought I know, sometimes succeed; for I remember, that having in a small bolt-head, purposely sublim'd some powder'd orpiment, we could, in the lower part of the sublimate, discern, here and there, reddish lines; tho' much of the upper part of it consisted of a matter not only purely yellow, but transparent. And we have also, by this means, obtain'd a sublimate, which, tho' it consisted not of rubies, yet small pieces of its lower part, that were numerous, afforded a pleasant reddish sparkling colour.

39. Take the dry'd buds, or blossoms, of the pomegranate-tree, commonly called balaustins, pull off the reddish leaves, and, by a gentle ebullition, or a competent infusion of them in fair water, extract a faint reddish tincture, which, if the liquor be turbid, you may clarify by the filtre; into this, if you pour a little spirit of urine, or some other

Various changes of colour, caused by saline spirits in the tinctures of vegetables.

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spirit, abounding in the like sort of volatile salt, the mixture will presently turn of a dark greenish colour: but if, instead of such a spirit, you drop into the simple infusion, a little rectify'd spirit of sea-salt, the pale liquor will immediately grow more transparent, and acquire a high red, like that of rich claret; and this may as suddenly be destroy'd, and turn'd into a dirty bluish green, by the affusion of a moderate quantity of the spirit of urine. This experiment gives light to two others that I met with in *Gassendus*. The experiments as we made them, were these. We took a glass of luke-warm water, and therein immerg'd a quantity of the leaves of *sena*, upon which there appear'd no redness in the water; but dropping into it a little oil of tartar, the liquor soon became reddish; tho' by a little oil of vitriol, such a colour could not be extracted from the infused *sena*. On the other hand, we took some dryed red rose-leaves, and shaking them in a glass of fair water, they communicated no redness to it; but upon the affusion of a little oil of vitriol, the water was immediately turn'd red; which it would not have been, if instead of oil of vitriol, we had employ'd oil of tartar. Our author tells us, there was no redness either in the water, the leaves of *sena*, or the oil of tartar; yet we have found that by steeping *sena* for a night in cold water, it would afford a very deep yellow, or reddish tincture, without the help of oil of tartar; which seems to do little more than assist the water more suddenly to extract a plenty of that red tincture wherewith the leaves of *sena* abound: for having made a tincture of *sena*, only with fair water, and decanted it from the leaves before it grew reddish, we could not perceive, by dropping some oil of tartar into it, that the colour was considerable, tho' it were thereby a little heighten'd into a redness. And the same thing may be alledg'd in the experiment with red rose-leaves; for we found that such leaves, by bare infusion; for a night and a day, in fair water, afforded a tincture bordering upon redness; and that colour being conspicuous in the leaves themselves, seem'd not wholly produc'd, but extracted by the oil of vitriol. But to improve the experiment, take the tincture of red rose-leaves, made with a little oil of vitriol, and a large quantity of fair water, pour off this liquor into a clear vial, half fill'd with limpid water, till the water held against the light hath acquired a competent redness, without losing its transparency: into this tincture, drop, leisurely a little spirit of urine; and shaking the vial, which you must still hold against the light, you will see the red liquor immediately turn'd into a fine greenish blue; a colour not to be found in any of the bodies upon whose mixture it ensued. And this change is the more remarkable, because tho' the degeneration of blue into red, be usual, the turning of red into blue, is very unfrequent. If, upon the falling of each drop of spirit of urine, you shake the vial containing the red tincture, you may observe a pretty variety of colours in the passage of that tincture from a red to a blue. And sometimes we have thus obtain'd such a liquor, as being view'd against, and from the light, seem'd faintly to emulate the tincture of *Lignum Nephriticum*. And if you make the tincture of red-roses very high, and, without diluting it with fair water, pour on the



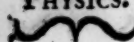
the spirit of urine, you may have a blue so deep as to render the liquor opaque; but if it be dropp'd upon white paper, the colour will soon disclose itself. Having, also, made the red, and consequently the blue tincture very transparent, and suffer'd it to rest in a small open vial for a day or two; we found that not only the blue, but the red colour, likewise, was vanish'd; the clear liquor remaining of a bright amber colour, at the bottom whereof subsided a large, light feculency, almost of the same colour; which seems to be nothing but the ting'd parts of the rose-leaves, drawn out by the acid spirits of the oil of vitriol, and precipitated by the volatile salt of the spirit of urine. This makes it the more probable, that the redness drawn by the oil of vitriol, was as well an extraction of the tinging parts of the roses, as a production of redness. And lastly, the colour of the tincture of roses, may be chang'd by many other sulphureous salts; as a strong solution of pot-ashes, oil of tartar, &c. tho' these are seldom so free from feculency, as the spirit of urine becomes by distillation. And I have a way of producing a full purple, by employing a liquor not made red artificially, instead of the tincture of red-roses; for having by infusing the powder of log-wood for a while in fair water, made that liquor red, I dropp'd into it a little urinous spirit, as that of sal-armoniac, by which the colour was instantly turn'd into a rich and lovely purple. But care must be taken not to let fall into a spoonful of the tincture, above two or three drops of the spirit, lest the colour become so deep as to make the liquor opaque. And if instead of fair water, I infus'd the log-wood in water, made a little sour by the acid spirit of salt; I obtain'd neither a purple, nor a red, but only a yellow liquor.

40. The following experiment is very surprizing; and, of all I have yet met with, seems the fittest to enforce our doctrine of colours, and to shew them not to be inherent qualities, flowing from the substantial forms of the bodies whereto they are said to belong; since by a bare mechanical change of texture, in the minute parts of bodies, colours may, in a moment, be generated quite *de novo*, and utterly destroy'd: for there is this difference betwixt the ensuing experiment, and most of the others we deliver, that in this, the colour of a body is not chang'd into another, but betwixt two bodies, each of them, a-part, colourless, there is, in a moment, generated a very deep colour, which, if it were let alone, would be permanent; and yet, by a very small parcel of a third body, that has no colour of its own, this otherwise permanent colour, will instantly be so utterly destroy'd, as to leave not the least sign of any colour in the whole mixture. This experiment is very easy, and thus perform'd. Take good common subli-

A colour instantly generated, and perfectly destroy'd.

mate, and fully satiate with it what quantity of water you please, carefully filtre the solution thro' clean close paper, that it may drop down clear and colourless as fountain water. Then, when you would shew the experiment, put about a spoonful of it into a small wine-glass, or other convenient vessel, and letting fall therein three or four drops of good oil of tartar *per deliquium*, well filtered, that it may likewise be without colour, these two limpid liquors will, in the twinkling of an eye, turn into an opaque

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 mixture of a deep orange-colour; which, by continually shaking the glass in your hand, you must prevent from settling too soon at the bottom. And when the spectators have view'd this first change, presently drop in four or five drops of oil of vitriol, and continuing to shake the glass pretty strongly, that it may the sooner diffuse itself, the whole colour will now immediately disappear, and all the liquor in the glass become clear and colourless, as before, without the least sediment at the bottom. But for the more neat performance of this experiment, observe, First, not to take too much of the solution of sublimate, nor drop in too much of the oil of tartar, to avoid the necessity of putting in so much oil of vitriol, as to cause an ebullition.

Secondly, 'tis convenient to keep the glass constantly in motion, both for the better mixing of the liquors, and hindering, as we said, the yellow substance from subsiding, which it would otherwise soon do; tho' when 'tis subsided, it will retain its colour, and be deprived of it by the oil of vitriol.

Thirdly, if any yellow matter stick to the sides of the glass, 'tis but inclining the vessel, till the clarify'd liquor can wash along it, and the liquor will presently imbibe it, and deprive it of its colour.

The chymical
reason of this
phenomenon.

My notion of the different natures of the several tribes of salts, having led me to devise this experiment; I can easily assign the chymical reason of the phenomenon. Having then observed, that mercury, dissolved in some menstrua, would yield a dark yellow precipitate; and supposing that common water, and the salts that stick to the mercury, would, in this respect, be equivalent to those acid menstrua which work upon the quick-silver, on account of their saline particles; I substituted a solution of sublimate in fair water, instead of a solution of mercury in *Aqua fortis*, or spirit of nitre: then, considering that what makes the yellow colour, is indeed but a precipitate, obtain'd by means of the oil of tartar, which generally precipitates metalline bodies corroded by acid salts; so that the colour in our case results from the coalition of the mercurial particles with the saline ones, wherewith they were formerly associated; and with the alkaline particles of the salt of tartar, that float in the oil. Considering also, that very many effects of lixivious liquors upon the solutions of other bodies may be destroyed by acid menstrua, I concluded, that if I chose a very strong acid liquor, which by its incisive power might undo the work of the oil of tartar, and disperse again those particles which the other had, by precipitation, associated into such minute corpuscles as were, before, singly invisible, they would disappear again, and consequently leave the liquor as colourless as before the precipitation was made. This seems to be the chymical reason of the experiment, and is that which induced me to make it. But to give a perfect mechanical solution of the whole phenomenon, is more than I pretend to. However, the yellowness of the mercurial solution, and oil of tartar, seems produced by the precipitation occasioned by the affusion of the latter; as the destruction of the colour proceeds from the dissipation of that curdled matter, whose texture is destroy'd, and the matter itself dissolved into minute and invisible particles, by the strong acid men-

menstruum: which is the reason why there remains no sediment at the bottom; the infused oil taking it up, and resolving it into invisible parts, as water dissolves salt or sugar. PHYSICS.

But, to confirm my conjecture as to the chymical reason of our experiment, I might add, that I cannot only make the mercury precipitate out of the first simple solution quite of a different colour from that mentioned; but if, instead of altering the precipitating liquor, I altered the texture of the sublimate in such a manner, as my notion about salts requires, I can produce the same phenomenon. For having sublimed together equal parts of sal-armoniac, and sublimate, first diligently mixed, the ascending flowers dissolved in fair water, and filtered, gave a solution limpid and colourless, like that of the other sublimate; and yet an alkali dropped into this liquor, did not turn it yellow, but white. And, upon the same foundation, we may with quick-silver, unassisted by common sublimate, prepare another sort of flowers dissoluble in water, without discolouring it; which will also change its colour in the same manner. And so much does the colour depend upon the texture resulting from the convention of the several sorts of particles, that tho', in our experiment, oil of vitriol destroys the yellow colour; yet with quick-silver, and fair water, by the help of oil of vitriol alone, we may easily make a kind of precipitate, of a fair and permanent yellow; as we shall see hereafter. I here make choice of oil of vitriol, because, when well rectified, 'tis not only colourless and scentless, but extremely strong and incisive. And common *Aqua fortis* will not perform the same thing so well.

This experiment may be several times repeated with the same parcels of the liquors; for I found, that after I had clarify'd the orange-coloured mixture, by the addition of as little of the oil of vitriol as would suffice to produce the effect, I could, at pleasure, re-produce the opake colour, by dropping in fresh oil of tartar; and destroy it again, by the re-affusion of more of the acid menstruum: tho', by the addition of so much new liquor, in proportion to the mercurial particles, the colour will, at length, appear weak and faint.

By boiling crude antimony in a strong and clear lixivium, you may separate a substance from it, call'd by some chymists, it's sulphur; which, when let fall by the liquor, upon its refrigeration, often settles in flakes of a yellow substance. Considering therefore, that common sulphur, boiled in a lixivium, may be precipitated out of it by *Rhenish*, or white-wine, which are sourish liquors, and contain an acid salt; and having found, by tryal, that with other acid liquors I could precipitate out of lixivious solvents, some other mineral concretions, abounding with sulphureous parts, of which sort is crude antimony; I concluded it easy to precipitate the antimony, thus dissolved, with the acid oil of vitriol. And tho' common sulphur yields a white precipitate, called *Lac Sulphuris*; yet I supposed the precipitated antimony would be of a deep yellow colour; as well, if made with oil of vitriol, as by refrigeration, and length of time. From hence 'twas easy to deduce this experiment, that if we put into one glass some of this

PHYSICS. this fresh filtered solution of antimony; and into another, some of the orange-coloured mixture, made with a mercurial solution, and oil of tartar; a few drops of oil of vitriol, let fall into the latter glass, would turn the deep yellow mixture into a clear liquor; whilst a little of the same oil dropped into the other glass, presently turn'd the moderately clear solution into a deep yellow substance. But this succeeds not well, unless you employ a lixivium that has lately dissolved the antimony, and before it suffers it to fall: yet, in summer-time, if the lixivium have been duly impregnated, and well filtered after it is quite cold, it will for some days retain antimony enough to exhibit, upon the affusion of the corrosive oil, as much of a good yellow substance as is necessary to satisfy the spectators of the possibility of the experiment.

To find what kind of salt, whether acid, volatile or fix'd, predominates in an assign'd liquor, or saline body.

The distinction of salts into acid, volatile, and fixed or alkaline, may possibly be so useful in natural philosophy, * as to render it an acceptable corollary of the preceding experiment, from thence to deduce a method of trying which, or whether any one of those salts is predominant in chymical liquors. We have already shewn a way, by means of the tincture of *Lignum Nephriticum*, or of syrup of violets, to discover whether a salt proposed be acid or not; yet we can thereby only find in general, that particular salts belong not to the tribe of acids: but cannot determine whether they belong to the tribe of the urinous; (under which I comprehend all those volatile salts of animal, or other substances, that are contrary to acids) or to that of alkalies: for both these salinophilphureous salts will restore the blue colour to that tincture, and turn the syrup of violets green. The present experiment, therefore, commodiously supplies this deficiency. I found, that all those chymical salts I thought fit to make trial of, would, if they were of a lixivious nature, make, with sublimate dissolved in fair water, an orange-tawny precipitate;

* About the nature of salts, Sir *Is. Newton* thus argues. "When mercury sublimate is re-sublimed with fresh mercury, and becomes *Mercurius dulcis*, which is a white tasteless earth, scarce dissolvable in water; and *Mercurius dulcis* re-sublimed with spirit of salt, returns into mercury sublimate; and when metals corroded with a little acid, turn into rust, which is an earth tasteless, and indissolvable in water; and this earth, imbibed with more acid, becomes a metallic salt; and when some stones, as spar of lead, dissolved in proper menstrua, become salts; do not these things shew, that salts are dry earth and watery acid united by attraction; and that the earth will not become a salt without so much acid as makes it dissolvable in water? Do not the sharp and pungent tastes of acids arise from

"the strong attraction, whereby the acid particles rush upon, and agitate the particles of the tongue? And when metals are dissolved in acid menstrua, and the acids in conjunction with the metal, act after a different manner, so that the compound has a different taste much milder than before, and sometimes a sweet one; is it not because the acids adhere to the metallic particles, and thereby lose much of their activity? And if the acid be in too small a proportion to make the compound dissolvable in water; will it not, by adhering strongly to the metal, become unactive, and lose its taste, and the compound be a tasteless earth? For such things as are not dissolvable by the moisture of the tongue, act not upon the taste. As gravity makes the sea flow round the denser and weightier

"tier

tate; but if they were of an urinous nature, the precipitate would be white and milky. So that having always at hand some syrup of violets, and a solution of sublimate; I can, by the help of the former, immediately discover whether a proposed salt, or saline body, be of an acid nature; if it be, I need inquire no further; but if not, I can very easily and readily distinguish between the other two kinds of salts, by the white, or orange-colour, immediately produced by letting fall a few drops or grains of the salt to be examined, into a spoonful of the clear solution of sublimate. Thus, for example, it has been supposed, that when sal-armoniac, mixed with an alkali, is forced from it by the fire in close vessels, the volatile salt thence obtained, is but a more fine and subtle sort of sal-armoniac; which, 'tis presumed, this operation only purifies more exquisitely than common solutions, filtrations, and coagulations. But this opinion may easily be proved erroneous, as by other arguments, so particularly by our method of distinguishing the tribes of salts. For the saline spirit of sal-armoniac, as it is in many other manifest qualities very like the spirit of urine; so, like that, it will instantly turn syrup of violets to a lovely green; a solution of good verdigrease into an excellent azure; and make the solution of sublimate deposite a white precipitate. Therefore, in most experiments, where I only design to produce a sudden change of colour; I scruple not to use spirit of sal-armoniac, when at hand, instead of spirit of urine; as indeed it seems chiefly to consist of the volatile urinous salt. And by this way of examining chymical liquors, we may not only, in general, conclude affirmatively, but, in some cases, negatively.

Thus, since spirit of wine, and, as far as I have try'd, the chymical oils will not turn syrup of violets red, or green; nor the solution of sublimate,

“ tier parts of the globe of the earth;
 “ so the attraction may make the watry
 “ acid flow round the compacter parti-
 “ cles of earth, for composing the parti-
 “ cles of salt. For otherwise the acid
 “ would not do the office of a medium
 “ between the earth and common water,
 “ for making salts dissolvable in the wa-
 “ ter; nor would salt of tartar readily
 “ draw off the acid from dissolved metals,
 “ nor metals the acid from mercury.
 “ Now, as in the great globe of the earth
 “ and sea, the densest bodies by their
 “ gravity sink down in water, and al-
 “ ways endeavour to go towards the
 “ centre of the globe; so in particles
 “ of salt, the densest matter may al-
 “ ways endeavour to approach the cen-
 “ tre of the particle: so that a particle
 “ of salt may be compared to a chaos,
 “ being dense, hard, dry, and earthy in
 “ the centre; and rare, soft, moist, and
 “ watry in the circumference. And

“ hence it seems to be, that salts are of a
 “ lasting nature, being scarce destroyed,
 “ unless by drawing away their watry
 “ parts by violence, or by letting them
 “ soak into the pores of the central
 “ earth by a gentle heat in putrefac-
 “ tion, till the earth be dissolved by the
 “ water, and separated into smaller par-
 “ ticles; which, by reason of their small-
 “ ness, make the rotten compound ap-
 “ pear of a black colour. Hence also it
 “ may be, that the parts of animals and
 “ vegetables preserve their several forms,
 “ and assimilate their nourishment; the
 “ soft and moist nourishment easily chang-
 “ ing its texture, till it becomes like the
 “ dense, hard, dry, durable earth in the
 “ centre of each particle. But when the
 “ nourishment grows too moist to be assi-
 “ milated, or the central earth grows too
 “ feeble to assimilate it, the motion ends
 “ in confusion, putrefaction, and death.”

Newton. Optic. p. 360—362.

white

PHYSICS. white or yellow, I infer, with probability, that either they are destitute of salt; or have such as belongs not to either of the three grand families mention'd.

And thus, upon examining the spirit of oak, or of such like substances, forc'd over the retort, I found by this means, amongst others, that those chymists are much mistaken, who make it a simple liquor, and one of their hypostatical principles; for, not to mention what phlegm it may have, a few drops of one of this sort of spirits, mix'd with a large proportion of syrup of violets, turn'd it purplish; by the affinity of which colour to redness, I conjectur'd that the spirit contain'd some acid corpuscles, and accordingly found, that it would destroy the blueness of the nephritic tincture; and that being put upon corals, it would corrode them like common spirit of vinegar, and other acid liquors. And to examine whether there were not a great part of the liquor of other than an acid nature; having separated the four part from the rest, we concluded, the remaining part, tho' it had a strong taste, as well as smell, to be of a nature different from that of any of our three sorts of salts; for it did as little as spirit of wine, and chymical oils, alter the colour of syrup of violets, and a solution of sublimate: whence we also inferr'd, that the change made of that syrup into a purple colour, was effected by the vinegar, that proved one of the ingredients of the liquor which usually passes for a simple, or uncompounded spirit.

And by the same way we may examine and discover many changes that are produced in bodies, either by nature or art; for both are able, by changing the texture of some substances, to qualify them to operate after a new manner, upon the syrup, or solution. Thus I have discover'd, that there are factitious bodies, which, tho' they run as readily as salt of tartar, belong, in other respects, not to the family of alkalies, much less to that of volatile or acid salts. Perhaps, too, I know a way of making a highly operative saline body, that shall neither change the colour of syrup of violets, nor precipitate the solution of sublimate. And I can, likewise, conceal the liquors wherewith I make such changes of colours as those lately mention'd, by quite altering the texture of some ordinary chymical productions.

We may here observe, that the reason why our method of examining salts, succeeds in the solution of sublimate, depends upon the particular texture of that solution, as well as upon the different natures of the saline liquors employ'd to precipitate it. For gold dissolv'd in *Aqua regia*, whether it be precipitated with oil of tartar, which is an alkali, or spirit of sal-armoniac, which is a volatile salt, will either way afford a yellow substance; tho' oil of vitriol, it self, would not precipitate the metal out of the solution, or destroy the colour of it; notwithstanding that oil will readily precipitate silver dissolv'd in *Aqua fortis*. And if you dissolve pure silver in *Aqua fortis*, and suffer it to shoot into crystals, the clear solution of these made in fair water, will afford a very white precipitate, whether occasion'd by an alkali, or an acid spirit; tho' with spirit of sal-armoniac, made with quick-

quick-lime, I cou'd obtain nothing like it ; this volatile spirit, as also that of urine, scarce doing any more than striking down a very small quantity of matter, which was not so much as whitish ; so that the remaining liquor, being suffer'd to evaporate its superfluous moisture, the greatest part of the metalline corpuscles, with the saline ones that had imbibed them, coagulated together into salt ; as is usual in such solutions, when the metal has not been precipitated.

41. From spirit of urine, made after fermentation, and a due proportion of copper-filings, having obtain'd a very lovely azure solution ; I pour'd into a clean glass, about a spoonful of this liquor, and found I could, by shaking into it some drops of strong oil of vitriol, immediately deprive it of its deep colour, and make it look like common water. And if into a small spoonful of a solution of good *French* verdigrease, made in fair water, I dropp'd and shook some strong spirit of salt, or dephlegm'd *Aqua fortis*, the greenness of the solution would, in a trice almost, totally disappear, and the liquor, held against the light, scarce appear other than clear, or limpid ; which is therefore remarkable, because we know, that *Aqua fortis*, by corroding copper, the thing which gives the colour to verdigrease, usually reduces it to a greenish blue solution. But if into this almost colourless liquor, you drop a just quantity either of oil of tartar, or spirit of urine, you may find, that after the ebullition is ceas'd, the mixture will disclose a lively colour, tho' somewhat different from that the solution of verdigrease had originally.

42. But these experiments tending either to alter the colour of a body, or entirely to destroy it, without giving it a successor ; I had a desire also, to turn a body of one colour into two, very different both in consistence and colour, by help of one that was colourless. In order to this, finding the acidity of spirit of vinegar to be wholly destroy'd, by working upon *Minium*, I concluded, that the solution of lead, in spirit of vinegar, would alter the colour of the juices and infusions of several plants, after the manner of oil of tartar ; and, accordingly, I was quickly satisfied, upon trial, that the infusion of rose-leaves would, by a small quantity of this solution, well mix'd with it, be immediately turn'd into a saddish green.

One body changed into more, of different colours, by a colourless ingredient.

And further, I had often found, that oil of vitriol will precipitate many bodies, both mineral and others, dissolv'd not only in *Aqua fortis*, but particularly in spirit of vinegar. I had also found, that the powders precipitated by this liquor, were usually fair and white. Laying these things together, 'twas not difficult to conclude, that if upon a tincture of red rose-leaves, made with fair water, I dropp'd a quantity of a strong solution of *Minium*, the liquor would be turn'd into the like muddy green substance, whereto oil of tartar would reduce it ; and that a convenient proportion of oil of vitriol, would have two distinct operations upon the mixture ; the one, to precipitate the dissolv'd lead in the form of a white powder ; the other, to clarify the muddy mixture, and both restore, and exceedingly heighten the redness of the infusion of roses :

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and making the experiment accordingly, in a conical glass, that the subsiding powder might seem to possess the more space, and be the more conspicuous; I found, upon shaking the green mixture, that the colour'd liquor might be the more equally dispers'd, a few drops of the rectified oil of vitriol, presently turn'd the opaque liquor into one that was clear and red, almost like a ruby; and threw down a large quantity of a powder, which, when settled, would have appeared very white, if some interspers'd particles of the red liquor had not a little allay'd the purity, tho' it did not blemish the beauty of the colour.

And to show that these effects flow from the oil of vitriol, as it is a strong acid menstruum, that has the property both to precipitate lead, as well as some other substances, out of spirit of vinegar, and to heighten the colour of red rose-leaves; I have done the same thing, tho', perhaps, not quite so well, with spirit of salt, but could not do it with *Aqua fortis*; because tho' that potent menstruum, as well as the others, heightens the redness of roses, yet it would not, like them, precipitate lead out of spirit of vinegar, but rather dissolve it. And as, by this way, we have produc'd a red liquor, and a white precipitate, out of a dirty green magistery of rose-leaves; so, by the same method, we may produce a fair yellow, and sometimes a red liquor, and the like precipitate out of an infusion of a curious purple colour. For, I lately intimated, that I had, with a few drops of an alkali, turn'd the infusion of log-wood into a lovely purple; but if, instead of that alkali, I substituted a very strong, and well-filtr'd solution of *Minium*, made with spirit of vinegar; and took about half as much of this liquor as there was of the infusion of log-wood; a convenient proportion of spirit of salt, would, if the liquors were briskly shook together, presently strike down a precipitate like the former, and turn the liquor that swam above it, for the most part, into a lovely yellow.

But to advance this experiment a little further, I consider'd, that in case I first turn'd a spoonful of the infusion of log-wood purple, by a convenient proportion of the solution of *Minium*; the affusion of spirit of sal-armoniac, would precipitate the corpuscles of lead, conceal'd in the solution of *Minium*, and yet not destroy the purple colour of the liquor; whereupon I thus proceeded. I took about a spoonful of the fresh tincture of log-wood, (for if it be stale, the experiment will not always succeed) and having put to it a convenient proportion of the solution of *Minium*, to turn it into a deep and almost opaque purple; I then dropp'd in as much spirit of sal-armoniac as I guess'd would precipitate about half the lead; and immediately stirring the mixture well together, I mix'd the precipitated parts with the others, so that they fell to the bottom, partly in the form of a powder, and partly in the form of a curdled substance, that retain'd, as well as the upper liquor, a bluish purple colour, sufficiently deep; and then instantly pouring in a pretty quantity of spirit of salt, the matter first precipitated, was, by the conical figure of the glass, preserv'd from the spirituous salt, which suddenly precipitated upon it a new bed of white powder, or the remaining corpuscles of the lead, that the urinous spirit had not

not struck down ; so that there appear'd in the glass, three distinct and very differently colour'd substances ; a purple, or violet-colour'd precipitate at the bottom ; a white and carnation precipitate over that ; and, at the top of all, a transparent liquor, of a lovely yellow, or red.

Thus, tho' to some, I may seem to have hit on this, and the like experiments, by chance ; whilst others imagine they proceed from some extraordinary insight into the nature of colours ; yet, indeed, the contrivance of them need not to be look'd upon as any great matter, in one who is a little vers'd in my notions about the differences of salts.

43. That the colour of a body may be chang'd by a liquor, which, of it self, is colourless, provided it be saline, we have already shewn by a multitude of instances ; and the thing does not appear strange, because saline particles swimming up and down in liquors, have been often observ'd to act powerfully in the production and change of colours : but it has seem'd surprizing to many, unacquainted with chymical operations, that a white dry body should immediately acquire a new rich colour, upon the bare affusion of clear spring water. And yet the way of producing such a change of colours, may be easily hit on by those who often make solutions of mercury. For we have try'd, that tho' by evaporating a solution of quick-silver in *Aqua fortis*, till the remaining matter began to be tolerably dry, fair water, pour'd on the remaining calx, turn'd it but a little yellowish ; yet, when we took good quick-silver, and three or four times its weight of oil of vitriol, and in a glass retort drew off the saline menstruum from the metalline liquor, till there remain'd a dry snow-white calx at the bottom ; upon pouring on it a large quantity of fair water, we did, almost in a moment, perceive it to pass to one of the loveliest light yellows that ever we beheld. And the turbith mineral is of a colour not much inferiour to this ; tho' it be often made with a different proportion of the ingredients, and after a more troublesome manner. But this colour, tho' so exquisitely fine, and so greatly wanted by painters, is, I fear, too costly to be employ'd by them, unless about curious pieces ; tho' I do not know how well it will agree with every pigment, especially with oil-colours.

Changes of colours produced in a dry, white body, by spring-water.

And whether this experiment be really of another nature, than those wherein saline liquors are employ'd, may be so plausibly doubted, that whether the water pour'd on the calx, do barely, upon imbibing some of its saline parts, alter its colour, changing its texture ; or whether by dissolving the coagulated salts, it becomes a saline menstruum, and, as such, operates upon the mercury, I leave to be consider'd. I have, however, several times, with fair water, wash'd from this calx abundance of strongly-tasted corpuscles, which, by the abstraction of the menstruum, I could reduce into salt.

But, to shew how much a real and permanent colour may be call'd out by a liquor that has neither colour, nor so much as saline, or other active parts ; provided it can but bring the parts of the body it imbibes, to convene into clusters, dispos'd after a particular manner, we made the following experiment.

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A permanent colour produced by a particular arrangement of parts.

44. We put some powder of good common vitriol into a crucible, and kept it melted in a gentle heat, till, by the evaporation of some parts, and the transposition of the rest, it had quite lost its former colour; what remain'd we took out, and found it to be a friable calx, of a dirty grey colour: on this we pour'd fair water, which it did not tinge either green or blue, but only seem'd to make a muddy mixture with it; then stopping the vial wherein the ingredients were, we let it stand in a quiet place for some days. The water having now dissolv'd a large part of the imperfectly calcin'd body; the vitriolic corpuscles swimming to and fro in the liquor, had time, by their occurrences, to constitute many little masses of vitriol, which gave the water they impregnated, a fair vitriolic colour; and this liquor being pour'd off, the remaining dirty powder, in time, communicated the like colour, but not so deep, to a second parcel of clear water, that we pour'd on it.

Various colours produced in different parts of the same liquor.

45. It may contribute to shew how much some colours depend upon the less or greater mixture, and contemperation of light with shades, to observe how the number of particles of the same colour, either receiv'd into the pores of a liquor, or swimming up and down in it, may seem greatly to vary the colour of it. I could produce several instances of solid bodies, wherein, if the colour be not a light one, as white, yellow, or the like, the closeness of parts in the pigment, makes it look blackish; tho' when laid on but thinly, it will, perhaps, appear blue, green, or red: but I shall rather insist upon liquors, than dry bodies.

If, then, you put a little fair water into a clear slender vial, and let fall into it a few drops of a strong decoction, or infusion, of cochineal, or of brazil, the ting'd drops will descend like little clouds into the liquor, thro' which, if by shaking the vial, you diffuse them, they will turn the water of a pink colour; by dropping in a little more of the decoction, you may heighten the colour into a fine red, almost like that of rubies; and by continuing the affusion, the liquor may be brought to a kind of crimson, and afterwards to a dark opaque redness. And whilst the liquor passes from one of these colours to the other, you may observe many less noted colours, bordering upon red, to which it is not easy to assign names; especially considering how much the proportion of the decoction to the fair water, and the strength of that decoction, may, with other circumstances, vary the phenomena of this experiment. But, to make it with the greater conveniency, we use, instead of a vial, a slender pipe of glass, of about a foot in length, and about the thickness of a man's little finger: for, if leaving one end of this tube open, you seal up the other hermetically, or otherwise exactly stop it, you have a glass, wherein may be observ'd, the variations of the colours of liquors, much better than in large vials; and wherein experiments of this nature, may be made with very small quantities of liquor. And in this pipe, may be produc'd various colours, in the various parts of the liquor, and be kept long swimming upon one another, unmix'd.

It has excited the admiration of some persons to see what a variety of colours we have sometimes produced in such glasses, by the bare infusion of brazil, variously diluted with fair water, and altered by the infusion of several chymical spirits, and other saline colourless liquors; and when the whole mixture is reduced to an uniform degree of colour, I have made it appear to be of colours gradually differing, by pouring it into glasses of a conical figure. Or even take a large round vial, fill it with the red infusion of brazil, hold it against the light, and you will discern a notable difference betwixt the colour of that part of the liquor which is in the body of the vial, and that which is more pervious to the light, in the neck.

I once had a glass, and a blue liquor, which was chiefly a certain solution of verdigrease, so fitted, that tho' in other glasses the experiment would not succeed; yet when this particular glass was filled with that solution, it appeared in the body of the vial of a lovely blue, and in the neck of a manifest green. I had also, a broad piece of glass, which being viewed against the light, seem'd clear enough; and, held from the light, appeared very little discoloured: yet it was a piece knock'd off from a great lump of glass, to which if we rejoin'd it, where it had been broken off, the whole mass appeared green as grass.

I have, likewise, several times used bottles and stopples, both made of the very same metal; and yet whilst the bottle appeared only inclining to green, the stopple was of so deep a colour, that it could hardly be thought possible they should be the same materials. And I have by me a flat glass, on which, if I look against the light with the broad side obverted to my eye, it appears like a good ordinary window-glass; but if I turn the edge of it to my eye, and stand in a convenient position, with regard to the light, it emulates an emerald.

I have sometimes made a fluid kind of pigment, which, dropped on a piece of white paper, appears, where any quantity of it falls, of a crimson colour; but, being spread thinly on the paper, presently exhibits a fair green.

Let me add, that having made many experiments with that blue substance, called by the painters litmase; we have sometimes observed, that being dissolved in a due proportion of fair water, the solution, either opposed to the light, or dropped upon white paper, appeared of a deep colour, betwixt crimson and purple; yet, when spread very thin on the paper, and suffered to dry there, the paper was thereby stain'd of a fine blue. This experiment also succeeded, when made on a flat piece of pure white glazed earth. And having let fall a few drops of the strong infusion of this litmase, in fair water, into a fine crystal glass, shaped like an inverted cone, and almost fill'd with clear water; I had the pleasure to see these few tinged drops, variously dispersing themselves thro' the limpid water, exhibit many colours, or varieties of purple and crimson. But when the corpuscles of the pigment seem'd to have equally diffused themselves thro' the whole; by adding to it two or three drops of spirit of salt, we perceived it first made an odd change in the colour of the liquor,

PHYSICS. as well as a visible commotion among its small parts, and in a short time changed it wholly into a very glorious yellow, like that of a topaz. After this, if I let fall a few drops of a strong heavy solution of pot-ashes, whose weight would quickly sink it to the sharp bottom of the glass; there would soon appear four very pleasant and distinct colours: *viz.* a faint bright one, at the sharpest part of the glass; a purple, a little higher; a deep and glorious crimson, in the confines betwixt the purple and the yellow; and an excellent yellow, the same that before adorned the whole liquor, reaching from thence to the top of the glass. And if I poured, very gently, a little spirit of sal-armoniac upon the upper part of this yellow, there would also arise there a purple, or a crimson, or both; so that the unaltered part of the yellow liquor appeared intercepted betwixt the two neighbouring colours. Hence we need not be surprized at the tricks of those mountebanks, who are commonly called water-drinkers. For tho' not only the vulgar, but many persons far above that rank, have wondered to see a man, after drinking large quantities of fair water, return it in the form of claret, sack, and milk; yet having by chance had occasion to oblige a wanderer, who made a profession of this, and other juggling tricks, he ingenuously confessed to me, that the art consisted rather in a few tricks than any great skill in altering the nature and colours of things. And I suspect there may be a great deal of truth in a little pamphlet, printed long ago in *English*; wherein the author undertakes to discover, from the confession of some of the accomplices themselves, that a famous water-drinker, then much admired in *England*, performed his pretended transmutations of liquors by the help of two or three inconsiderable preparations and mixtures of obvious fluids; and chiefly of an infusion of brazil variously diluted and made pale, yellowish, &c. with vinegar. And, for my part, what most surprizes me in this affair, is, that the drinkers can take down so much water, and spout it out again with violence; tho' custom, and a vomit seasonably taken before-hand, may in some of them greatly facilitate the work. But as for the changes they make in liquors, those are but few and slight.

Changes of colour may greatly depend upon the peculiar texture of the men-
struum.

46. *Helmont* used to make a preparation of steel, which a very ingenious chymist sometimes employs for a *Succedaneum* to the spaw-waters; diluting this *Essentia Martis liquida*, as he calls it, with a due proportion of water. And tho' this preparation be almost of the colour of a *German* amethyst, and consequently remote from green; yet a very few drops of it being let fall into a large proportion of *Rhenish*, or white-wine, it immediately turns them to a lovely green. By which phenomenon we may learn how requisite it is in experiments, about the changes of colours, carefully to regard the circumstances of them; for water will not, as I have purposefully try'd, concur to the production of any such green; nor did it give that colour to moderate spirit of wine, wherein I dissolved it: and wine itself is a liquor that few would suspect able, of a sudden, to work any such change in a metalline preparation of this nature. And to satisfy myself that this new colour proceeds rather from the peculiar texture of the

the wine, than from any greater acidity that *Rhenish*, or white-wine has, in comparifon of water; I sharpened the folution of this effence in fair water, with a large quantity of fpirit of falt; and then the mixture acquired no greennefs.

To vary the experiment a little, I try'd, that if into fome *Rhenish* wine made green by this effence, I dropped an alkaline folution, or urinous fpirit; the wine would prefently grow turbid, and of an odd dirty colour. But if, inftead of diffolving the effence in wine, I diffolved it in fair water, sharpen'd with a little fpirit of falt; then either the urinous fpirit of fal-armoniac, or the folution of the fixed falt of pot-afhes, would immediately turn it of a yellowifh colour; the fixed or urinous falt precipitating the vitriolic fubftance contained in the effence. And as our effence imparts a greennefs to wine, but not to water; *Olaus Wormius* tells us of a rare kind of turn-fol he had, whose beautiful rednefs would be eafily communicated to water, but fcarce to wine, and not at all to fpirit of wine: in which laft circumftance it agrees with our effence, tho' they difagree in other particulars.

47. I have often taken notice, that metals, as they appear to the eye, before they come to be altered by other bodies, exhibit colours very different from thofe which the fire, or a menftruum, either feparately or conjointly, produce in them; efpecially confidering, that thefe metalline bodies are, after all their difguizes, reducible not only to their former confiftence, and other more effential properties, but to their colour too; as if nature had given them an external and an internal colour. But, upon a more attentive confideration of this difference of colours, it feems probable to me, that many of thofe we call internal, are rather produced by the coalition of metalline particles with thofe of the falts, or other bodies employ'd to work on them, than by the bare alteration of the parts of the metals themfelves. Of thefe adventitious colours of metalline bodies, the chief forts feem to be three; viz. fuch as are produced by the fole action of the fire; fuch as emerge from the coalition of metalline particles, with thofe of fome menftruum employ'd to corrode, or precipitate the metal; and, laftly, the colours afforded by metalline bodies, either melted with, or otherwife penetrating into others, efpecially fuch as are fufible.

The different colours of metals in different ftates.

As to the firft of thefe colours; 'tis well known to chymifts, that tin calcined by fire alone, affords a white calx; and lead, by the fame means, yields that common red powder we call *Minium*; copper alfo, calcined *per fe*, by a long or violent fire, gives a very dark or blackifh powder; iron, likewise, may, by the action of reverberated flames, be turn'd into a colour almoft like that of faffron, as we fee in the preparation of *Crocus Martis per fe*; and mercury, by the power of fire, will be turn'd into a red powder, call'd precipitate *per fe*.

Befides, the fame metal may, by the fucceffive operations of the fire, receive feveral adventitious colours; as is evident in lead, which, before it arrives at fo deep a colour as that of *Minium*, may pafs thro' feveral others.

Not

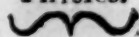
PHYSICS.

And, not only the *Calces*, but the glasses of metals, vitrify'd *per se*, may have colours different from the obvious or natural colours of the metal; as I have observed in the glass of lead, made by long exposing that metal crude to a violent fire. I have likewise seen a piece of very dark glass, which an ingenious artificer, who shewed it me, professed he made of silver alone, by an extreme violence of the fire.

Minerals also, by the action of the fire, may be brought to afford colours very different from their own; as was observed about the variously coloured flowers of antimony. To which we may add, the whitish grey colour of its calx, and the yellow or reddish one of the glass, into which that calx may be fluxed. And vitriol, calcined with a very gentle heat, and afterwards with higher degrees of it, may be made to pass thro' several colours, before it descends to a dark purplish one, whereto a strong fire will at length reduce it.

48. The adventitious colours produc'd in metals, by saline liquors, are many of them, well known to chymists. That gold, dissolv'd in *Aqua regia*, communicates its own colour to the menstruum, is a common observation; but the solutions of mercury, in *Aqua fortis*, are not generally observ'd to give any notable tincture to the menstruum; tho' sometimes, when the liquor first falls upon the quick-silver, I have observ'd a very remarkable greenness, or blueness to be produc'd. Tin, corroded by *Aqua fortis*, till the menstruum will work no farther on it, becomes exceeding white; and easily, of it self, acquires the consistence not of a metalline calx, but of a coagulated matter, so like either to curdled milk, or curdled whites of eggs, that a person unacquainted with such solutions, might easily be mistaken in it. But when I purposely prepar'd a menstruum that wou'd dissolve it, as *Aqua fortis* dissolves silver, not barely corrode it, and quickly let it fall again; I remember no particular colour in the solution: as if the more whitish metals did not much tinge their menstrea, tho' the high-colour'd ones, as gold and copper, do. For lead dissolv'd in spirit of vinegar, or *Aqua fortis*, gives a clear solution: and, if the menstruum be abstracted, appears either diaphanous, or white. And 'tis worth noting, that tho' when iron is dissolv'd in oil of vitriol, diluted with water, it affords a salt, or magistery, so like in colour, as well as some other qualities, to green vitriol, that chymists properly call it, *Vitriolum Martis*; yet, by changing the menstruum, and pouring upon the filings of steel, *Aqua fortis*, instead of oil of vitriol, I obtain'd not a green, but a saffron-colour'd solution, or rather a thick liquor, of a deep yellowish red. Common silver, dissolv'd in *Aqua fortis*, yields a solution ting'd like that of copper; which is not to be wonder'd at, because, in coining silver, they give it an allay of copper; and what is sold for refin'd silver, is not so perfectly free from that ignobler metal, but that a solution of it in *Aqua fortis* will give its tincture to the menstruum. But we could not observe, upon the solution of some silver perfectly refin'd, that the menstruum, tho' held against the light, in a crystal vial, manifestly disclos'd any tincture; only it sometimes seem'd not quite destitute of a very faint bluish cast.

But



But, of all the metals, there is not one which so easily and constantly discloses its colour as copper. For, in acid menstrua, as *Aqua fortis*, and spirit of vinegar, it not only gives a bluish green solution; but if it be almost any way corroded, it appears of one of those two colours. And so constant is the disposition of copper, notwithstanding the disguise artists put upon it, to discover its colour, that we have, by forcing it up with sal-armoniac, obtain'd a sublimate of a bluish cast. Nay, a famous chymist affirms, that the very mercury of it is green; but till he teaches us an intelligible way of making such a mercury, we must content ourselves to say, that we have had a cupreous body precipitated out of a distill'd liquor, which seem'd to be the sulphur of that metal, and even when flaming, appear'd of a greenish colour. And, indeed, copper is a metal so easily wrought upon, by liquors of several kinds, that, I might say, I know not any mineral which concurs to the production of such a variety of colours, as copper dissolv'd in several menstrua, viz. spirit of vinegar, *Aqua fortis*, *Aqua regia*, spirit of nitre, of urine, of foot, oils of several kinds, &c. if the variety of them were not comprehended within the limits of greenish blue, or bluish green.

But being desirous to try if I could not with crude copper make a green solution, without the bluishness that usually accompanies it, I concluded upon two menstrua, which, tho' I had not known employ'd to work on this metal, prov'd successful; the one was spirit of sugar, and the other, oil, or spirit of turpentine, which affords a fine green solution, useful on several occasions. And yet to shew that the adventitious colour may result as well from the true and permanent copper it self, as the salts wherewith 'tis corroded; if you take a piece of good *Dantzick* copperas, or any other vitriol wherein copper is predominant, and having moisten'd it with fair water, rub it upon a bright piece of iron, or steel; it will (as we have formerly said) presently stain it with a reddish colour, like that of copper.

We have sometimes try'd what colours such minerals as tin-glass, antimony, spelter, &c. would yield in several menstrua. The like we have also done with stones; among which, that famous one call'd by *Helmont*, *Paracelsus's Ludus*, has afforded in menstrua able to dissolve so solid a stone, sometimes a yellowish, and sometimes a red solution. And from minerals I have obtain'd, with several menstrua, very different colours; and some such, as, perhaps, would scarce be expected from such bodies.

The colours of metals may, in many cases, be further alter'd, by employing either precipitating salts, or other convenient substances, to act upon their solutions. If quick-silver be dissolv'd in *Aqua fortis*, and precipitated out of the solution, either by water impregnated with sea-salt, or the spirit of that concrete, it falls to the bottom in the form of a white powder; but if precipitated with an alkali, it affords a yellowish powder: and if no precipitation be made, and the menstruum be drawn off with a convenient fire, the corroded mercury will remain at the bottom, and may be made to appear of different colours, by different degrees of heat.

PHYSICS.

Thus having abstracted *Aqua fortis* from some quick-silver that we had dissolv'd in it, till there remain'd a white calx: by exposing that to several degrees of fire, and afterwards to a naked one, we obtain'd some new colours; and at length, the greatest part of the calx lying at the bottom of the vial, and being brought partly to a deep yellow, and partly to a red colour, the rest appear'd elevated to the top and neck of the vial; some in the form of a reddish, and some of an ash-colour'd sublimate. And even the succedaneum to a menstruum, may sometimes serve to change the colours of a metal. The lovely red, which painters call vermilion, is made of mercury, which appears of the colour of silver; and of brimstone, which resembles that of gold, sublim'd up together in a certain proportion.

49. The third of the principal kinds of adventitious colours in metals, is that produc'd by associating them, especially when calcin'd with other fusible bodies, and especially *Venice*, and other fine colourless sorts of glass.

I formerly gave an example of a metal, imparting a colour to glass, very different from its own, in shewing how silver turns it to a lovely golden colour; I shall now add, that tho' I learn'd from one of the chief artificers in painted glass, that those of his trade colour it yellow, with a preparation of the calx of silver; yet, having mix'd a few grains of shell-silver, such as is employ'd with the pencil and pen, with a convenient proportion of powder'd crystal-glass, and kept them for two or three hours in fusion, I was surpriz'd to find the melted mass appear, upon breaking the crucible, of a lovely sapphirine blue: which made me suspect my servant might have brought me a wrong crucible; but he constantly affirm'd it to be the same wherein the silver was put; and considerable circumstances countenanc'd his assertion: whence, till farther trial, I suspect either that silver, which is not very probable, brought to a perfect fusion with glass, may impart other colours thereto, than when neal'd upon it; or else, which is less unlikely, that tho' silver-beaters usually chuse the finest coin they can get, as that which spreads most under the hammer; yet the leaf-silver, whereof this shell-silver was made, might retain so much copper, as to give the predominant tincture to the glass.

Take it, as another instance of the adventitious colours of metals, that tho' copper, calcin'd *per se*, affords but a dark and basely colour'd calx, yet the glass-men tinge their glass green therewith. And, I remember, when taking some crude copper, and by frequent ignition, and quenching it in water, till we had reduced it to a darkish-colour'd powder; and afterwards keeping it in fusion in about a hundred times its weight of fine glass, we obtain'd a blue-colour'd mass, which wou'd, perhaps, have been green, if we had hit upon the right proportion of the materials, the degree of fire, and the time wherein it ought to be kept in fusion. But tho' copper thus gives somewhat near the like colour to glass, that it communicates to *Aqua fortis*; yet it seems worth inquiring, whether those new colours, which mineral bodies disclose in melted glass, proceed from the

the coalition of the corpuscles of the mineral with the particles of the glass, or from the action of the alkaline salt (a principal ingredient of glass) upon the mineral body; from the concurrence of both these causes, or from any other.

We may observe, that putty, made by calcining together a proportion of tin and lead, as it is itself a white calx; so it turns the matter of the purer sort of glass, wherewith it is melted, into a white mass; which, if it prove opaque enough, is employ'd for white enamel.

When the materials of glass, melted with calcin'd tin, have composed an opaque white mass; 'tis made the basis of all those fine concretes the goldsmiths, and several artificers employ in the curious art of enamelling: for this white fusible substance receives and preserves the colours of many other mineral substances, which, like it self, will endure the fire.

And, as it appears, that several minerals will impart to fusible masses, colours different from their own; so, by the making and compounding of enamels, it is manifest, that many bodies will both retain their colour in the fire, and impart the same to some others wherewith they were vitrify'd. Thus we formerly saw, that a blue and yellow composed a green enamel. And 'tis pretty to see, that some colours are of so fixed a nature, as to be capable of mixture, without receiving any damage from the fire; and that mineral pigments may be mix'd by it, almost as regularly and successfully as the vulgar colours, by the help of water, in the vats of dyers.

Both metalline, and other mineral bodies, may be employ'd to give tinctures to glass; and 'tis worth observing, how small a quantity of some mineral substances will tinge a comparatively vast proportion of it. We have sometimes attempted to colour glass even with precious stones; and had no cause to think the experiment ill bestow'd. 'Tis known that the artificers in glass tinge their metal blue, with that dark mineral, zaffra, which some would have to be a mineral earth; others, a stone; and others, again, neither; but it is confessedly of a dark, not a blue colour. 'Tis, likewise, a remarkable practice among them, that they employ manganese to exhibit in glass, not only other colours than its own, (wherein it is so like to the load-stone, that it is given by mineralists for one reason of its *Latin* name *Magnesia*) but colours different from one another: for tho' they use it to clarify their glass, and free it from that bluish greenish colour, which it wou'd otherwise be subject to; yet they also employ it in certain proportions, to tinge their metal, both with a red colour, and with a purplish, or murrey; and, by putting in a greater quantity, they also make with it that deep obscure glass, which passes for black. And this agrees very well with what we formerly observ'd of the apparent blackness of those bodies that are over-charg'd with the corpuscles of such pigments, as are red, blue, green, &c.

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An easy method
of examining
ores.

And as by several metals, and other minerals, we can give various colours to glass; so, on the other hand, from the different colours that mineral ores, or other mineral powders, by being melted with glass, disclose therein, a probable conjecture may be often made of the metal, or known mineral, that a propos'd ore either contains, or is nearest allied to. And this easy method of examining ores, may, in some cases, prove very serviceable; but great circumspection is requisite to keep it from proving fallacious, upon account of the variations of colour, producible by the different proportions that may happen, betwixt the ore and the glass; by the richness and poorness of the ore it self; by the degree of fire; and especially by the length of time, during which the matter is kept in fusion.

There is another way, different from those already mentioned, whereby metals may be brought to exhibit adventitious colours. And, in this case, the metal does not so much impart a colour to another body, as receive one from it; or rather, both bodies, by the new texture resulting from their mixture, produce a new colour. I will not here insist upon the examples afforded us by yellow orpiment, and common sea-salt; from which, sublimed together, chymists unanimously affirm their white or crystalline arsenic to be made: but 'tis worth noting, that tho' yellow orpiment be acknowledged by far the most predominant of the two ingredients of arsenic; yet arsenic, duly added to the highest coloured metal, copper, when in fusion, gives it a whiteness both within and without. Thus, also, *Lapis Calaminaris* changes and improves the colour of copper, by turning it into brass. And I have sometimes, by the help of zinc, duly mixed after a certain manner, given to copper as rich a golden colour as ever I saw in the best true gold.

The way of making counterfeit
gems.

Upon knowing the different methods of producing the adventitious colours of metals and minerals in bodies capable of vitrification, depends the pretty art of making counterfeit gems: for whilst pure sand, or calcined crystal, gives the body in their preparation; 'tis, for the most part, some metalline or mineral calx, mixed in a small proportion with it, that gives the colour. Calcined lead, fused with fine white sand, or crystal, reduced by ignitions and subsequent extinctions in water, to a subtile powder, will, of itself, be brought, by a due decoction, to give a clear mass, coloured like a *German* amethyst. But this colour may be easily over-powered by those of several other mineral pigments, so that with a glass of lead you may emulate the fresh and lovely green of an emerald; tho', in many cases, the colour which the lead itself, upon vitrification, tends to, may vitiate that of the pigment design'd to appear in the mass. These colours also depend so much upon the texture of the materials, that we have made the glass of lead itself, composed of about three parts of litharge, or *Minium*, fused with one of crystal, or sand, very finely powdered, pass thro' different colours, according as we kept it more or less in fusion. But the degrees of coction, and other circumstances, may so vary the colour produced, that, in a small crucible, I have had fragments

ments of the same mass; in some of which, perhaps, not so big as a hazelnut, four distinct colours might be discerned.

Besides the three mentioned sorts of adventitious colours in metals, there may be others reducible under the same head; of which I shall instance only in two.

The first is afforded us from the practice of scarlet-dyers. A most famous master in this art assured me, that neither he, nor others, can strike the lovely colour, called the bow-dye, unless their materials be boiled in vessels of a particular metal. Secondly, metals will afford uncommon colours, by imbuing several bodies with solutions of them, made in proper menstrua. Thus, tho' copper, plentifully dissolved in *Aqua fortis*, will communicate to several bodies the colour of that solution; yet some other metals will not; as I have often try'd. Gold, dissolved in *Aqua regia*, dyes the nails and skin, the hafts of knives, and other things made of ivory, not with a yellow, but a purple colour; which, tho' it manifest itself but slowly, is very durable, and can scarce ever be washed out. Thus we formerly said, that the purer crystals of fine silver made with *Aqua fortis*, tho' they appear white, will presently dye the skin and nails of a black, not to be washed off like ordinary ink. And many other bodies may, in the same manner, be dy'd, some of a black, and others of a blackish colour.

Mineral solutions, also, may produce colours different from those of the liquors themselves. I shall not fetch an example of this from what happens in the salting of beef, which often appears green, and sometimes of a reddish colour, when boil'd; nor shall I insist on the practice of giving, by salt-peter, and a certain proportion of common salt, a fine redness to such flesh as would otherwise appear purely white: I rather chuse to say, that I have several times found a solution of the sulphur of vitriol, or even of common sulphur, tho' the liquor appeared clear, immediately to tinge a piece of new coin, or other clean silver, sometimes with a golden, sometimes with a deeper, and more reddish colour; according to the strength of the solution, and the quantity of it, that chanced to adhere to the metal. This renders it the less surprizing, that the water of the hot spring at *Bath*, abounding with dissolved substances of a very sulphureous nature, should, for a while, tinge such pieces of coin yellow, as are for a due time immersed in it. And even vegetable liquors, whether by degeneration, or by altering the texture of the body that imbibes them, may stain other substances with colours very different from their own. 'Tis affirmed, that the green juice of *Alcanna* dyes the skin and nails of a lasting red.

50. Meeting, in an *Italian* author, with a way of preparing what he calls a *Lacca* of vegetables, by which the *Italians* mean a kind of extract fit for painting, like that rich *Lacca*, in *English* commonly call'd lac, employ'd by painters as a glorious red; and finding the experiment considerable, but very imperfectly related, we improved upon it in the following manner:

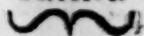
The method of preparing a yellow vegetable lac.

PHYSICS. manner: tho' the thing, as there delivered, is, by the *Italians*, esteem'd a great secret.

Put what quantity of powdered turmeric you please into fair water; adding to every pound of that liquor a large spoonful of a very strong lixivium of pot-ashes, clarify'd by filtration; let all this simmer over a soft fire in a clean glazed earthen vessel, till you find, by the immersion of a sheet of white paper, that the liquor is sufficiently impregnated with the golden tincture of the turmeric: then take the decoction off the fire, and filtre or strain it; and afterwards, leisurely dropping into it a strong solution of roch-alum, the decoction will, as it were, be curdled; and the tinged part of it either emerge, subside, or swim up and down in little yellow flakes: when, if you pour this mixture into a funnel, lined with cap-paper, the liquor that formerly filter'd yellow, will now pass clear, and leave its tinged parts behind in the filtre; into which fair water must be so often poured, till the matter therein contain'd be dulcify'd; that is, till the water passes thro' it, as tasteless as when it was poured on. But if, without filtration, you would obtain the flakes of this vegetable lac, pour a large quantity of fair water upon the decoction, after the affusion of the aluminous solution; and you shall find the liquor to grow clearer, and the lac to settle together at the bottom, or emerge to the top of the water: tho', sometimes, having not employ'd a sufficient quantity of fair water, the lac has partly subsided, and partly emerged, leaving all the liquor clear in the middle. But, to make this lac fit for use, it must, by repeated affusions of fresh water, be dulcify'd from the adhering salts, as well as that separated by the filtre; and be spread to dry leisurely upon pieces of cloth, with brown paper, chalk, or bricks under them, to imbibe the moisture.

Alum, being a stony matter dissolved by acid, may, when used as a precipitate, be, itself, precipitated.

'Tis supposed, that the magistery of vegetables, obtain'd by this means, consists only of the more soluble and coloured parts of the respective plant that affords it. But I must take the liberty to question the supposition; for, according to my notion of salts, alum, tho' to sense a homogeneous body, ought not to be reckoned among true salts, but is to be look'd upon as a kind of magistery; since, as native vitriol contains both a saline substance, and a metal corroded and associated with it; so alum, (which is so near allied to vitriol, that, in some places of *England*, the same stone will sometimes afford both) seems manifestly to contain a peculiar kind of acid spirit generated in the bowels of the earth, and a kind of stony matter dissolved by it. In making our ordinary alum, 'tis true, the workmen use the ashes of a sea-weed, vulgarly called kelp, and urine; yet I am inform'd, that here in *England*, there is, besides the factitious alum, another sort made by nature, without the help of those additions. Now, considering this composition of alum, and that alkaline salts precipitate what acid ones have dissolved; I could not but suspect that the curdled matter, call'd the magistery of vegetables, may have in it a considerable proportion of a stony substance, precipitated out of the alum by the lixivium



vium wherein the vegetable had been boiled. And, to shew there is no necessity that all the curdled substance must belong to the vegetable; I took a strong solution of alum, and having filtered it; by pouring in a convenient quantity of a strong solution of pot-ashes, I presently turn'd the mixture into a white curdled matter; which, committed to the filtre, left a great quantity of a very white stony calx, that seem'd to be of a mineral nature, as well from other signs, as that little bits of it being put upon a live coal, they did neither melt, nor fly away, whilst that was blown upon; and a quantity of this white substance, being for a long time kept in a red-hot crucible, was neither diminished nor spoiled: hot water too, wherein I kept another parcel of such calx, seem'd only to wash away the looser salts from the stony substance. And I have, by gentle exhalation, recovered from the liquor that pass'd thro' the filtre, and left this calx behind, a body, in appearance like salt; for 'twas very white, and consisted of innumerable exceeding slender shining particles, which would, in part, easily melt at the flame of a candle, and, in part, fly away with some little noise. I have likewise, with urinous salts, such as the spirit of sal-armoniac, as well as with the spirit of urine, and even with stale urine undistilled, easily precipitated such a white calx out of a limpid solution of alum. Upon the whole, circumspection is required in judging of the nature of aluminous liquors by precipitation; otherwise, we may sometimes imagine that to be precipitated out of a liquor by alum, which is rather precipitated out of alum by the liquor.

This method of making lacs we have also practised with madder, which yielded us a red lac; and with rue, which afforded an extract nearly of the same colour with that of its leaves. But, because 'tis here, principally, the alkaline salt of the pot-ashes, which enables the water so powerfully to extract the tincture of the vegetables; I fear the decoction will not always be of the very same colour with the vegetable it is made of. For lixivious salts, tho', by penetrating and opening the bodies of vegetables, they prepare and dispose them to part with their tincture readily; yet they not only draw out some tinctures, but likewise alter them: as will easily appear from several of the experiments already delivered. And tho' alum be of an acid nature, and may, in some cases, destroy the adventitious colours produced by the alkali, and restore the former; yet we have produced examples, that, in many cases, an acid will not restore a vegetable substance to the colour destroy'd by an alkaline salt; but make it assume a third, very different from both. Besides, I have, after this manner, made magisteries of brazil, of cochineal, and of other things, red, yellow, or green; that appeared, some of them, of a rich colour, and others of no bad one: in some, however, the colour of the lac seem'd rather inferior to that of the plant; and in others again, both very different, and much worse.

But 'tis time to conclude this sketch of a history of colours.

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I shall attempt to build no theory * upon the experiments and observations here delivered ; but leave the subject to the prosecution of others. I only make it my request, that the reader would not presently conclude me mistaken in giving the matters of fact concerning the changes of colours set down, tho' he should not immediately find them exactly agreeable to his own experiments. For, besides the contingencies to which this kind of tryals is obnoxious, the omission or variation of a seemingly inconsiderable circumstance, may hinder the success of an experiment,

* From the discoveries of Sir *I. Newton* we may now settle the whole theory of colours ; which, as it regards natural bodies, is, in short, this.

First, 'tis found by experience, that the rays of light are composed of dissimilar particles ; that is, some of them are, probably, larger than others : as appears from their different refrangibility.

2. Those particles of light which are most refracted, make rays of a violet colour ; that is, the most minute particles of light, being thus separately collected, probably excite the shortest vibrations in the *Retina* ; which are thence propagated to the brain, along the solid *Capillamenta* of the optic nerves, and there excite the sense of a violet colour, the most faint and languid of all colours ; whilst the particles which are least refracted, make a red ray ; that is, the greatest particles of light make the longest vibrations in the *Retina* ; and so excite the sensation of red, the most vivid colour : the other particles, according to their several intermediate degrees of magnitude and refrangibility, exciting the intermediate colours ; almost in the same manner as the vibrations of the air, by their different magnitudes, cause different sounds.

3. The colours of these rays are not adventitious modifications of them, but original, primitive, and necessary properties, depending, probably, upon the magnitudes of their parts ; and being constant and immutable, they are unalterable by any farther refraction, reflexion, or other modification whatever.

4. As light is separated into rays of different colours by the refractions of a glass prism, and other gross bodies ; so it is differently separable by very thin plates of any transparent matter : for all such plates

that have less than a determinate thickness, suffer the rays of all colours to pass thro' them, and reflect none : but as their thickness is increased in an arithmetical proportion, they begin to reflect, first the blue rays, then the green, yellow, and pure red ; next, the blue, green, yellow, and red, more dilute and mixed ; till at length, at a certain thickness, they reflect all the rays of light intimately united together, as they fall thereon ; that is, white. But on whatever parts these thin plates reflect any colour, as blue for instance, they there transmit the contrary colour ; as, in this case, a red or yellow.

5. Now all natural bodies consist of very thin transparent plates, which being so laid together, that no reflections or refractions can be made in their interstices, the body becomes transparent : but if the distances between them are so great, or fill'd with such a matter, or are so empty of matter, that many reflexions and refractions are made within the body, this body will be opaque. And, therefore, those opaque bodies which consist of the thinnest plates, are black ; those which consist either of the thickest, or of such as differ greatly in their thickness from one another, whence consequently they become fit to reflect all colours, are white : but such as consist of plates of different intermediate thicknesses, are blue, green, yellow, or red ; because they severally reflect the rays of those colours more copiously than the other rays, which they either in great measure stifle and absorb, or sometimes transmit. And hence it is, that some liquors appear red, or yellow, by reflected light, but blue by transmitted light ; and that leaf-gold appears yellow by the former, but green by the latter, &c. See *Clark. Annals. in Robault. p. 194, 195.* & *Newton. Optic. p. 320—323, & alibi passim.*

wherein

wherein no other fault has been committed. Thus in dying scarlet, tho' you should see every ingredient that is used about it; tho' I should particularly inform you of the weight of each; and tho' you should be present at the kindling of the fire, and at the increasing and remitting of it, whenever the degree of heat is to be altered; and tho', in a word, you should see every thing done so particularly, that you would scarce harbour the least doubt of your comprehending the whole art: yet if I should not tell you, that the vessels, which immediately contain the tinging ingredients, are to be made of, or lined with tin; you would never be able, by a due observance of all the other circumstances, to bring the tincture of cochineal to give a perfect scarlet.



FREE INQUIRY

Into the Vulgar

NOTION

O F

NATURE.

SECT. I.

AS the human soul is, it self, a true and positive Being, 'tis apt to conceive all other things, as such. But this propensity, I fear, makes us think and speak of chimerical things, and of negations or privations, as of true and positive Beings.

We should, therefore, be very careful of being insensibly mis-led by such an innate and unheeded temptation to error, as we bring into the world with us. And, perhaps, among other particulars, in which this deluding propensity of our minds has too great an influence; it may have impos'd on us, in the notion we usually frame of nature. Now, this notion being the fruitful parent of others; and being so general in its applications, and important in its influence, we ought not over-easily to admit it: doubtless, it most highly deserves to be warily examin'd, before it be throughly entertain'd.

The vulgar notion of nature, prejudicial to religion and philosophy.

I have sometimes seriously question'd, whether the vulgar notion of nature, has not been both injurious to the glory of God, and a great impediment to the solid and useful discovery of his works.

For, it seems to detract from the honour of the great author and governor of the world; that men should ascribe most of the admirable things to be met with in it, not to him, but to a certain nature, which themselves do not well know what to make of. 'Tis true, many confess that this nature is a thing of his establishing, and subordinate to him; but, tho' they own it, when they are ask'd the question, yet there are several, who seldom or never regarded any higher cause. And whoever takes notice of their way of ascribing things to nature, may easily discern, that, whatever their words sometimes are, the agency of God is little in their thoughts. Doubtless, it shews the wisdom of God, to have so fram'd things at first, that there can seldom or never need any extraordinary interposition of his power; or the employing, from time to time, an intelligent overseer, to regulate, assist, and control the motions of matter.

Aristotle, by introducing the notion of the eternity of the world, in most men's opinion, at least, openly deny'd to God the production of it; and by ascribing the admirable works of the divine Being, to what he calls nature, tacitly denies him the government thereof.

But my opinion hinders me not at all from acknowledging God to be the author of the universe, and the continual preserver and upholder of it (which is much more than the peripatetic hypothesis allows) for those things which the school-philosophers ascribe to the agency of nature, interposing upon emergencies, I ascribe to the wisdom of God, in the first fabric of the universe; which he so admirably contriv'd, that, if he but continue his ordinary and general concurrence, there will be no necessity of extraordinary interpositions: so that mere matter, particularly determin'd, shall, in certain conjunctures of circumstances, do all that philosophers ascribe on such occasions, to nature.

This notion is more respectful to a divine Being, than to imagine, as we commonly do, that he has appointed an intelligent and powerful agent, as his vice-gerent, continually to watch for the good of the universe in general, and of the particular bodies that compose it; whilst this Being appears not to have the skill, or the power, to prevent such irregularities as often prove destructive to multitudes of animals, and other noble creatures, as in plagues, &c. and sometimes prejudicial to greater portions of the universe, as in earth-quakes, &c.

But farther, whilst men indulge themselves so general and easy a way of solving difficulties, as to attribute them to nature; shame will not reduce them to a more industrious search after the reasons of things; nor curiosity it self greatly move them to it. Thus the cause of the ascent of water in pumps, and other phenomena of that kind, had never been known, if the moderns had acquiesced in that imaginary one, that the world was govern'd by a watchful Being, call'd nature, who abhors a *Vacuum*; and consequently is always ready to do whatever is necessary to prevent it.

And the veneration men commonly have for what they call nature, has obstructed and confined the empire of man over the inferior creatures. For many have look'd upon it as impossible to compass, and

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others, as impious to attempt, the removing those boundaries which nature seems to have settled among her productions. And whilst they look upon her as such a venerable thing, some make a scruple of conscience to endeavour so to imitate any of her works, as to excel them.

"I will here, I doubt not, be said, that I contradict the sense of the generality of mankind: I answer, that in philosophical inquiries, we are not so solicitous about what has been, or is believ'd, as what ought to be; and certainly the sense of the generality of men, ought little to sway us in some particular questions. But 'tis no wonder men should be generally prepossess'd with such a notion of nature, as I call in question; since education has imbued them with it from their infancy: and even in their maturer years, they find it taken for granted, and employed by the most learned writers, and never hear it call'd in question. Besides, it exceedingly complies with our innate propensity, to think that we know more than we do: to vouch nature for a cause, is an expedient, that can scarce be wanting upon any occasion, to be produced as a reason for any thing we are ignorant of.

But to talk of a thing as a real and positive Being, and attribute great matters to it, weighs but little with me, when I consider, that, tho' fortune be only a certain loose and undetermin'd notion, which a modern metaphysician would refer to the class of non-entities; yet not only the *Gentiles* made it a goddess, whom many of them seriously worshipp'd, but eminent writers, both heathen and christian, ancient and modern, and all sorts of men, in their common discourse, seriously talk of it, as of a kind of antichrist, that usurped a great share in the government of the world; and ascribe little less to it, than to nature. And to pass over what poets, moralists, and divines tell us of the powers of ignorance and vice, which are but moral defects; the generality of mankind seriously attribute a great and fatal dominion to death; which, tho' said to do so many and such wonderful things, is neither a substance, nor a positive entity, but a mere privation. As for revelation, it countenances no such notion as that vulgarly receiv'd of nature: the word is not once mention'd in scripture; no, not by *Moses*, in his account of the creation. And, indeed, till the *Israelites* were overrun, and corrupted by idolatrous nations, there was, for many ages, a deep silence as to such a Being.

There are two things whereof I must advertise the reader, before I proceed farther.

1. That when, in the present discourse, I speak of the opinions of *Aristotle*, I mean those that are by the generality of scholars taken for his, or the *Aristotelian* and *Peripatetic* doctrines; by which, if he be mis-represented, the blame ought to light upon his commentators, and followers.

2. That I here set aside the consideration of the rational soul or mind of man, all other parts of the universe being, according to the receiv'd opinion, the works of nature,

A considering person may well suspect, that men have generally had **PHYSICS.** but imperfect and confused notions concerning nature; since they apply that name to several things, and those too such, as have, some of them, very little dependance on, or connexion with the others. *Aristotle* has a whole chapter expressly written to enumerate the various acceptations of the *Greek* word φύσις, commonly render'd nature; of which, he there mentions six; and, in *English*, we have more significations of that term. Sometimes, we use it,

The great ambiguity of the word nature.

1. For that author of nature, whom the school-men call *Natural naturans*; as when 'tis said, that nature hath made man partly corporeal, and partly immaterial.

2. Sometimes we mean by the nature of a thing, the essence, or that which the school-men call the quiddity of a thing; that is, the attribute which makes it what it is, whether the thing be corporeal, or not; as when we attempt to define the nature of an angel, a triangle, or a fluid.

3. Sometimes we confound that which a man has by nature, with what accrues to him by birth; as, when we say, that such a man is noble by nature.

4. Sometimes we take nature for an internal principle of motion; as when we say, that a stone let fall in the air, is, by nature, carried towards the centre of the earth; and, on the contrary, that fire, or flame, naturally moves upwards.

5. Sometimes we understand by nature, the establish'd course of things; as when we say, that nature makes the night succeed the day; or that nature hath made respiration necessary to the life of men.

6. Sometimes we take nature for an aggregate of powers belonging to a body, especially a living one; as, when physicians say, that nature is strong, weak, or spent; or that in such or such diseases, nature left to herself, will perform the cure.

7. Sometimes we take nature for the universe, or system of the corporeal works of God; as when 'tis said of a phoenix, or a chimera, that there is no such thing in nature.

8. Sometimes, too, and that most commonly, we would express by the word nature, a semi-deity, or other strange kind of Being; which is the notion we here examine.

And, besides these more absolute acceptations of the word nature, it has several others more relative. Nature is set in opposition, or contradistinction to other things; as when a stone falls downwards, we say, it does it by a natural motion; but that if it be thrown upwards, its motion that way is violent. So chymists distinguish vitriol into natural and factitious. In like manner 'tis said, that water kept suspended in a sucking-pump, is not in its natural place, as that is, which stagnates in the well. We say, also, that wicked men are still in the state of nature; but the regenerate, in a state of grace: that cures wrought by medicines, are natural operations; but the miraculous ones, wrought by Christ and his apostles, supernatural. Nor are these the only forms of speech that might be alledg'd, to manifest the ambiguity of the word nature; tho' some of these already mention'd,

PHYSICS. mention'd, should be judged so near as to be co-incident. Among *Latin* writers, the acceptations of the word nature are so many, that I remember, one author reckons up no less than fourteen or fifteen. Hence we see how easy 'tis for the generality of men, without excepting those who write of natural things, to impose upon others and themselves, in the use of a word so apt to be mis-employ'd.

I have often look'd upon it as an unhappy thing, and prejudicial both to philosophy and physic, that the word nature hath been so frequently, and yet so unskilfully employ'd, by all sorts of men. For the very great ambiguity of this term, and the promiscuous use made of it, without sufficiently attending to its different significations, render many of the expressions wherein 'tis employ'd, either unintelligible, improper, or false. I, therefore, heartily wish, that philosophers, and other leading men, would, by common consent, introduce some more significant, and less ambiguous terms and expressions, in the room of the licentious word nature; and the forms of speech that depend on it: or, at least, decline the use of it, as much as conveniently they can; and where they think they must employ it, declare in what clear and determinate sense they use it. For unless somewhat of this kind be done, men will very hardly avoid being led into mistakes, both of things, and of one another; whence such wranglings about words and names will be still kept on foot, as are usually managed with much heat, and little advantage.

But 'tis far more difficult than any one, who hath not try'd, would imagine, to discourse long of the corporeal works of God, and especially of the operations and phenomena attributed to nature, and decline the frequent use of that term, or forms of speech whereof 'tis a principal part; without frequent, and tedious circumlocutions. But to avoid, as much as possible, this inconvenience; I shall hereafter call those who maintain the vulgar notion of nature *Naturists*: an appellation I rather chuse than that of *Naturalists*, because many, even of the learned among them, are not philosophers. This inconvenience might perhaps be a little farther remedied, if,

1. Instead of the word nature, taken for *Natura naturans*, we use the term God, which 'tis put to signify.

2. If, instead of nature, used for that which makes a thing what it is, we employ the word essence; sometimes also we may make use of the word quiddity; which, tho' a barbarous term, is yet frequently employ'd, and well understood in the schools; and tho' very comprehensive, is free from ambiguity.

3. If what is meant by the word nature, taken for what belongs to a living creature at its nativity, or accrues to it by its birth, be expressed, sometimes, by saying, that an animal is born so; and, sometimes, by saying, that a thing has been generated such; and, sometimes also, that 'tis thus or thus qualified by its original temper and constitution.

4. If, instead of the word nature, taken for an internal principle of local motion; we say, sometimes, that this or that body moves as it were, or seems to move spontaneously, upwards, downwards, &c. or that it is
put

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put into this or that motion, or determined to this or that action, by the concurrence of particular causes.

5. If, instead of nature, used for the established course of things corporeal, we substitute, what it denotes, the established order, or the settled course of things.

6. If, instead of nature, taken for an aggregate of the powers belonging to a body, we employ the constitution, temper, mechanism, or the complex of the essential properties or qualities; and, sometimes, the condition, the structure, or the texture of that body: and, speaking of the greater portions of the world, we may use the terms, fabric of the world, system of the universe, &c.

7. If, where men employ the word nature for the universe, or the system of the corporeal works of God; we use the word world, or universe; and, instead of the phenomena of nature, substitute, the phenomena of the universe, or of the world.

8. And, lastly, if, instead of using the word nature, taken for either a goddess, or a kind of semi-deity; we wholly reject, or very seldom employ it.

Some, I know, will have the nature of every thing, to be only the law that it receives from the creator, and according to which it acts on all occasions. And, indeed, this opinion, tho' neither clear nor comprehensive, seems capable of a fair construction. There is often some resemblance between the orderly and regular motions of inanimate bodies, and the actions of agents, that proceed conformably to laws. And I, sometimes, scruple not to speak of the laws of motion and rest, that God has established among things corporeal, and, now and then, to call them the laws of nature.

Whether the nature of a thing be the law it receives from the creator.

But, in strictness, to say, that the nature of this or that body, is but the law of God prescribed to it, is an improper, and figurative expression. For this gives us but a very defective idea of nature, since it omits the general fabric of the world, and the contrivances of particular bodies; which yet, are as necessary as local motion itself, to the production of particular effects and phenomena: and, to speak properly, a law being but a notional rule of acting according to the declared will of a superior, 'tis plain, that nothing but an intellectual Being can be properly capable of receiving and acting by a law. For if it does not understand, it cannot know what the will of the legislator is; have any intention to accomplish it; or act with regard thereto. Now 'tis intelligible, that God should, at the beginning, impress determinate motions upon the parts of matter, and guide them, as he thought requisite, for the primordial constitution of things: and that ever since he should, by his ordinary and general concurrence, maintain those powers, which he gave the parts of matter to transmit their motion to one another. But I cannot conceive, how a body, destitute of understanding and sense, truly so called, can moderate and determine its own motions; especially so, as to make them conformable to laws, that it has no knowledge of. And that inanimate bodies,

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bodies, how strictly soever called natural, properly act by laws, cannot be proved by their acting sometimes regularly, and, as men think, in order to determinate ends: since, in artificial things, we see many motions very orderly perform'd, and with a manifest tendency to particular and design'd ends. Thus, in a watch, the motions of the spring, wheels, and other parts, are so fitted and regulated, that the hand upon the dial-plate moves with great uniformity, and seems to moderate its motion, so as not to arrive at the points that denote the time of the day, either a minute sooner, or a minute later than it should do. And when a man shoots an arrow at a mark, so as to hit it; tho' the arrow moves towards the mark, as it would, if it could, and did design to strike it; yet none will say, that this arrow moves by a law, but by an external impulse.

Aristotle's definition of nature obscure and unsatisfactory.

But, possibly, the definition of a philosopher may exempt us from the perplexities, to which, by the ambiguous expressions of common writers, we are expos'd. I therefore consider'd, with more than ordinary attention, the famous definition of nature, that is left us by *Aristotle*; which I shall recite rather in *Latin* than *English*, because 'tis very familiarly known, among scholars in that language; and because there is somewhat in it, that seems difficult to be, without circumlocution, render'd intelligibly in *English*: *Natura* (says he) *est principium & causa motus & quietis ejus, in quo inest, primò per se, & non secundum accidens*. Now as, according to *Aristotle*, the whole world is but a system of the works of nature; we might well expect, that the definition of a thing, the most important in natural philosophy, should be clearly and accurately deliver'd; yet, to me, this celebrated definition seems so dark, that I receiv'd no assistance from it, towards framing a clear and satisfactory notion of nature. For I dare not hope, that what, as to me, is not itself intelligible, should make me understand what is to be explained by it. And, consulting some of *Aristotle's* interpreters upon the sense of this definition, I found the more considerate of them so puzzled with it, that their discourses about it seem'd to tend, rather to free the definer from tautology and self-contradiction, than to manifest the definition itself to be good and instructive. And indeed, tho' the immoderate veneration they have for their master, engages them to make the best they can of his definition, even when they cannot justify it without strain'd interpretations; yet what every one seems to defend in gross, almost every one of them censures in parcels; this man attacking one part of the definition, and that another, with objections so weighty, that if I had no other arguments to urge against it, I might borrow enow from the commentators on it, to justify my dislike thereof.

Several things are commonly received as belonging to the idea of nature, that are not manifestly or at all comprehended in this *Aristotelian* definition, which never declares whether the principle, or cause here mentioned, is a substance, or an accident; and if a substance, whether corporeal or immaterial: nor is it clearly contain'd in this definition, that nature does all things most wisely, and still acts by the shortest ways, without ever missing of her end; and that she watches against a *Vacuum*, for the welfare of the universe.

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SECT. II.

THE best way to discover the common opinion of nature, is, to consider what axioms pass for current about her, what titles and epithets are unanimously given her by philosophers, other writers, and by the generality of men who have occasion to discourse of her, and her actions.

The received notion of nature; what?

Of such axioms and epithets, the principal seem to be these.

Nature is exceeding wise, and all her works are perform'd with understanding. Nature does nothing in vain. Nature never fails of her purpose. Nature always does what is best. Nature always acts in the shortest manner. Nature is neither too lavish, nor too sparing in necessary things. Nature always preserves itself. Nature cures diseases. Nature always watches to preserve the universe. Nature dreads a Vacuum.

From which particulars put together, it appears, that the vulgar notion of nature may be expressed by some such description as this.

Nature is a most wise being, that acts nothing in vain, never misses of her ends, but always does what is best, and that by the most direct way, neither employing any things superfluous, nor being wanting in things necessary; teaching and inclining every one of her works to preserve itself: and as, in the human frame, she cures diseases; so, in the world, for the conservation of the universe, she abhors a *Vacuum*; making particular bodies act contrary to their own inclinations and interests, to prevent it.

If I were to propose a notion, as less unfit than any I have met with, to pass for the principal one of nature; with regard to which, many axioms and expressions, relating to that word, may be conveniently understood; I should first distinguish between the universal, and the particular nature of things. And of universal nature, the notion I would offer, should be something like this. *Nature is the aggregate of the bodies, that make up the world, in its present state, considered as a principle, by virtue whereof, they act and suffer, according to the laws of motion, prescribed by the author of things.* And this makes way for the other subordinate notion; since the particular nature of an individual consists in the general nature, apply'd to a distinct portion of the universe; or, supposing that placed as it is, in a world, framed by God, like ours, it must be a convention of the mechanical properties, (such as magnitude, figure, order, situation, and local motion) of parts, convenient and sufficient to constitute of, or entitle to its particular species, or denominations, the particular body they make up; the concurrence of all these being considered as the principle of motion, rest, and changes in that body.

A new notion of nature, general and particular, advanced.

The vulgar notion of nature, as it has had, doubtless, may have an ill effect on religion. The looking upon merely corporeal, and often inanimate things, as endow'd with life, sense, and understanding; and ascribing to nature,

Ill effects of the vulgar notion of nature upon religion.

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and some other Beings, things that belong to God alone, have been grand causes of the polytheism and idolatry of the *Gentiles*.

The most ancient idolatry, probably, was the worship of the celestial lights, especially the sun and moon; as appears both from sacred and profane history. The great *Hippocrates* attempts to account for the origin of the world from his immortal heat, which he esteems as a deity. And *Galen* himself, who was not unacquainted with *Moses's* writings, and with christianity, fancy'd the earth had a certain soul or mind imparted to it by the superior bodies. And an inquisitive person, who, having liv'd many years in *China*, and several of the neighbouring kingdoms, assured me, that, in a solemn conference he had with some of the more eminent doctors of the *Chinese* religion; they frankly profess'd, they believed the heavenly bodies to be truly divine, and deserving to be worshipp'd, because they imparted to men such good things, as light, heat, rain, &c. And this belief, they declared, they thought more rational than that of the *Europeans*, who worship a deity, whose shape, colour, and motion, and whose efficacy on sublunary things, were invisible.

Many of the ancient philosophers held the world to be animated, and believed, that the mundane soul was not barely a living, but a most intelligent and wise active Being; the *Stoics* held the world to be an animal; and the notion of the soul of the world, and of nature, seem so near allied, that some of the old sages appear to have confounded them, and to have made no other universal nature, than the soul of the world. However, the great and pernicious errors they were led into, by believing that the universe itself, and many of its nobler parts, besides men, were endowed with life, understanding, and providence; may make us christians jealous of admitting such a Being, as that which men venerate under the name of nature: since they ascribe to it as many wonderful powers and prerogatives, as the idolaters did to their adored mundane soul. They, also, sacrilegiously abused this Being, as well under the very name of nature, as under that of the soul of the world: sometimes making it the same with the world, at others the same with God; and *Orpheus* has a hymn address'd to nature as a goddess.

Aristotle, indeed, and his commentators, do not so directly idolize nature, as did *Orpheus*; yet, I doubt, they go further than they can justify, when they so freely and often assert many extraordinary things of her, particularly when they call the works of God, the works of nature, and mention him and her together, not as a creator and a creature, but as two co-ordinate governors.

Those who thought the sun endowed, not only with a living soul, but with understanding and a will, must, if they had duly consider'd, have been much more puzzled, to find food for so vast a body, and organs in him necessary to prepare and digest it, and to perform the other functions that belong to animal nutrition, than the philosophers who maintain'd him to be fire. 'Tis without proof presum'd and asserted, that the celestial bodies are endow'd with understanding and prudence, especially, so as to be able to know the particular

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conditions and transactions of men, and hear and grant the prayers of their worshippers. And the moon, which was anciently a principal deity, is so rude and mountainous a body, that 'tis a wonder speculative men, who consider'd how many, how various, and how noble functions belong to a sensitive soul, could think a mass of matter, so very remote from being fitly organized, should be animated and govern'd by a true, living and sensitive soul. Indeed, these deifiers of the celestial globes, and the heathen disciples of *Aristotle*, besides several of the same mind, among the christians, say great and lofty things of the quintessential nature of the heavenly bodies, and their consequent incorruptibility; of the regularity of their motions, and of their divine quality of light, that makes them refugent. But the persuasion they had, of this quintessential nature of the superior part of the world, seems not grounded upon any solid physical reason, but entertain'd by them for being agreeable to the opinion they had of the divinity of the celestial bodies; of which, *Aristotle* himself speaks in a way that hath greatly contributed to such an excessive veneration for those bodies, as is neither agreeable to true philosophy, nor true religion. He himself takes notice, that the *Pythagoreans* held our earth to be one of the planets; and that it moved about the sun, which they placed in the middle of the world. And since this hypothesis of the earth's motion, was in the last age reviv'd by *Copernicus*, not only *Kepler*, *Galileo*, and *Gassendus*, but most of the eminent modern astronomers, have embraced it: which, indeed, is far more agreeable to the phenomena, than the doctrine of *Aristotle* (who was plainly mistaken about the order and consistence of the heavens) and the ancient and generally received *Ptolemaic* system. Now, supposing the terraqueous globe to be a planet; who can consider it as a round mass of very heterogeneous substances, whose surface is very rude and uneven, and its body opaque, (unless as it happens to be enlightened by the sun, moon, and stars) and so very inorganic and unfit for so much as nutrition, that it seems wholly unfit to be an animal, much less a rational one; and ascribe understanding and providence to it? The like may be said of the celestial bodies.

As for the boasted immutability of the heavenly bodies; it may be very probably call'd in question from the phenomena of some of the comets, that by their parallax were found to be above the moon, and consequently in the celestial region of the world. But, the incorruptibility and immutability of the heavenly bodies are more disproveable by the sudden and irregular generation, changes, and destruction of the spots of the sun; which sometimes so suddenly disappear, that in the year 1660, on *May* 8. having left, in the morning, a spot, whose motions we had long observ'd thro' an excellent telescope, with an expectation that it would continue for many days visible to us; we were surpriz'd to find, that when we came to observe it again in the evening, it was quite dissipated: and by comparing it to the sun, we estimated the extent of its surface to be equal to that of all *Europe*. As to the constancy of the motions of the stars; if the earth, which we know to be inanimate, is a planet, it moves as constantly and

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regularly about the sun, as the other planets do, or as the moon doth about the earth. And supposing our globe was not a planet, yet there would manifestly be a constant regular motion of a great part of it: since there is a regular ebbing and flowing twice a day, and spring-tides twice a month, of that vast aggregate of waters, the ocean; which perhaps is not inferior in bulk to the whole body of the moon.

And lastly, as a great proof of the divinity of the stars is drawn from their light; so, tho' I grant it to be the noblest of sensible qualities, yet I cannot think it a good proof of the divine nature of bodies endow'd with it. For tho' the *Zabians* and *Chaldeans* considered and adored the planets as the chief gods, our telescopes discover to us, that they shine but by a borrow'd light; so that *Venus*, as vivid as it appears to the naked eye, is sometimes horn'd like the moon. Thus also the earth, whether a planet or no, is enlighten'd by the sun; and possibly, as a body forty times bigger, communicates more light to the moon, than it receives from her; as seems probable from the light on the surface of the moon in some of her eclipses. And tho' in the night, when the darkness hath widened the *Pupilla* of our eyes, and the moon shines with an unrival'd lustre, she seems exceeding bright; yet she may be, for ought I know, more opake than the solid part of the terrestrial globe. For I have more than once observ'd a small cloud in the west, where the moon then was, about sun-set; and comparing them together, the cloud reflected the light as strongly to my eye, as did the moon, that seem'd to be not far from it; both of them appearing like little whitish clouds: tho' afterwards, as the sun descended lower beneath the horizon, the moon grew more luminous. And, speaking of light indefinitely, 'tis so far from arguing a divine nature in the bodies endow'd with it, (whether, as the planets, by participation from an external illuminant; or, as the sun, from an internal principle) that a calcined stone, witness that of *Bolonia*, will afford, in proportion to its bulk, incomparably more borrow'd light than one of the planets. And a light, from internal constitution, may be found, not only in such abject creatures as insects, whether winged, as the *Cucupias* of *Hispaniola*, or creeping, as the glow-worm; but also in bodies inanimate, and corrupted; as in rotten wood, putrefy'd fish, &c.

Reasons against
admitting the
vulgar notion
of nature.

The reasons that have made me backward to entertain such a notion of nature, as I have hitherto discours'd of, may be comprized under the following.

1. Such a nature seems to be either asserted, or assumed, without sufficient proof; and this single reason, if well made out, is alone sufficient to discredit it. For, in matters of philosophy, where we ought not to take any thing upon trust, or allow it without proof; 'tis enough to keep us from believing a thing, that we have no positive argument to induce us to assent to it; tho' we have no particular arguments against it. Now, I have yet met with no physical arguments, either demonstrative, or considerably probable, to evince the existence of the nature we examine; yet, surely, so universal an agent, supposed to act immediately, in abundance of

of phenomena, should, if it really existed, give some manifest proofs of itself.

2. Such a nature is unnecessary. For, since a great part of the work of true philosophers has been to reduce the principles of things to the smallest number they can, without making them insufficient; why should we take in a principle, of which we have no need? Supposing the common matter of all bodies to have been at first divided into innumerable minute parts, by the wise author of things, and these parts to have been so dispos'd, as to form the world as it now is; and, supposing the universal laws of motion, among the parts of matter, to have been establish'd, and several conventions of particles contriv'd into the seminal principles of various things; all which may be effected by the mere motion of matter, skilfully guided at the beginning of the world: supposing all this, together with God's ordinary and general concurrence, which we very reasonably may; I see not, why the same phenomena, that we now observe in the world, should not be produc'd, without taking in any such powerful and intelligent being, distinct from God, as nature is represented to be. And, 'till some instance is produc'd to the contrary, I shall think that the phenomena we observe, will genuinely follow from the mere fabric and constitution of the world. Thus, supposing the sun and moon to have been put, at first, into such motions about the earth, as experience shews they have; the determinate celerity of these motions, and the lines wherein they are performed, will make it necessary, that the moon should exhibit such several phases as every month she doth; and that at some times she and the sun should have a trine, or a quartile aspect, &c. and that now one, and now the other of them, should, at set times, suffer an eclipse: tho' these eclipses were by the Romans, and others of old, and are by many unlearn'd nations at this day, look'd upon as supernatural things. And, indeed, the difficulty we find to conceive how so great a fabric as the world, can be preserved in order, and kept from falling again to a chaos, seems to arise from hence, that men do not sufficiently consider the unsearchable wisdom of the divine architect, whose piercing eyes were able to look at once quite thro' the universe, and take into his view, both the beginning and end of time: so that, perfectly knowing what would be the consequences of all the possible conjunctures of circumstances, into which matter, divided and mov'd according to particular laws, could, in an engine so constituted as the present world is, happen to be put; there can nothing fall out, unless when a miracle is wrought, that shall be able to alter the course of things, or prejudice the constitution of them, any further, than he from the beginning foresaw, and allow'd.

3. The nature I question, is so dark and odd a thing, that 'tis hard to know what to make of it; being scarce intelligibly propos'd by those who lay the most stress upon it. For, it appears not clearly, whether they will have it to be a corporeal, or an immaterial substance; or some such thing, as may seem to be betwixt both: such as many *Pecipatetics* represent substantial forms, and what they call real qualities, which several school-

men.

PHYSICS men hold to be separable from all matter whatsoever. If it be merely corporeal, I confess, I understand not, how it can be so wise an agent, as they would have it pass for: if it be a body, I would gladly know what kind of body it is; and how, since, among bodies, there can be no penetration of dimensions, this so intimately pervades, as they pretend, all the other bodies of the world? If it be said, that nature is a *Semi-substantia*, as some modern school-men call substantial forms, and real qualities; I answer, that I acknowledge no such chymical, and unintelligible Beings. It remains therefore, that this nature we speak of, if positive, is an immaterial substance. But, to have recourse to such an one, as a physical agent, and the grand author of the motion of bodies; and that, especially, in such familiar phenomena, as the ascent of water in pumps, the suspension of it in watering-pots, the running of it thro' syphons, &c. and thence to assign the cause hereof, will, I think, prove exceeding difficult.

4. Since many naturalists are christians, it may not be improper to add, that another thing, for which I dislike the vulgar notion of nature, is, its being dangerous to religion, in general, and, consequently, to the christian.

For this erroneous conceit defrauds the true God of several acts of veneration and gratitude, that are due to him from men, upon account of the visible world; and diverts them to that imaginary being they call nature, which has no title to them: for, whilst nature is suppos'd to be an intelligent thing, that wisely and kindly administers all that is done among bodies, 'tis no wonder that the generality of men should admire and praise her, for the wonderful, and useful things they observe in the world. And, in effect, tho' nature, in our present sense of the word, be never found in the sacred writings; yet, nothing is more frequently met with in the books of philosophers, than nature and her effects. And, whatever has been said by some, in excuse of *Aristotle*, yet the generality of the *Peripaterics*, from whom the vulgar notion of nature is chiefly received, make the world eternal; and refer all the transactions among the bodies it contains, to what they call nature. Whence 'twill not be difficult to perceive, that, if they do not quite exclude God; yet, as they leave him no interest in the first formation of the universe, so they leave him but very little in the administration of the parts it consists of, especially in such as are sublunary. Instead, therefore, of the true God, they have substituted, for us, a kind of a goddess, with the title of nature; upon whom they look as the immediate agent and director in all excellent productions, and ascribe to her the praise and glory of them.

This great error, in a point of such importance, seems to undermine the foundation of religion. For since the most general and effectual argument which has persuaded men that there is a God and a providence, is afforded by the consideration of the visible world, wherein so many operations and other things are observ'd to be managed with such conduct and benignity, as cannot justly be ascribed but to the wisdom and goodness of a deity; they who ascribe these things to mere nature, much weaken the force of

of that argument, if they do not quite take away the necessity of acknowledging a deity; by shewing, that, without having recourse to him, an account may be given of the administration of the world, and of what is perform'd among things corporeal. Indeed, when men are put upon considering the matter, and press'd to declare themselves more clearly; they fear to affirm, that God and nature are the same thing, and will confess, that she is but his vicegerent; yet, in practice, their admiration, and their praises, are frequently given to nature, not to God.

In short, as nature is so frequently recurr'd to, and so magnify'd in the writings of philosophers, that the excessive veneration men have for her, caused some (as the *Epicureans*) to deny God; so, 'tis to be fear'd, that it makes many forget him. And, perhaps, a suspicious person would add, that, were it not for other principles, this erroneous idea of nature would, too often, have a strong tendency to shake, if not to subvert, the very foundations of all religion; by misleading those who are inclined to be its enemies, from overlooking the necessity of a God, to the denial of his existence.

5. There are many phenomena, which do not agree with that notion, or representation of nature, I call into question. For, if there were such an intelligent, powerful, and vigilant being, as philosophers describe nature to be, several things would not be done, which experience assures us are done.

The great vigilance of nature, for the common good of the universe, is pretended to be demonstrated from the care she takes to prevent, or replenish a *Vacuum*; which would be very prejudicial to the fabric of the world. 'Tis alledged, that nature, in pumps, and the like cases, raises the heavy body of water, in spite of its tendency towards the centre of the earth, to obviate, or fill up a vacuity; and that out of a gardener's watering-pot, or an inverted tube, stopped at one end, neither water, nor quick-silver will fall down, lest it should leave a *Vacuum* behind it: but I demand how it comes to pass, that if a glass-tube be but a foot longer than 34 or 35 feet; or an inverted tube, fill'd with quick-silver, but a finger's breadth longer than 30 inches, the water in the one, and the quick-silver in the other, will subside? Is it possible, that nature, which, in pumps, is said to raise up every day so many hundred ton of water; and, if we believe the schools, would raise it to any height; should not have the discretion, or the power, to lift up, or sustain, as much water as would serve to fill one foot in a glass tube, or as much quick-silver as an inch of a slender pipe will contain; to obviate, or replenish, the *Vacuum*, she is said to abhor? Sure, at this rate, she must either have very little power, or very little knowledge of the power she has. So, likewise, when a glass-bubble is blown very thin at the flame of a lamp, and hermetically seal'd whilst 'tis very hot, the reason assign'd for its being apt to break, when it grows cold, is, that the inward air, which was before rarify'd by the heat, coming to be condensed by the cold, lest the space deserted by the air, that thus contracts itself, should be

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be left void, nature, with violence, breaks the glass: but if the glass be blown a little stronger than ordinary, tho' at the flame of a lamp, the bubble, as I have often try'd, will continue unbroken, in spite of nature's abhorrence of a *Vacuum*. But if, tho' nature did not hinder the water, or the quick-silver, from falling down, there would no such *Vacuum* ensue as she is said to abhor; why does she seem so solicitous to hinder it; why keep three or four and thirty feet of water, in perpendicular height, contrary to the nature of all heavy bodies, suspended in the tube; and why so furiously break to pieces a thin seal'd bubble, to hinder a *Vacuum*, if, in case she did not break it, no *Vacuum* would ensue? On the other side, if we admit her endeavours to hinder a *Vacuum*, not to have been superfluous, we must confess, that, where these endeavours succeed not, there is really produced such a *Vacuum*, as she is said to abhor: so that, either she must be very indiscreet to trouble herself, and transgress her own ordinary laws, to prevent a danger she need not fear; or else her strength must be very small, that is not able to fill a small vacuity or to break a brittle glass-bubble.

Another grand instance given of the wisdom of nature, and her watchfulness for the good of the whole world, is, the appetite she has implanted in all heavy bodies, to descend to the centre of the earth, and in all light ones, to ascend. But, for positive levity, till I see it better prov'd, than it hath hitherto been, I allow no such thing implanted in sublunary bodies.

A ball being let fall upon the ground, will rebound, perhaps, several times, before it rests. If you ask why the ball being dropt out of the hand, does not move on this or that side, or upwards, but fall directly toward the centre of the earth, in that shortest line, the diameter of the earth produced to the centre of gravity of the ball? 'Twill be readily answer'd, that this proceeds from the ball's gravity, an innate appetite, whereby it tends to the centre of the earth the nearest way. But then I demand, whence comes this rebound, this motion upwards? for, 'tis plainly the genuine consequence of the motion downwards, and is, therefore, increas'd as that motion in the ball was increas'd, by falling from a greater height: so that it seems, that nature, in such cases, plays a very odd game, since she forces a ball, against the laws of heavy bodies, to ascend several times upwards, upon account of that very gravity whose office it is to carry it downwards the directest way: at least, she seems, in spite of the wisdom ascribed to her, to take her measures very ill, in making the ball move downwards with so much violence, as occasions it several times to fly back from the place she intended it should go to. As if nature could not manage a ball without letting it be hurried on with far greater violence, than her design requir'd.

The same may be said of a pendulum: for, since 'tis unanimously affirm'd, that this falls to the perpendicular, upon account of its gravity, 'tis certainly a motion proceeding from the same gravity, that the swinging weight passes beyond the perpendicular, consequently ascends, and
often

often makes a multitude of vibrations; and, therefore, very frequently ascends, before it comes to rest in the perpendicular.

There are several instances of persons choak'd with a hair. The reason of this fatal accident, is probably, the irritation made, by the stay of so unusual a thing in the throat; whence arise very violent and disorderly motions to expel it, in the organs of respiration; by which means, the continual circulation of the blood, necessary to life, is hinder'd, the consequence whereof is speedy death. But this agrees very ill with the vulgar supposition of such a kind and provident Being, as nature, who is represented as always at hand to preserve the life of animals, and succour them in their dangers and distresses: for since a hair is so slender a body, that it cannot stop the throat, so as to hinder, either the free passage of aliment into the stomach, or that of air to or from the lungs; were it not much better for nature, to let the hair stay there, 'till the juices of the body have resolv'd or consum'd it, or some favourable accident remov'd it, than like a passionate and transported thing, oppose it with such blind violence, that instead of ejecting the hair, she expels the life of the person?

How the care and wisdom of nature will be reconcil'd to so improper and disorderly a procedure, I leave her admirers to consider. But it will appear very reconcileable to providence: for, in regard of the use and necessity of deglutition, and, in many cases of coughing and vomiting, it was, in the general, most convenient, that the parts subservient to these motions, should be irritated by the sudden sense of things that are unusual, tho', perhaps, they would not be otherwise dangerous or offensive; because it was fit that the providence of God should, in providing for the welfare of animals, have more regard to that which usually and regularly befalls them, than to extraordinary cases, or unfrequent accidents; and the like considerations will sufficiently keep the divine providence from appearing to act irrationally or injuriously in other cases.

It has been frequently observ'd, that pregnant women have been made to miscarry by the smell of an extinguish'd candle, which would before have indeed displeas'd, but not endanger'd the same persons: so that nature, in these cases, seems very far from being so prudent and careful as men usually fancy her, since by an odour she is put into such unruly transports: and instead of watching for the welfare of the woman, whose condition needed an extraordinary measure of her care and tenderness, she violently precipitates her charge into a danger, that often proves fatal, not only to the mother, but also to the child.

How the gross aberrations of nature, in the productions of monsters, will agree with that great uniformity, and excellent skill, ascribed to her in seminal productions, I leave the naturists to consider. Some of them, I know, lay the fault upon the stubbornness of the matter, that would not obey the plastic power of nature; but 'tis strange it should be pretended, that nature, which they make a kind of semi-deity, cannot mould and fashion so small, soft, and tractable a portion of matter, as that wherein the first model and efformation of the embryo is made; when, at the same time, they

PHYSICS. tell us, she is able, in sucking-pumps, to raise and sustain whole tons of water, to prevent a *Vacuum*; and can toss up into the air, houses, walls, castles, and the rocks they are built on, to let kindled gun-powder have the expansion its new state requires.

It may be said, that unless we admit such a Being as nature, to contrive and manage corporeal things, and, in a regular, and methodical way, direct them to their respective ends, there will appear no visible footsteps or proof of a divine wisdom in the world. This argument, I confess, is so specious, that it made me long hesitate what to think of the receiv'd notion of nature. But having further consider'd the matter, I saw it might be answer'd, that the curious contrivance of the universe, and many of its parts, and the orderly course of things, with a manifest tendency to determinate ends, are matters of fact, and depend not upon the supposition of such a Being as they call nature; but, setting aside this or that hypothesis, may be known by inspection, if those who make it be attentive and impartial. Thus when a man sees a human body skilfully dissected, he cannot, if he be intelligent and unprejudic'd, but acknowledge, that there is a most curious and exquisite contrivance in that incomparable engine, and in the various parts of it, which are admirably fitted for distinct and determinate uses. So that I cannot suppress the manifest tokens of wisdom and design, that are to be observed in the wonderful construction and orderly operations of the world and its parts; but endeavour to refer these indications of wisdom to the true and proper cause. And, as in the hypothesis of the objectors, there may be three causes assign'd of these foot-steps of wisdom, *God, Nature, and Chance*; if, according to our doctrine, *Nature* be laid aside, the competition will remain only between *God* and *Chance*: and sure he must be very dull, or very strongly prejudic'd, who shall think it reasonable to attribute such admirable contrivances, and such regular conducts, as are observable in the corporeal world, rather to chance (which is no proper cause at all) than to a most intelligent Being, from which the most curious productions may well be expected. But if such a celebrated thing, as nature is commonly thought, be admitted, 'twill not be near so easy to prove the wisdom of God by his works, since these may have another cause; that most watchful and provident Being, which men call nature. And this will be more particularly difficult in the *Peripatetic* hypothesis of the eternity of the world: for, according thereto, there appears no necessity, that God should have any thing to do with it, since he did not make this *Automaton*, but it was always self-existent, not only as to matter, but as to form too: and as for the government or administration of the bodies it consists of, that is the proper business of nature. If it be objected, that this Being is by its assertors acknowledg'd to be subordinate to God; I answer, that, as it may justly be question'd, whether many philosophers, and, perhaps, some sects of them, who are adorers of nature, confess'd her to be but the substitute of a superior and divine Being; so, this distinction and subordination is not easy to be prov'd against those that side with the other ancient philosophers, who either acknowledg'd no such thing, or expressly deny'd

deny'd it. Besides, this objection supposes the existence and superiority of a Deity, which therefore needs to be prov'd by other ways; whereas, on my hypothesis, the same phenomena that discover admirable wisdom, and manifest designs in the corporeal world, of themselves afford a solid argument, both of the existence, and of some of the grand attributes of God, with which the rest, that properly belong to him, have a necessary connexion.

The reasons which I conceive may have induced philosophers to take up, and rely on the receiv'd notion of nature, are such as these.

*The reasons
whereon the vul-
gar notion of na-
ture depends,
examined.*

1. One of the most obvious may be taken from the general belief, or, as men suppose, observation, that several bodies, as particularly the earth, water, and other elements, have each of them its natural place assign'd it in the universe; from which place, if any portion of the element, or a mix'd body, wherein that element predominates, happens to be remov'd, it has a strong incessant appetite to return to it; because, when there, it ceases to gravitate, and is then in a place which nature has qualified to preserve it.

Now, I readily grant, that there being such a quantity of very bulky bodies in the world, 'twas necessary they should have places adequate to their bigness; and 'twas thought fit by the wise architect of the universe, that they should not be all blended together, but that a great portion of each should, at the beginning of things, be dispos'd of and lodg'd in a distinct and convenient place. But I see no necessity of granting what is asserted in the argument: for inanimate bodies having no sense or perception, it must be all one to them in what place they are; because they cannot be concern'd to be in one place rather than in another, since such a preference would require a knowledge that inanimate things have not. And, for the same reason, a portion of an element, remov'd by force or chance, from what they call its proper place, can have no real appetite to return thither; for, who tells it that 'tis in an undue place, and that it may better its condition by removing into another? And, who informs it, whether that place lies on this side of it, or on that, above it, or beneath it? Some philosophers, indeed, have been aware of the weakness of the argument, drawn from the vulgar instance of terrestrial bodies; which being let fall from an eminence, or thrown into the air, fall of themselves, in a direct line, towards the centre of the earth; and therefore they have strengthen'd it, by pretending, that these bodies have not (as the ancients pretended) an appetite to descend to the centre of the earth, but to the great mass of their connatural bodies. I will not, therefore, accuse these philosophers, of the inconsiderate opinion of their predecessors, who would have nature make all heavy things affect to lodge themselves in the center of the earth, which being but a point, cannot contain any one of them; yet the hypothesis of these is liable to other weighty objections.

For, first, it is not conceivable, how an inanimate body should have an appetite to re-unite with homogeneous bodies, whose situation, and distance are unknown to it.

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(2.) It does not appear that all bodies have such an appetite, as is presum'd, of joining themselves to greater masses of connatural bodies. If you file the end of an ingot of silver or of gold, the filings will not stick to their own mass, placed ever so near, or made to touch them, much less will they leap to it, when 'tis at a distance from them. The like may be said of almost all the consistent bodies we are acquainted with, except the load-stone and iron, and bodies that participate of one or the other of them.

(3.) 'Tis obvious, that what makes terrestrial matter fall thro' the air to the earth, is some general agent, which, according to the wise disposition of the author of the universe, determines the motion of those bodies we call heavy, by the shortest ways permitted them, towards the central part of the terraqueous globe; whether the body, put into motion downwards, be of the same, a like, or a quite different nature, from the greater mass of matter, to which, when 'tis associated, it rests. If, from the side of a ship, you let fall a chip of wood, when your arm is so stretch'd out, that the perpendicular, or shortest line, between that and the water, lies ever so little without the ship; that chip will fall into the sea, which is a fluid body, and quite of another nature than itself, rather than swerve in the least from its line of direction, to rejoin itself to the great bulk, of wood, whereof the ship consists. On the other side, if a man, standing upon the shore just by the sea, shall pour out a glass of water, holding the glass just over his feet; that water will fall into the sand, where 'twill be immediately soaked up, and dispersed, rather than deviate a little, to join itself to so great a mass of connatural body, as is the ocean.

To the general belief, usually made part of the preceding argument, that water does not gravitate in water, its own natural place; I reply, that water does gravitate in water, as well as out of it, and I elsewhere prove it.

2. Another argument, in favour of the receiv'd opinion of nature, may be drawn from the strong appetite that bodies have to recover their usual state, when, by any means, they are put out of it, and thereby forc'd into a state that is called preternatural; as air, violently compressed in a blown bladder, will, as soon as the force is remov'd, return to its first dimensions: as the blade of a sword bent, restores itself by its innate power, as soon as the force ceases; and as water, made hot by the fire, hastens, when 'tis removed thence, to recover its former coldness.

But this argument, is grounded on the affections of inanimate bodies; and since an inanimate portion of matter is confessedly destitute of knowledge and sense; it is incapable of concern to be in one state, or constitution, rather than another; for, it has no knowledge of that wherein it is at present, nor remembrance of that from which it was forc'd; and, consequently, no appetite to forsake the former, and return to the latter. But every inanimate body, being, of itself, indifferent to all places and states, continues in that place, or state, to which the action and resistance of other bodies, and especially contiguous ones, effectually determine it.

And, before it be asserted, that water heated, returns, of itself, to its natural coldness; it were fit, that the assertors should determine, what

what degree, or measure of coldness is natural to that liquor; and this, if I mistake not, will prove no easy task. And I think it yet more difficult to determine, what degree of coldness is natural to water, since this liquor perpetually varies its temperature, as to cold and heat, according to that of the contiguous, or neighbouring bodies, especially the air. And therefore the water of an unshaded pond, for instance, tho' it rests in its proper and natural place, as they speak; yet, in autumn, if the weather be fair, the temper of it will much vary in the compass of the same day; and the liquor be much hotter at noon, than early in the morning, or at midnight; tho' this great diversity be the effect only of a natural agent, the sun, acting according to its regular course. And, in the depth of winter, 'tis generally confessed, that water is much colder than in the heat of summer; which seems to be the reason of what is observ'd by watermen, that, on rivers, boats equally laden, will not sink so deep in winter as in summer; the cold condensing the water, and, consequently, making it specifically heavier in the former season than in the latter.

As to the motion of restitution, observable upon the removal, or ceasing, of the force in air violently compressed, and in the blade of a sword, forcibly bent; I confess, it seems to me very difficult to assign the true mechanical cause of it: yet, I think it far more likely, that the cause should be mechanical, than that the effect proceeds from such a watchfulness of nature, as is pretended. For I question whether we have any air here below, that is in other than a violent state; the lower parts of our atmosphere being constantly compressed by the weight of the upper.

A long narrow plate of silver, that has not been hammer'd, compressed, or made red-hot in the fire, and suffer'd to cool leisurely, may be bent any way, and will constantly retain the last curve figure, that you gave it. But if, having again straiten'd this plate, you give it a few smart strokes with a hammer, it will, by that mere mechanical change, become a springy body. The like may be observ'd in copper, but not so remarkably, and scarce at all, in lead. From these phenomena, I demand why, if nature be so careful to restore bodies to their former state, she does not restore the silver-plate to its straitness, when it is bent this way, or that way, before it be hammer'd? and why a few strokes of a hammer should entitle the plate to nature's peculiar care, and make her solicitous to restore it, when bent? And why, if the springy plate be again heated and cooled of itself, nature abandons her former care, and suffers it quietly to continue in what crooked posture one pleases to put it? And what is the reason of nature's greater partiality to silver, copper, and iron, than to lead and gold, with regard to the motion of restitution? However, even in sword-blades, it has been often observed, that tho' if, soon after they are bent, the force that bent them be withdrawn, they will suddenly return to their former straitness; yet, if they be kept too long curved, they will lose their elastic power, and continue in that crooked posture, tho' the force that put them into it, ceases to act: so that, it seems, nature easily forgets the care she was here presumed at first to take.

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3. It passes for current, and seems much to favour the opinion of the naturalists, that what is violent, is contrary to nature, and therefore cannot last long. This trite sentence is, by the schools, so particularly apply'd to local motion, that some of them have made it the characteristic, whereby to distinguish natural motions from others; that the former are perpetual, or at least very durable, but the latter, being continually check'd by the contrariety of nature, continually decay, and are, within no long time, suppressed.

But it may be justly questioned, whether there is any motion among inanimate bodies, that deserves to be called violent, in contradistinction to natural; since, among such, all motions, where no intelligent agent intervenes, are made according to universal, and mechanical laws.

And methinks, the *Peripatetics*, who are most forward to employ this axiom, should find but little reason for doing so, if they consider'd how unfuitable it is to their doctrine, that the vast body of the firmament, and all the planetary orbs are, by the *Primum Mobile*, with a stupendous swiftness, whirl'd about from east to west, in four and twenty hours, contrary to their natural tendency; and, that this violent and rapid motion, of the incomparably greater part of the universe, has lasted as long as the world itself, that is, according to *Aristotle*, for innumerable ages.

We may also observe here below, that the ebbing and flowing of the sea, which is generally suppos'd to proceed either from the motion of the moon, or that of the terrestrial globe, or some other external cause, has lasted for some thousands of years, and probably will last as long as the present system of our vortex continues. That other great ocean too, the atmosphere, consists of numberless myriads of corpuscles, that are here below continually kept in a violent state; since they are elastical bodies, whereof the lower are still compress'd by the weight of the upper. And, to make a spring of a body, it is requisite that it be forcibly bent or stretch'd, and have such a perpetual endeavour to fly open, or to shrink in, that it will not fail to do either, as soon as the force, that hinder'd it, is remov'd. And, as for the states of inanimate bodies, I see not, that their being or not being natural, is, with any certainty, manifest from their being or not being very durable. For leaves that wither in a few months, and blossoms that often fade in a few days, are as well natural bodies, as the solid and durable trees that bear them. And 'tis obvious, that, whether we make the state of fluidity, or of congelation, to be natural to water, and the other violent; its change from one of those states into the other, and even its return to its former state, is often, at some seasons, and in some places, made perhaps in an hour or less, by causes acknowledg'd to be natural. Mists, hail, whirlwinds, lightning, falling-stars, &c. tho' natural bodies, are far from being lasting; especially in comparison of glass, wherein the ingredients, sand and fix'd salt, are brought together by great violence of fire. And the motion that a thin plate of this glass can exercise, to restore itself to its former position, when forcibly bent, is in great part a lasting effect of the same violence of the fire.

fire: and so is the most durable perseverance of the alkaline salt, one of the two ingredients of glass, tho' very easily soluble in water and other liquors, and even in the moist air itself.

4. There is a distinction of local motion, into natural and violent, so generally received and used, both by philosophers and physicians, that it deserves our notice here; since it implicitly contains an argument for the existence of the thing call'd nature, by supposing it so manifest, that an important distinction may justly be grounded on it.

This objection, I confess, is difficult to clear; not for any force in it, but the ambiguity of the terms, wherein the distinction is employ'd: for most men speak of this distinction of motion, in so obscure, or so uncertain a way, that 'tis not easy to know what they mean by either member thereof. Yet some there are, who endeavour to speak intelligibly, and define natural motion to be that, whose principle is within the moving body; and violent motion, that which bodies are put into by an external agent. But, even according to this explication, I am not satisfied with the distinction: for, as 'tis a principle received, and frequently employ'd, by *Aristotle* and his followers, *That whatever moves, is put in motion by somewhat else*; it seems, that, according to this axiom, all motion may be call'd violent, since it proceeds from an external agent. And indeed, according to the school-philosophers, the motion of far the greatest part of the visible world, tho' this motion be most regular and lasting, must hence be reputed violent; since they assert, that the immense firmament itself, with all the planetary orbs, is perpetually, and against its native tendency, hurry'd about the centre of the world, once in twenty-four hours, by an external, invisible agent, which they therefore call the *Primum Mobile*. And as for the criterion of natural motion, that *its principle is within the moving body*, it may be said, that all bodies, once in the state of actual motion, whatever cause first brought them to it, are moved by an internal principle: for instance, an arrow, that actually flies in the air towards a mark, moves by some principle or other residing within itself; for it does not depend on the bow 'twas shot out of, since 'twould continue, tho' that were broken, or annihilated; nor does it depend upon the medium, which more resists than assists its progress. And if we should suppose the air either to be annihilated, or render'd incapable of furthering or hindring its progress, I see not why the motion of the arrow must necessarily cease, since in this case there remains no medium to oppose its progress. When in a watch that is wound up, the spring endeavours to unbend itself; and when the string of a drawn bow is broken or let go; the spring of the former, and the woody part of the latter, both return to a less curved line. And tho' these motions are occasioned by the forcible acts of external agents, yet the watch, the spring, and the bow, have in themselves, for ought appears to those I reason with, an inward principle, by which they are mov'd, till they have attain'd their position. Some would add, that a squib, or a rocket, tho' an artificial body, seems, as well as a falling-star, to move from an internal principle. On the other side, external agents are requisite

to many motions, acknowledg'd to be natural; as the heat of the sun to the germination and flourishing of several plants, hung up in the air in the spring. And if in the air-pump you place several insects, and withdraw the common air from the receiver, they will lie moveless, as if they were dead, tho' it be for several hours, whilst they are kept from enjoying the air: but when that is permitted again to return upon them, they will presently revive, and be brought to move again; as if a fly, for instance, resembled a little wind-mill, in being moveless of itself, and requiring the action of the air to put its wings, and other parts into motion. But, since motion is not essential to matter, as divisibility and impenetrability are believ'd to be; the motions of all bodies, at least at the beginning of things, and of most, the causes of whose motions we can discern, were impress'd on them; either by an external immaterial agent, God, or by external portions of matter acting on them.

And tho' motion be deservedly made a principal part of *Aristotle's* definition of nature; yet men usually call such motions natural, as are very hard to distinguish from those they call violent. Thus, when water falls to the ground, they tell us, this motion is natural to that liquor, as 'tis a heavy body; but when a man spurts water into the air, they pronounce the motion, because of its tendency upwards, to be contrary to nature: yet when he draws water into his mouth thro' a pipe held perpendicularly, they will have this motion, tho' directly upwards, to be not violent, but natural. So when a blown bladder, let fall upon a floor, rebounds, the descent and ascent are both said to be natural motions; tho' the former tends towards the centre of the earth, and the latter from it. And so, if, from a considerable height, you let fall a sphere of some close wood, not too heavy, into a deep vessel of water, it will descend far in that liquor, by a natural motion; yet its contrary motion, upwards, ought not to be esteem'd violent, since, according to the schools, being lighter *in specie* than water, 'tis natural to it to affect its proper place; for which purpose, it must ascend to the top of the liquor, and float. 'Tis, however, from these tendencies to opposite points, that men judge many motions of bodies to be natural, or violent.

And, indeed, since it must be indifferent to a lifeless and insensible body, to what place 'tis made to move, all its motions may, in one respect, be said to be natural; and, in another, violent: for as very many bodies, of a visible bulk, are set in motion by external impellents, upon which account, their motions may be said to be violent; so the generality of impell'd bodies, move upwards, downwards, &c. toward any part of the world, in what line soever they find their motion least resisted; which impulse and tendency being given by virtue of what they call the general laws of nature, the motion may be said to be natural.

The vulgar distinction of motion, therefore, into natural and violent, is not so clear and well-grounded, as to oblige us to admit that there is such a Being as the naturists assert.

5. I come now to consider the argument that may be drawn in favour of the receiv'd notion of nature, from the critical evacuations, which happen in diseases; and the strange stratagems that nature is said to make use of, to free herself from the noxious humours that oppress her.

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The vulgar notion of a crisis examin'd.

This argument I willingly acknowledge to be very considerable; for we really see, that in continual fevers, especially in hot climates, there usually happen, at certain times of the disease, notable commotions, or conflicts, after which the morbid matter is disposed of, and discharged by ways strange and surprizing; to the great and sudden relief of the patient. Upon this account, I take the argument drawn from crises to be much the strongest that can be urged for the opinion from which I dissent.

But the body of a man is an incomparable engine, which the most wise author of things has so skilfully framed, for lasting many years, that, if there were in it an intelligent principle of self-preservation, (as the naturalists suppose there is) things would not, in most cases, be better, or otherwise managed, for the preservation of the animal, than they generally are. So that the question is not, whether there be a great deal of providence and wisdom exercised in the crises of diseases; but upon what account it is, that these apposite things are perform'd? 'Tis the universal opinion of physicians, that 'tis the intelligent principle they call nature; which, being solicitous for the welfare of the patient, and distressed by the quantity, or hurtfulness of the morbid matter, watches her opportunity to expel it hastily out of the body, by the most safe and convenient ways, which, in the present condition of the patient, can be taken. I, on the other side, attribute crises to the wisdom and ordinary providence of God, exerting itself by the mechanism of that great machine, the world; and of that smaller engine, the human body, as constituted in the patient's circumstances. The reasons that hinder me from acquiescing in the general opinion of physicians, about crises, are principally these.

Crisis, properly so call'd, very seldom happen, except in fevers, and the like acute diseases; where, according to the common course of things, the disorder is soon terminated, either by recovery, death, or a change into some other disease: but chronical sicknesses, unless they happen to be accompany'd with feverish distempers, usually have no crises. This argues, that nature doth not make critical evacuations, upon account of such care and watchfulness, as physicians ascribe them to; since she neglects to employ so wholesom an expedient in diseases, that are often no less dangerous and mortal than acute ones, which she attempts to cure by crises.

Again, critical evacuations may be procured by the bare mechanism of the body; for, by virtue of that, the fibres, or motive organs of the stomach, *Viscera*, and other parts, being distended, or vellicated, by the abundance, or acrimony, of the peccant matter, will, sometimes, by such an irritation, be made to contract themselves vigorously, and to throw out the matter that offends the part, either by the emunctories of the body, or by whatever passages it can, with most ease, be discharged. Thus,

PHYSICS. when some men find their stomachs burden'd with aliment, they provoke it to unload itself with a feather, or the like, without being beholden to nature's watchfulness for a crisis, which, probably, she would not attempt. And as 'tis usual for crises to be made in fevers by large hemorrhagies at the nose, and other parts; which is ascribed to nature's care for the patient's recovery; I must add, that it hath been often observed, that even, after death, large bleedings have happen'd at several other parts of the body; which shews, that such excretions may be made by virtue of the structure of the machine, or the turgescence and acrimony of the humours, without any design of nature to save the life of the patient.

Indeed, if it appear'd, by experience, that all crises of diseases either expell'd the morbid matter, or notably relieved the patient; the critical attempts of nature would much favour the opinion men have conceiv'd of her vigilance and conduct: but unwelcome instances daily shew, that, as some crises are salutary, so others prove mortal. And, among those that do not directly kill the patient, there are several which leave him in a worse condition than he was before. I, therefore, wonder not, that physicians have thought themselves obliged to lay down several circumstances, as necessary requisites of a laudable crisis; if any of which be wanting, 'tis not thought of the best kind: and if the contrary to some of them happen, 'tis to be judged either pernicious, or hurtful. For, as there are two general ways, supposed to be employ'd by nature, in making crises; the one by expulsion of the peccant matter out of the body; and the other, by the settling of the matter somewhere within it; neither of these is constantly successful.

Whence experience hath obliged physicians to divide crises not only into perfect, that fully determine the event of the disease; and imperfect, that only alter it for the better or the worse; but into salutary, that quite relieve the patient; and mortal, which destroy him. And, to a perfect and salutary crisis, some require no less than six conditions; that it be preceded by signs of coction; that it be made by a manifest and sufficient excretion, or translation; that it happen upon a critical day; that it leave no relicks behind, to endanger a relapse; that it be without dangerous symptoms; and, lastly, suitable to the nature of the disease, and the patient.

Hence it may appear no common thing to meet with a perfect and salutary crisis, since so many laudable conditions must concur in it. And, indeed, nature usually takes up but with imperfect ones; and it were happy if she made no better, provided she made no worse. But 'tis found, by sad experience, that she rouses herself up to make a crisis, not only upon improper days, but also when there appear no signs of coction; and, by these unseasonable attempts, weakens the patient and encreases the malady, or, perhaps, makes it speedily mortal. Nor will it justify nature, to say, that these attempts are accidentally brought on, by the acrimony, or importunity of the morbid matter, by which she is provoked, before the time, to endeavour an expulsion of it. For if nature be so prudent and

and watchful a guardian, as is thought, she should not suffer herself to be provoked to act preposterously, and make furious attempts, that lavish the little strength the patient hath so much need of. And, therefore, physicians often do very well, when, agreeably to the dictates of prudence, they forget how much wisdom they are apt to ascribe to nature, and employ their best skill and remedies to suppress, or moderate, the inordinate motions, or the improper and profuse evacuations, that she, when irritated, rashly begins to make. And tho' the crises that are made by a translation of the peccant matter, or by lodging it in some particular part of the body, be often, when they are not salutary, somewhat less hurtful than those made by excretion; yet these, frequently, prove dangerous; sometimes producing inward imposthumes, or external tumors, in parts either noble by their functions, situation, connexion, or sympathy with others, that are not to be without hazard, or great inconvenience, oppress'd.

I know that physicians make it a great argument of nature's providence and skill, that she watches for the concoction of the peccant matter, before she attempts to expel it by a crisis. But what is to be meant by this coction of humours, they do not clearly declare. As I understand it, when they say, that a portion of peccant matter is brought to coction, they mean, that it has acquir'd such a disposition, as makes it more fit than before, to be separated from the sounder portion of the mass of blood, or from the consistent parts to which it, perhaps, formerly adhered; and to be afterwards expell'd out of the body. As where the lungs are affected by a fresh taken cold, we see, that, after a few days, the phlegm is made more fluid, and that lodg'd in them, is easily brought up by the coughing, which could not dislodge it before.

And, in fevers, that separation in the urine, which physicians look upon as a good sign of coction, seems to be produc'd by some part of the peccant matter, that, beginning to be separated from the blood, mixes with the urine, and is not usually distinguish'd from it, whilst this liquor is warm; but when grown cold, on account of its weight, or texture, appears in a distinct form; as of a cloud, sediment, &c.

But, whatever they mean by coction, 'tis plain that, on many occasions, nature waits not for it, but unseasonably, and often with danger, attempts to throw off the matter that offends her, before it be duly prepar'd for expulsion.

'Tis a circumstance of crises, thought the most wonderful, that nature often, by very unusual ways, and at unexpected places, discharges the matter that offends her, and thereby either cures, or notably relieves the patient.

But, setting aside extraordinary cases, it seems probable, that the performances of nature, in common crises, are, in some measure, referable to the particular condition of the matter to be expell'd, but principally to a peculiar disposition in the primitive fabric of some parts of the patient's body; or some unusual change made in the construction of these parts by the disease itself, or

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other accidents : which original, or adventitious disposition being invisibl to us, at least whilst the patient lives, we are apt to ascribe the unexpected accidents of a crisis, if it prove salutary, to the wonderful providence nature ; and if it happen otherwise, to overlook them. Plentiful evacuations, procured by medicines, are a kind of artificial crises. Some bodies, we see, are so constituted, that tho' the peccant humour, wrought on by the medicine, ought, as the physician thinks, to be expell'd by stool, in the generality of those who take rhubarb or senna ; yet the peculiar disposition of the patient's stomach will make that an emetic, which was intended for a cathartic. Nor does this constitution of the stomach equally regard all purging medicines ; for the same stomach that will reject them in the form of a potion, will quietly retain them, in that of pills. Thus mercury, which tho', when duly prepared, is usually given to procure salivation, especially to succulent bodies ; yet there are some patients, wherein, instead of salivating, 'twill work violently downwards, like a purge ; or make some other unexpected evacuation. And I have seen a patient, who, tho' young and very fat, could not be brought to salivate, either by the gentler ways ; by turbith-mineral, or other harsher medicines, tho' administred by very skilful physicians and surgeons.

And this peculiarity may be as well contracted, as native : for some persons, especially after surfeits, having been ruffly dealt with, or, at least, tired out with a medicine of this or that kind of form, will afterwards nauseate and vomit up the like, tho' in other bodies it be very far from emetic.

We see, also, that sometimes sudorific medicines, instead of procuring sweat, prove briskly diuretic, and sometimes either purging or vomitive.

From all this we may argue, that the qualities of the irritating matter, and much more the particular disposition of the patient's body, may procure evacuations at unexpected places.

There are also instances of periodical and critical evacuations, at very inconvenient, and unusual vents. Thus some women are recorded to have had their menses, sometimes at the eyes, sometimes at the navel, sometimes at the mouth, &c. of which there seems no cause so probable, as some peculiar structure, whether native or adventitious, of the internal parts concern'd in that discharge : and of such unusual structures, anatomists must have seen many, since I myself have observ'd more than one or two. Now if these uncommon ways of disposing of the morbid matter, we real ways salutary to the patient, the argument grounded on them would have more weight : but tho' most men take notice of this sort of crises, only when they are lucky, yet an impartial observer shall often find, that ill-condition'd and hurtful ones, may be made by unusual and unexpected ways. And, in some translations of the morbid matter to distant and nobler parts, perhaps it will be as difficult to shew, by what channels or known ways the matter pass'd from one to another, as 'tis to determine, how it was conducted to the parts that most happily vented it.

'Tis

'Tis my settled opinion, that divine prudence is, at least, often concern'd, in a peculiar manner, about the actions of men, and the things that happen to them, or has a necessary connexion with the one, the other, or both. And tho' I think it probable, that in the conduct of that far greatest part of the universe, which is merely corporeal, the wise author of it seldom manifestly procures a recess from the settled course of the universe, and especially from the most general laws of motion; yet, where men, who are creatures, that he is pleas'd to endow with free-will (at least with regard to things not spiritual) are nearly and highly concern'd; he may, not only sometimes by those signal and manifest interpositions we call miracles, acted in a supernatural way; but, as the sovereign lord and governour of the world, frequently give, by the intervention of rational minds, as well united, as not united to human bodies, several such determinations to the motion of parts in those bodies, and of others, which may be affected by them, as by laws merely mechanical, those parts of matter would not have had: and by motions so determin'd, either salutary or fatal crises, and many other things, conducive to the welfare or detriment of men, may be produced.

The interposition of divine providence, in cases of life and death, might be easily shewn to christians, from several passages of scripture. And, I think, it becomes a christian philosopher, to admit in general, that God sometimes in a peculiar, tho' secret way, interposes in the ordinary phenomena and events of crises; but that this is done so seldom, at least in a way that we can certainly discern, that we are not hastily to have recourse to an extraordinary providence, and much less to the strange care and skill of that question'd Being call'd nature, in a particular case, if it may be probably accounted for by mechanical laws, and the ordinary course of things.

When we consider the world, and the physical changes that happen in it, with regard to the divine wisdom and providence; the arguments for the affirmative ought, in their kind, to have more force than those for the negative. For it seems more allowable, to argue a providence from the exquisite structure and symmetry of the mundane bodies, and the apt subordination and train of causes, than to infer from some physical irregularities, that things are not fram'd and administered by a wise author and governour. For the characters and impressions of wisdom, conspicuous in the curious fabric, and orderly train of things, can, with no probability, be referr'd to blind chance, but to a most intelligent and designing agent. Whereas, on the other hand, the irregularities we speak of, are incomparably fewer than those things which are regular, and produc'd in an orderly way; and the divine maker of the universe being a most free agent, and having an intellect infinitely superiour to ours, may, in the production of seemingly irregular phenomena, have ends unknown to us, which even these irregularities may be very fit to compass.

*Axioms about
nature, how far,
and in what
sense to be ad-
mitted.*

TO proceed, the most general and current axioms concerning nature, employ'd in the writings of philosophers, may have a fair account given of them, agreeably to the doctrine hitherto propos'd; tho' some of these axioms suppose, and others seem strongly to support the receiv'd notion of nature.

But before I consider the particular maxims receiv'd concerning nature's actions, let us see in what sense nature may, or may not, be said to act at all, or to do this or that.

For ought I can clearly discern, whatsoever is perform'd in the merely material world, is really done by particular bodies, acting according to the laws of motion, &c. settled and maintain'd by God, among things corporeal. In which hypothesis, nature seems rather a notional thing, than a true physical, distinct efficient; such as would be, in case *Aristotle's* doctrine were true, one of those intelligences he presum'd to be the movers of the celestial orbs. But men often express themselves so very ambiguously, or intricately, when they say, that nature does this, or acts thus, that 'tis scarce possible intelligibly to translate their expressions into any forms of speech, adequate to the originals.

1. In this axiom, every nature preserves it self; by the word nature, I suppose, is understood a natural body, otherwise I know not what it means. Thus, then, it may either signify, that no one body tends to its own destruction, or that, in every body, there is a principle call'd nature, upon account whereof the body is vigilant and industrious to preserve its state, and to defend itself from the violence and attempts of all other bodies that oppose, or endeavour to destroy or harm it.

In the former sense, the axiom may be admitted, without any prejudice to our doctrine: according to which, inanimate bodies can have neither appetites, hatreds, nor designs; so that we may easily grant, they have none to destroy themselves.

But, according to the other sense of the axiom, 'twill import, that every body has within it self a principle, whereby it desires, and with all its power endeavours to compass its own preservation; and both to do those things, that tend thereto, and oppose all endeavours, that outward agents, or internal distempers, may use to compass the destruction of it.

And, as this is the most vulgar sense of the axiom, so 'tis chiefly in this, that I am concern'd to examine it.

I conceive, then, that the most wise creator of things at first so framed the world, and settled such laws of motion between the bodies, which, as parts, compose it; that by the assistance of his general concurrence, the portions of the universe, are lodg'd in such places, and furnish'd with such powers, that, by the help of his general providence, they may have their beings continued and maintained, as long as the course he thought fit to establish amongst things corporeal, requires.

Upon

Upon this supposition, which is but reasonable, there will appear no necessity to have any recourse, for the preservation of particular bodies, to such an internal appetite and in-bred knowledge in each of them, as the naturists presume: since, by virtue of the original frame of things, and established laws of motion, bodies are necessarily determined to act on such occasions, after the manner they would, if they had really an aim at self-preservation.

Thus, 'tis all one to a lump of dough, whether you make it into a round loaf, a long roll, or a flat cake: for in whatever figure the hands leave it, that it retains without having any appetite to return to the former. The mariner's needle, before it is excited, may have no particular propensity to one part of the heavens more than another; but when it has been duly touch'd upon a load-stone, the flower-de-luce will be determin'd to the north, and the opposite extreme to the south. So that, if the lily be drawn aside, towards the east or the west, as soon as the force that detain'd it, is remov'd, it will return to its former position, and never rest, till it regard the north. But, in spite of this seeming affection of the lily to that point of the horizon, yet, if the needle be duly touch'd upon the contrary pole of a vigorous load-stone, the lily will presently lose its former inclination, and regard the south part of the heavens.

2. Another received axiom concerning nature, is, that she never fails of her end. This proposition is very ambiguous. However, if by nature we here understand the Being, that the school-men style *Natura naturans*; I allow that nature never misseth its end. For the omniscient and almighty author of things having once framed the world, and establish'd in it the laws of motion, which he constantly maintains, there can no irregularity happen, especially among the greater mundane bodies, that he did not from the beginning foresee and permit. And tho', on some special occasions, this instituted order, either seemingly or really, has been violated; yet these things happening but rarely, and for weighty ends and purposes, by the peculiar intervention of the first cause, either guiding or over-ruling the propensities and motions of secondary agents; it cannot be said, that God is frustrated of his ends by these exorbitances, whereby he most wisely and effectually accomplishes them. But, if by nature be meant such a subordinate principle, as men usually understand by that name, I doubt the axiom is in many cases false: for tho' the material world is so constituted, that, for the most part, things are brought to pass by corporeal agents, as regularly, as if they designed the effects they produce; yet there are several instances wherein things happen quite otherwise.

Thus when a woman is pregnant, the aim of nature is, to produce a perfect human foetus; yet we often see, nature widely missing her mark, instead of that, produces a monster. Thus the sap, that nature raises with intent to feed the fruit of a white-thorn, for instance, is by grafting brought to nourish a fruit of quite another kind. So, when the malster makes barley to sprout, whence nature intends to produce stalks and ears, 'tis perverted to a very different purpose.

3. Another

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3. Another celebrated axiom concerning nature, is, that she always acts by the shortest ways. But this rule, as well as several others, requires to be explained and limited, before it be admitted. 'Tis true, the omniscient author of the universe has so framed it, that most of its parts act as regularly in order to the ends proposed, as if they did it with design. But since inanimate bodies have no knowledge, it cannot reasonably be supposed, that they moderate and vary their own actions, according to the exigency of particular circumstances, wherewith they must of necessity be unacquainted; it were therefore strange, if various occurrences did not determine them to act by other than the shortest ways, that lead to particular ends, if those other ways be more agreeable to the general laws or customs established among things corporeal. This I prove by instances taken from gravity itself, a quality perhaps very probably refer'd to an innate power and propension. For if a heavy body be let fall into the free air, 'twill take its course directly towards the centre of the earth; and, if it meet with an inclining plane, which puts it out of its way, it will not lose its tendency towards the centre, but run along that plane, by which means its tendency downwards is prosecuted, tho' not, as before, in a perpendicular line, yet in the shortest way it is permitted to take. These obvious phenomena, I confess, agree very well with the vulgar axiom, and possibly were the chief things that induced men to frame it. But now let us suppose, that a little sphere of marble or steel, after having long fallen thro' the air, lights upon a pavement of hard stone, that lies horizontal; in this case, experience shews, that the falling globe will rebound to a considerable height, and falling down again, rebound again, and so for several times successively, before it approaches as near as is permitted it, to the centre of heavy bodies. But if nature acted in all cases by the shortest ways, this sphere ought not to rebound at all. And having taken a good sea-compass, and suffer'd the magnetic needle to rest north and south; if I held the proper pole of a good load-stone at a convenient distance, on the right or left hand of the lily, this would be drawn aside from the north point towards the east or west, as I pleased; and then the load-stone being quite remov'd, the lily of the needle would indeed return northward, tho' not stop in the magnetic meridian, but passing on several degrees beyond it, 'twould thence return without stopping at the meridian line: and so, by its vibrations, describe many arches still shorter and shorter, till at length it came to settle on it, and recover that position, which, if nature always acted by the most compendious ways, it should have rested at the first time it had regain'd it. The truth is, that, at least, inanimate bodies, acting without knowledge or design of their own, cannot stop or moderate their own actions, but must necessarily move as they are determined by the general laws of motion; according to which, in one case, the impetus, that a body acquires by falling, is more powerful to carry it on beyond the line of direction, than the action of the causes of gravity is to stop it, as soon as it comes to the nearest place they can give it to the centre of the earth. And something like this happens in levity, as well as gravity;

gravity; for if you take an oblong and conveniently shaped piece of light wood, and having sunk it to the bottom of deep stagnant water, give it liberty to ascend, it will not only regain the surface of the water, where, by the laws of gravity, it ought to rest, and did rest before it was forc'd down; but, rise far beyond that surface, and in part, as it were, shoot itself up into the incumbent air, then fall down again, and rise a second time, and perhaps much oftner, and fall again, before it settles in its due place, wherein it is in an equilibrium with the water, that endeavours to press it upwards.

4. Another maxim, generally receiv'd concerning nature, is, that she always does what is best. But of this it will not be safe to deliver an opinion, till I have remov'd the ambiguity of the words; for they easily admit of two different senses. They may signify, that nature, in the whole universe, does always that which is best for the preservation of it in its present state; or that, with regard to each body in particular, nature still does what most conduces to the preservation and welfare of that body. In the first of these senses, the axiom will be less liable to exception; but then, I fear, it will be difficult to be positively made out, by such instances as prove that nature acts otherwise than necessarily according to mechanical laws. And, therefore, till I meet with such proofs, I shall proceed to the other sense; which, tho' the most usual, I cannot admit, without it be both explain'd and limited. I readily grant, as I have often occasion to repeat, that the all-wise author of things corporeal has so framed the world, that most things happen in it, as if the particular bodies that compose it, were watchful both for their own welfare, and that of the universe. But I think, withal, that particular bodies, at least such as are inanimate, acting without either knowledge or design, their actions tend not to what is best for them in their private capacities, any further than suits with the general laws of motion, and the important customs establish'd among things corporeal: so that, in conformity hereto, several things are done, that are neither the best, nor so much as good, with regard to the welfare of particular bodies.

We often see, that fruit-trees, especially when they grow old, will, for one season, be so overcharg'd with fruit, that they decay and die soon after; and even, whilst they flourish, the excessive weight upon them, sometimes breaks off the branches, and thereby both hinders the maturity of the fruit, and hastens the death of the tree. Now this fatal profusion would have been prevented, if a wise nature, harbour'd in the plant, did, as is presumed, sollicitously watch for its welfare.

We see, also, in several diseases, and in the unseasonable and hurtful crises of fevers, how far, what men call nature, often is from doing that which is best for the patient's preservation. In many diseases, a great part of the physician's work is to appease the fury, and to correct the errors of this pretended nature; which being, as 'twere, transported with a blind and impetuous passion, unseasonably produces those dangerous

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disorders in the body, that, if she were wise and watchful of its welfare, she would have been as careful to prevent, as the physician to remedy.

And if nature be so provident and watchful for the good of men, and other animals, and of that part of the world wherein they live; how comes she, from time to time, to destroy such multitudes of men and beasts, by earth-quakes, pestilences, famine, &c.

5. The word *Vacuum* being ambiguous, and used in differing senses; 'tis requisite, before I declare my opinion about the receiv'd axiom of the schools, that nature abhors a *Vacuum*, to premise the chief acceptations, in which I have observ'd the term *Vacuum* to be used for it has sometimes a vulgar, and sometimes a philosophical, or strict signification. In common speech, to be empty, usually denotes, not to be destitute of all body whatsoever; but of that body men suppose should be in the thing spoken of, or of that which it was framed, or design'd to contain. Thus, we say, a purse is empty, if there be no money in it; or a bladder empty, when the air is squeez'd out, &c. The word *Vacuum* is, also, taken in another sense, by philosophers who speak strictly, when they mean by it a space within the world, wherein there is not contain'd any body whatsoever.

Now the chief, if not the only, reason that moves the generality of philosophers to believe, nature abhors a *Vacuum*, is, that, in some cases, they observe an unusual endeavour, and, perhaps, a forcible motion in water, and other bodies, to oppose a *Vacuum*. But I am not apt, without absolute necessity, to ascribe to inanimate, and senseless, bodies, the appetites and hatreds that belong to rational, or sensitive, Beings; and, therefore, think it a sufficient reason to decline employing such improper causes, if, without them, the motions, ascribed thereto, can be accounted for.

If the *Cartesian* notion of the essence of a body, consisting in three dimensions, be admitted, it can scarce be deny'd, that nature does not produce these great, and irregular efforts to hinder a *Vacuum*; since, it being impossible there should be any, 'twere a fond thing to suppose, that nature, who is represented to us as a most wise agent, should take pains, and do extravagant things, to prevent an impossible mischief.

If the atomical hypothesis be admitted, it must be granted, that nature is so far from abhorring a *Vacuum*, that a great part of the things she does, require it; since they are brought to pass by local motion; and there are very many cases, wherein, according to these philosophers, the necessary motions of bodies cannot be perform'd, unless the corpuscles, that lie in their way, have little empty spaces to retire, or be impell'd into; when the body, that impels them, endeavours to displace them. Whence the axiom, that nature abhors a *Vacuum*, agrees with neither of the two great sects of the modern philosophers.

But, for ought appears by the phenomena employ'd to demonstrate nature's abhorrence of a *Vacuum*; either nature does not abhor a *Vacuum*, even when she seems solicitous to hinder it; or, she has but a very moderate hatred of it, in that sense wherein the vulgar philosophers take the word *Vacuum*.

For,

For, in almost all visible bodies here below, and even in the atmosphere itself, there is more or less of gravity, or tendency towards the centre of our terraqueous globe; whence nature need not disquiet herself, and act irregularly, to hinder a *Vacuum*: since, without her abhorrence of it, it may be prevented or replenished, by her affecting to place all heavy bodies as near the centre of the earth, as heavier than they will permit. And even, without any design of her's, a vacuity will be as much oppos'd, as we really find it, by the gravity of most, if not of all bodies here below, and the fluxility of liquors. For, by virtue of their gravity, and the minuteness of their parts, they will be determin'd to insinuate themselves into, and fill all the spaces, that they find not already possessed by other bodies, either more ponderous, *in specie*, than themselves, or, by reason of their firmness of structure, capable of resisting, or hindering, their descent. Accordingly, we observe, that, where there is no danger of a *Vacuum*, bodies may move, as they do, when they are said to endeavour its prevention. Thus, if you would thrust your hand deep into a vessel of sand, and afterwards draw it out again; there will need nothing but the gravity of the sand to make it fill up the greatest part of the space deserted by the hand. And if the vessel be replenished, instead of sand, with an aggregate of corpuscles more minute and smooth than the grains of that, as, for instance, with quick-silver, or with water; then the space, deserted by the hand, will be, to sense, compleatly fill'd by the corpuscles of the fluid. And if you take a glass-pipe, whose cavity is too narrow to let water and quick-silver pass by one another in it; and having lodg'd a small cylinder of mercury, of about half an inch long, in the lower part, you carefully stop the upper orifice with your finger, the quick-silver will remain suspended in the pipe: and if, then, you thrust the tube directly downwards, into a deep glass of water, till the quick-silver be depressed about a foot beneath the surface of the water; and then take off your finger from the orifice of the pipe, the quick-silver will, immediately, ascend swiftly, for five or six inches, and remain suspended at this new station. Here, therefore, we have a sudden ascent, of so heavy a body as is quick-silver, and a suspension of it in the glass, not produced to prevent, or fill a *Vacuum*; for the pipe was open at both ends: the phenomena being but genuine consequences of the laws of the equilibrium of liquors.

Considering how great a power the school-philosophers ascribe to nature, I am the less inclined to think her abhorrence of a *Vacuum* so great as they imagin'd. I have shewn, that her aversion to, and watchfulness against it, are not so vehement, but that, in the sense of the *Peripatetics*, she can quietly admit it, in some cases; where, with a very small endeavour, she might prevent, or replenish it. When the *Torricellian* experiment is made, tho' it cannot, perhaps, be fully prov'd, either against the *Cartesians*, or some other *Plenists*, that, in the upper part of the tube, deserted by the quick-silver, there is a *Vacuum*, in the strict philosophical sense of the word; yet, as the *Peripatetics* declare their sense, by their reasonings

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against a *Vacuum*, 'twill appear very hard for them to shew, there is not one in that tube. And, as by the scholastic way of arguing, nature's hatred of a *Vacuum*, from the suspension of water, and other liquors, in tubes and conical watering-pots, it appears, they thought that any space here below, deserted by a visible body, not succeeded by another visible body, or at least by common air, may be reputed empty; so, the space deserted by the quick-silver, at the top of of a barometer, thirty-one inches long, will invite one to doubt, whether a *Vacuum* ought to be thought so formidable a thing to nature, as they imagine she thinks it? For, what mischief would ensue to the universe, upon the production or continuance of such a *Vacuum*, tho' the deserted space were much more than an inch, and continued many years, as has several times happen'd in the taller sort of mercurial barometers? And those *Peripatetics* who tell us, if there were a *Vacuum*, the influences of the celestial bodies, that are absolutely necessary to the preservation of the sublunary ones, would be intercepted, since motion cannot be made *in vacuo*, would do well to prove such a necessity; and to consider, that in our case the top of the quick-silver, to which the *Vacuum* reaches, usually appears protuberant; which shews, that the beams of light are able to pass thro' that *Vacuum*, being, in spite of it, reflected from the *Mercury* to the eye. And in such a *Vacuum*, as to common air, I have try'd, that a load-stone will emit its effluvia, and move iron or steel placed therein.

In short, it is not evident, that, here below, nature so much endeavours to hinder or fill up a *Vacuum*, as to manifest an abhorrence of it: and, without much peculiar solicitude, a *Vacuum*, at least a philosophical one, is as much provided against, as the welfare of the universe requires, by gravity and the fluxility of the liquors, and other bodies.

6. I come now to the celebrated saying, that nature cures diseases, taken from *Hippocrates*, who expresses it in the plural, *νασάν φυσίς ἰσπρί*. And because this axiom is generally receiv'd among physicians and philosophers, and seems to be one of the principal things, that has made them introduce such a Being as they call nature; I shall attentively consider, in what sense, and how far, this famous axiom may, or should not, be admitted.

First, then, I conceive it may be taken in a negative sense, so as to import, that diseases cannot be cured in such persons, in whom the aggregate of the vital powers, or faculties of the body, is so far weaken'd or deprav'd, as to be utterly unable to perform the functions necessary to life; or at least to actuate and assist the remedies employ'd by the physician, to preserve or recover the patient.

This I take to be the meaning of such usual phrases as these, *Physic comes too late. Nature is quite spent, &c.* And in this sense I readily acknowledge the axiom to be true. For, where the engine has some necessary parts, whether fluid or solid, so far deprav'd or weakned, as to render it altogether unable to co-operate with the medicine, it cannot be rationally expected, that the administration of that medicine, should prove effectual. But there

there is a positive sense of our axiom, wherein it is the most usually employ'd ; for men commonly believe there resides in the body of a sick person, a certain provident or watchful Being, that industriously employs itself, by its own endeavours, as well as by any occasional assistance, to rectify whatever is amiss, and restore the distemper'd body to its pristine state of health.

Now, I conceive that the wise and beneficent maker of the world, intending that men should, for the most part, live a considerable number of years, in a condition to act the parts assign'd them ; he was pleas'd to frame human bodies so, that, with the ordinary succours of reason, &c. they may in many cases recover a state of health, if they chance to be put out of it by less accidents than those, that God, in compliance with the great ends of his general providence, did not think fit to secure them from, or enable them to surmount. Many things, therefore, that are commonly ascrib'd to nature, may be better ascribed to the mechanism of the universe, and the human body.

The causes that disorder the human frame, are often transient, but the structure of the body it self, and the causes that conduce to the preservation of its structure, are more stable and durable ; and, on that account, may enable the engine to out-last many things that are unfriendly to it, as slight hurts, and diseases.

Several phenomena of diseases may be illustrated, by supposing dirt thrown into a vial of fair water, and then the vial to be well shaken ; whereby the water will lose its transparency, upon a double account ; that of the dirt, whose opaque particles are confounded with it ; and that of the bubbles, which swim at the top of it : yet to purify this water, and make it recover its former transparency, there needs no particular care or design of nature ; but, according to the common course of things, after some time the bubbles will break and vanish at the top, and the earthy particles, that compose the mud, will, by their gravity, subside to the bottom, and settle there, whence the water becomes clear again.

There is indeed one thing, to which the maxim, *Nature cures diseases*, may be very speciously applied ; and that is the healing wounds ; which, if but in the flesh, may often be cured without medicines. However, this healing seems to be only an effect or consequent of that fabric of the body, on which nutrition depends. For the alimental juice being, by the circulation of the blood and chyle, carried to all parts of the body ; if it meets any where, either with preternatural concretions, or with a gap made by a cut or wound, its particles there concrete into a kind of flesh, or other body, which that juice, in the place and other circumstances 'tis in, is fitted to constitute. Thus not only wens and scrophulous tumors are nourish'd in the body, but mis-shapen mola's grow in the womb, as well as embryo's. And we see, in wounds, that fungus's are as well produced and nourish'd by the aliment brought to the wounded part, as the true and genuine flesh ; so that either nature seems much mistaken, if she designs the production and maintenance of such superfluous and inconvenient bodies ;

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But there are many diseases, as well acute as chronical, wherein, 'tis confess'd, that nature alone does not work the cure ; so that the aphorism, which makes nature the curer of these, is not true, but in a limited sense. 'Tis, indeed, pretended, that even here, nature is the principal agent, by whose direction the physician acts in subservience to her designs ; and physicians themselves acknowledge, that they are but nature's ministers. Let us, therefore, see in what sense it is fit, according to our doctrine, to admit these assertions.

One great cause of the common mistakes about this matter, is, that the body of a man hath been look'd upon, rather as a system of parts, whereof most are gross and consistent, and not a few hard and solid too, than as, what indeed it is, a very compounded engine ; that, besides these parts, consists of the blood, chyle, gall, and other liquors ; and of more subtle fluids, as spirits and air : all which are incessantly and variously moved, and thereby put several of the solid parts into frequent and differing motions. When, therefore, the constitution, or the motions, that in a sound body, regularly belong to the fluids, happen, the former to be deprav'd, or the latter to grow irregular ; the engine is immediately out of order, tho' the gross solid parts were not primarily affected : so, when by proper remedies the vitiated texture of the blood, or other juices, is corrected, and the inordinate motions of them and the spirits are calm'd and rectify'd, the grosser and more solid parts of the body, and so the whole animal œconomy, will be restored to a more convenient state.

The physician, in my opinion, is to look upon his patient's body, as an engine out of order ; yet so constituted, that, by concurring with the endeavours, or tendencies, of the parts of the machine, it may be brought to a better state. If, therefore, he find, that, in the present disposition of the body, there is a propensity to throw off the matter that offends it, in a convenient way, and at commodious places ; he will then act so, as to comply with, and further that way of discharge, rather than another. As, if there be a great appearance, that a disease will quickly have a crisis by sweat ; he will rather further it by covering the patient with warm clothes and giving sudorific medicines ; than, by endeavouring to carry off the peccant matter by purging or vomiting, unseasonably hinder a discharge, that probably will be beneficial. And, in this sense, men may say, if they please, that the physicians are ministers or servants of nature ; as sea-men, when the ship sails before a good wind, will not shift their sails, nor alter the ship's motion, because they need not. But to shew, that

that 'tis, as 'twere, by accident, that the physician, in this case, obeys nature, (to speak in the language of the naturists;) there are many other cases, wherein the physician, if skilful, will be so far from taking nature for his guide, to direct him by her example; that a great part of his care and skill is employ'd, to hinder her from doing what she seems to design; and to bring to pass other things very differing from, if not contrary to, what she endeavours.

Thus, tho' nature, as 'tis call'd, importunately craves for drink in drop-sies, the physician thinks himself obliged to deny it; as he does what his patients greedily desire in the *Pica*, &c. Thus also the chirurgeon often hinders nature from closing up the lips of a wound, as she would unskilfully do, before it be well and securely heal'd at the bottom. So the physician, by purging or phlebotomy, carries off the matter, that nature would more dangerously throw upon the lungs.

In short, I look upon a good physician, not properly as a servant to nature, but a counsellor and a friendly assistant; who, in his patient's body, furthers these motions, and other things, that he judges conducive to the welfare and recovery of it: but as to those, that he perceives likely to be hurtful, either by increasing the disease, or otherwise endangering the patient, he thinks it his part to oppose or hinder, tho' nature manifestly seems to endeavour to exercise or carry on those hurtful motions.

Hence, I fear, the commendations which *Hippocrates* gives of nature, in the cure of diseases, make many physicians less courageous and careful than they should be, to employ their own skill, on several occasions that greatly require it.

As, in some cases, the physician relieves his patient in a negative way, by opposing nature in her unseasonable, or disorderly attempts; so, in others, he may do it in a positive one; by employing medicines that either strengthen the parts, or make sensible evacuations of matter, necessary to be proscribed by them; or, by using remedies, that by their manifest qualities oppose those of the morbid cause. And, perhaps, in some cases, the physician may, in a positive way, contribute more to the cure even of an inward disease, than nature herself seems able to do: for, if any such medicine may be prepared by art, as *Helmont* affirms to be attainable from *Paracelsus's Ludus*, by the alkahest; or, as *Cardan* relates, that an empiric had in his time, who travell'd up and down *Italy*, curing such where-ever he came, as were tormented with the stone of the bladder; the physician might, by such instruments, perform that which, for ought appears, is not to be done by nature herself; since we never find, that she dissolves a confirm'd stone in the bladder. Nay, sometimes, the physician does, even without the help of a medicine, control and over-rule this nature, to the great and sudden advantage of the patient, as in the case of swooning.

It appears from the whole, that as there are some phenomena, which seem to favour the doctrine of the naturists, about the cure of diseases; so there are others, that more manifestly agree with our hypothesis. And both these

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these sorts of phenomena, being consider'd together, may well suggest a suspicion, that the most wise and free author of things, having framed the first individuals of mankind, so as to be fit to last for many years, and endow'd those originals with the power of propagating their species; it thence comes to pass, that in the subsequent engines we call human bodies, when neither particular providence, nor the rational soul, nor over-ruling impediments interpose, things are generally perform'd according to mechanical laws and courses; whether the effects and events of these prove conducive to the welfare of the engine itself, or else cherish and foment extraneous bodies or causes, whose preservation and increase are hurtful to it. Hence the happy things, referr'd to nature's prudent care for the recovery and welfare of sick persons, are usually genuine consequences of the mechanism of the world, and the patient's body; which effects luckily happen to be co-incident with his recovery, rather than to have been purposely and wisely produced in order to it; since what is called nature, seems careful to produce, preserve, and cherish things hurtful to the body, as well as those beneficial to it.

Thus, not only worms, but frogs and toads, taken in their spawn, with corrupted water, have been cherish'd in the stomach, till the eggs being grown compleat animals, have produced horrid symptoms in the body. And if, according to the receiv'd opinions of physicians, stubborn quartans are produced by a melancholy humour seated in the spleen; it may be said, that nature seems to busy herself to convert some parts of the fluid chyle into so tenacious a juice, that, in many patients, notwithstanding the neighbourhood of the spleen and stomach, neither strong emetics, purges, nor other usual remedies, are able, in a long time, to dislodge, resolve, or correct it.

But the poison of a mad-dog, nature sometimes seems industriously to preserve; for we have instances of a little foam convey'd into the blood by a slight hurt, which, notwithstanding the constant heat and perspirable make of the human body, and the dissipable texture of the foam, so preserved, and sometimes, too, for many years, that, at the end of that time, it has broke out, and display'd its fatal efficacy with as much vigour and fury, as if it had but newly been receiv'd into the body.

Thus, tho' the quantity of poison, in the bite of the *Tarantula*, is scarce visible, being communicated by the tooth of so small an animal as a spider; yet, in many patients, 'tis preserv'd during a great part of their lives, and manifests its continuance in the body by annual paroxysms. And, a person of great quality, complain'd to me, that being in the *East*, the biting or stinging of a creature, whose offensive arms were so small, that the eye could very hardly discern the hurt; had so lasting an effect upon him, that, for about twelve years after, he was reminded of his mischance, by a pain he felt in the hurt place, about the same time of the year that the mischief was first done him. And, in some hereditary diseases, as the gout, the falling-sickness, &c. nature seems to act as if she did, with care and skill, transmit to the unhappy child such morbid seeds, or
impres-

impressions of the parents, that, in spite of all the various alterations the younger body passes thro', during the course of many years, this protected enemy is able to exert its power and malice, after forty or fifty years concealment.

In the same manner, but with light variations, other axioms about nature might be easily explain'd.

But tho' we could not intelligibly explain all the particular axioms about her, and the phenomena of inanimate bodies, that are thought to favour them, by mechanical principles; it would not follow, that we must therefore yield up the whole cause to the naturists. For we have already shewn, that the supposition of such a Being as they call nature, is far from enabling her patrons to give intelligible accounts of these and other phenomena of the universe. And tho' our doctrine should be insufficient to give a good account of things corporeal; yet a less degree of probability may serve in arguments employ'd but to justify a doubt, than is requir'd in those that are to demonstrate an assertion.

'Tis true, the naturists tell us, that the nature they assert is the principle of all motions and operations in bodies; which infers, that in explaining them, we must have recourse to her.

But before we acquiesce in, or confidently employ this principle, it were very fit we knew what it is: I, therefore, demand of those who assert such a nature as is vulgarly described, whether it be a substance, or an accident? If it be the latter, it should be declar'd, what kind of accident it is; how a solitary accident can have right to all those attributes, and produce those numerous and wonderful effects, that they ascribe to nature: and why a complex of such accidents, as are the mechanical affections of matter, may not altogether as probably as that accident they call nature, be conceiv'd to have been instituted by the perfectly wise author of the universe, to produce those changes among bodies, which are intelligibly referable to them. And if things be not brought to pass by their intervention, 'twere very fit we should be inform'd, by what other particular and intelligible means nature can effect them better.

But if it be said, as by most it is, that the principle call'd nature, is a substance; I demand, whether it be corporeal or immaterial? If it be immaterial, I further ask, whether it be created, or not? If not, we have God under another name, and our dispute is at an end, by the removal of its subject; which is said by the schools, to be God's vicegerent, not God himself. But if nature be affirm'd (as she is by all christian philosophers) to be a created Being; I then demand, whether she be endow'd with understanding, so as to know what she does; for what ends, and by what laws she ought to act? If the answer be negative, the supposition of nature will be of very little use to afford an intelligible account of things. And if it should be said, that nature is endowed with understanding, and performs such functions as many of the ancients ascribe to the soul of the world; this hypothesis is near of kin to heathenism; and I do not think, that they who suppose a kind of soul of the universe, will

PHYSICS. find this principle sufficient to explain the phenomena of it. For tho' nature be admitted to have reason, yet a multitude of phenomena may be mechanically produced, without her immediate intervention; as, we see, that in man, tho' the rational soul has so narrow a province to take care of, as the human body, and is supposed to be intimately united to all the parts of it; yet abundance of things are done in the body, by the mechanism of it, without being produced by the soul. In sleep, the circulation of the blood, the regular pulsation of the heart, digestion, nutrition, respiration, &c. are perform'd without the immediate agency, or so much as the actual knowledge of the mind. And, when a man is awake, many things are done in his body, not only without the direction, but against the bent of his mind; as often happens in cramps, and other convulsions, coughing, yawning, &c. Nay, tho' some brutes, as, particularly, apes, have the structure of many parts of their bodies very like that of the correspondent ones in men; yet that admirable work of the formation and organization of the fœtus in them, is granted, by philosophers, to be made by the soul of the brute, which is neither an incorporeal, nor a rational substance. And even the human fœtus, if we admit the general opinion of philosophers, physicians, divines, and lawyers, is form'd without the intervention of the rational soul, that is not infused, till this hath obtain'd an organization, fitting to receive such a guest; which it is reputed to do, about the end of the sixth week, or before that of the seventh.

If it be urged, that nature, being the principle of motion in bodies; their various motions, which amount to a considerable part of their phenomena, must be explained, by having recourse to her: I answer, 'tis very difficult to conceive how a created, immaterial substance, can, by a physical power, move a body; the agent having no impenetrable part, wherewith to impel the corporeal *Mobile*. God, indeed, who is an immaterial spirit, ought to be acknowledg'd the primary cause of motion in matter; because motion necessarily belongs not to corporeal substances. But, then, there is that infinite distance between the incomprehensible creator, and the least imperfect order of his creatures, that we ought to be very cautious how we make parallels between them; and draw inferences from his power, and manner of acting, to theirs: since he, for instance, can immediately act upon human souls, as having created them; whilst they are not able so to act upon one another. And it seems the more difficult to conceive, that, if nature be an incorporeal substance, she should be the great mover of the mundane matter; because we see, that, in a human body, the rational soul, tho' vitally united to it, can only determine the motion of some of the parts; not give motion to any, or so much as regulate it in most. And, if nature be said to move bodies, in another than a physical way; I doubt, whether the supposition of such a principle will be of much use to philosophers, in explaining phenomena. I should scarce think him a judicious physician, who imagines, that he gives an intelligible and particular account of the surprizing symptoms of those

those strange diseases, that many impute to witchcraft, when he says, that their phenomena are produced by a wicked immaterial spirit, call'd a devil.

But, 'tis thought, that the greater number of philosophers, at least among the moderns, take nature to be corporeal: I, therefore, demand whether these philosophers believe nature, tho' corporeal, to act knowingly, that is, with consciousness of what she does, and for designed ends; or else to be blindly and necessarily moved and directed by a superior agent, endow'd with an excellent understanding?

The *Cartesians* would ask, how, if nature be a corporeal substance, we can conceive her capable of thinking; and of being a most wise and provident director of all the motions that are made in the corporeal world?

And a philosopher may justly demand, how a corporeal Being can so pervade the universe, as to be intimately present with all its minute parts, whereof 'tis said to be the principle of motion?

We may, also, demand, whence nature, being a material substance, comes itself to have motion; since motion does not belong to matter, in itself? For a body is as truly a body when it rests, as when it moves. If it be answer'd, that God, at first, put it into motion; I reply, that the same cause may as probably be supposed to have put the unquestion'd mundane matter into motion; without the intervention of another corporeal Being, in the conception whereof as matter, motion is not involv'd.

It may, likewise, be ask'd, how the laws of motion come to be observ'd, or maintain'd, by a corporeal Being, which, as merely such, is either incapable of understanding them, or of acting with respect to them; or, at least, is not necessarily endow'd with any knowledge of them, or power to conform to them, and to make all the parts of the unquestion'd mundane matter do so too?

And I see not, how the taking in such an unintelligible, and undesigning principle, will free our understandings from great difficulties, when we come to explain the phenomena of bodies. For, if nature be a bodily creature, and acts necessarily; I see no cause to look upon it as other than a kind of engine: and the difficulty may be as great, to conceive how all the several parts of this suppos'd engine, call'd nature, are, themselves, framed, and moved, by the great author of things, and how they act upon one another, as well as upon the undoubted mundane bodies; as 'tis to conceive, how, in the world itself, which is, manifestly, an admirably contriv'd automaton, the phenomena may, by the same author, be produced, in consequence of the primitive construction, and motions, that he gave it; without the concurrence of such a thing, as they call nature. For this, as well as the world, being a corporeal creature; we cannot conceive, that either of them act otherwise than mechanically. And it seems very suitable to the divine wisdom, to employ in the world, already framed and completed, the fewest, and most simple means, by which the phenomena, design'd to be exhibited, could be produced. Nor need we be much mov'd, to hear some naturists say, that nature, tho' not an in-

PHYSICS. corporeal Being, is of an order superior to mere matter. For, who can clearly conceive an order, or kind of Beings, that shall be real substances, and yet neither corporeal, nor immaterial? Nor do I see, how the supposition of this unintelligible, at least unintelligent Being, tho' we should grant it to have a kind of life, or soul, will much assist us to solve phenomena.

To draw, at length, towards a conclusion; what we have here deliver'd upon this subject, may, perhaps, do some service both to natural philosophy, and to religion.

The use and advantages of this inquiry.

We have attempted to dissuade philosophers from often employing, and without great need, a term, which, by reason of its great ambiguity, and the little care taken by such as use it, to distinguish its different acceptions, occasions both a great deal of darkness, and confusion, in what men say and write about things corporeal; and a multitude of controversies, wherein really they wrangle about words. And this discourse may, possibly, wean many from the fond conceit they cherish, that they understand, or explain a corporeal subject, or a phenomenon, when they ascribe it to nature. For, to do that, one need not be a philosopher; since a peasant may easily do the same.

A man has never well perform'd the part of a true philosopher, till he circumstantially, or particularly, deduces the phenomenon he considers, by intelligible ways, from intelligible principles; which he will be constantly put in mind of doing, or discover that he hath not done, if, by forbearing general and ambiguous terms, he endeavours to explain things, by expressions that are clear to all attentive readers. And this perspicuous way of philosophizing should be greatly recommended, by the valuable discoveries which those who employ'd it, have happily made, in hydrostatics, optics, anatomy, botanics, and many other parts of real learning.

Our doctrine may, also, be serviceable to religion in three respects.

And, first, it may keep many, who were inclined to have an excessive veneration for what they call nature, from running into those extravagant and sacrilegious errors, that have been, upon plausible pretences, embraced, not only by many of the old heathen philosophers, but by several modern professors of christianity.

Secondly, it may conduce to justify some remarkable proceedings of divine providence, against those who boldly censure it, upon the account of some things they judge to be physical irregularities; such as monsters, earth-quakes, floods, eruptions of vulcano's, famines, &c. For, according to us,

1. God is a most free agent, and created the world, not out of necessity, but voluntarily; having framed it as he pleased, and thought fit, at the beginning of things; when there was no substance but himself, and, consequently, no creature, to which he could be obliged, or by which he could be limited.

2. God having an understanding infinitely superior to that of man, in extent, clearness, and other excellencies; he may, rationally, be suppos'd to have framed so great and admirable an automaton as the world, and
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the subordinate engines comprized in it for several ends and purposes; some of them relating chiefly to his corporeal, and others to his rational creatures: of which ends, he hath vouchsafed to make some discoverable by our dim reason; whilst others are, probably, not to be penetrated by it, but lie conceal'd in the deep abyfs of his unsearchable wisdom.

3. It seems probable, that this most excellent and glorious Being thought fit to order things so, that both his works and actions might bear some marks of his attributes; and, especially, to stamp upon his corporeal works some tokens, or impresses, of his divine wisdom, discernible by the human mind.

4. Upon this supposition, it became the divine author of the universe to give it such a structure, and such powers, and to establish among its parts such general and constant laws, as best suited his purposes in creating the world; and to give these universal laws, and particular parts, or bodies, such subordinations to one another, and such references to the original fabric of the grand system of the world, that, on all particular occasions, the welfare of inferior, or private portions of it, should be only so far provided for, as their welfare is consistent with the general laws by him settled in the universe; and with such of those ends, that he propos'd to himself in framing it, as are more considerable than the welfare of those particular creatures.

5. The last service that, I hope, our doctrine may do religion, is, to induce men to pay their admiration, their praises, and their thanks, directly to God himself; who is the true and only-creator of the sun, moon, earth, and those other creatures, that men call the works of nature. And in this way of expressing their veneration of the true God, and their gratitude to him, they are warranted by the examples of the ancient people of God, the *Israelites*; and not only by the inspired persons of the old testament, but by the promulgators of the new, and even by the celestial spirits; who, in the Revelation of St. *John*, are introduced praising and thanking God himself for his works; without taking any notice of his pretended vicegerent, nature.



A N
I N Q U I R Y
I N T O T H E
F I N A L C A U S E S
O F
Natural Things.

S E C T. I.

I Here propose to deliver my thoughts upon these four questions.
1. Whether, indefinitely speaking, there be any final causes of things corporeal, knowable to men ? 2. Whether, if that question be resolv'd in the affirmative, we may consider final causes in all sorts of bodies, or only in such as are peculiarly qualified ? 3. Whether, or in what sense, the acting for ends may be ascribed to an unintelligent and inanimate body ? 4. And lastly, how far, and with what cautions, arguments may be framed upon the supposition of final causes ?

Whether the final causes of natural things are knowable to men ?

To begin with the first question. Those who would exclude final causes from the consideration of the naturalist, seem to do it, either because, with *Epicurus*, they think the world was produc'd by atoms and chance, without the intervention of a Deity ; and, consequently, that 'tis in vain to seek for such causes: or because, with *Des Cartes*, they imagine, that God being omniscient, 'tis rash and presumptuous for men to think they know, or can discover what ends he propos'd to himself, in his creatures. The supposition on which the *Epicureans* have rejected final causes, has been disallow'd by the philosophers of almost all other sects ; and some have written sufficient confutations of it : I shall, however, in the progress of our work,

work, make such occasional observations, as may serve to discredit so unreasonable an opinion. But the *Cartesian* argument having been so prevalent among learned and ingenious men, I shall bestow the more pains in the consideration of that.

One thing, perhaps, that kept so great a philosopher as *Des Cartes*, from allowing the consideration of final causes in physics, was, that the school-philosophers, and others, are apt to propose it too unwarily; as if there were no creature in the world, that was not solely, or chiefly design'd for the service of man. And, indeed, I have seen a body of divinity, publish'd by a famous author, wherein he urges this argument for the annihilation of the world; "That since the world was made for the sake of man, in his travelling capacity; when once man is possess'd of his everlasting state of happiness, or misery, there will be no further use of the world." The opinion that gives rise to such presumptuous and unwarrantable expressions, was, as I conjecture from his objection, very shocking to *Des Cartes*; but the indiscretion of men ought not to prejudice truth, which is not to be thrown away with the groundless conceits that some people have pinn'd upon it.

Since then, I cannot close in either with the doctrine of the *Epicureans*, or *Cartesians*, I shall leave each party to maintain its respective opinion, and proceed to declare my own: but, to clear the way, 'tis necessary to premise a distinction.

In speaking of the ends which the author of nature is said to have in things corporeal, any of these four particulars may be signified. *Final causes, what they may signify.*

First, Some grand and general ends of the universe; such as exercising and displaying the creator's wisdom; the communication of his goodness, and the admiration and thanks due to him from his intelligent creatures, &c. And these ends, because they regard the whole creation, I call the universal ends of God, or nature.

Secondly, In a more restrain'd sense, the ends design'd in the number, fabric, situation, and motion of great masses of matter, that make large parts of the world; since 'tis very probable, that these bodies, such as the sun, moon, fix'd stars, and the terraqueous globe, were so framed, and placed, as not only to persevere in their own present state, but also to conduce to the universal ends of the creation, and the good of the whole, whereof they are considerable parts. Upon which accounts, these ends may be call'd cosmical, or systematical; as they regard the symmetry of the great system of the world.

Thirdly, ends that more peculiarly concern the parts of animals, and, perhaps, plants too; or those to which the particular parts of animals are destined for the welfare of the whole creature, consider'd as an entire and distinct system of organiz'd parts, design'd to preserve himself, and propagate his species, upon that stage to which his structure, and circumstances determine him to act his part. And these ends, to distinguish them from others, may be call'd animal ends.

Lastly,

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Lastly, The same expression may signify another sort of ends, which, because they relate particularly to man, may be called human ends; and are those aimed at by nature, where she is said to frame animals, vegetables, &c. for the use of man. And these ends may farther be distinguish'd into mental and corporeal; not only as man is an animal framed, like others, for his own preservation, and the propagation of his species; but also, as he is made to have dominion over other animals, and works of nature, and fitted to make them subservient to his purposes. This distinction being thus settled, I declare my dissent, as well from the vulgar notion of final causes, which allows of none but those we have call'd human ones; as from theirs who wholly reject them all.

'Tis an acknowledg'd principle of the *Cartesian* philosophy, that there is always the same quantity of motion in the world*; because, say they, there is no reason why God, who is immutable, should, at the beginning of things, have given such a quantity of motion to matter, as should need to be afterwards augmented, or lessen'd. But do not those who employ this negative argument, take upon them to judge of the ends that God may have propos'd to himself in natural things? For, without supposing that they know what God design'd in setting matter in motion, 'tis hard for them to shew, that his design could not be best accomplish'd by sometimes adding to, and sometimes taking from the quantity of motion, he originally

* Sir *Is. Newton* makes it evident, that there is not always the same quantity of motion in the world. "The *vis inertia*, says that great philosopher, is a passive principle, by which bodies persist in their motion, or rest; receive motion in proportion to the force impressing it, and resist as much as they are resisted. By this principle, alone, there could never have been any motion in the world. Some other principle was necessary for putting bodies into motion; and now they are in motion, some other principle is necessary for conserving the motion. For, from the various composition of two motions, 'tis very certain, there is not always the same quantity of motion in the world. For, if two globes join'd by a slender rod, revolve about their common centre of gravity, with an uniform motion, while that centre moves on uniformly, in a right line, drawn in the plain of their circular motion; the sum of the motions of the two globes, as often as the globes are in a right line, described by their common centre of gravity, will be bigger than the sum of their motions,

"when they are in a line perpendicular to that right line. By this instance it appears, that motion may be got or lost. But by reason of the tenacity of fluids, and attrition of their parts, and the weakness of elasticity in solids, motion is much more apt to be lost, than got; and is always upon the decay. For, bodies which are either absolutely hard, or so soft as to be void of elasticity, will not rebound from one another. Impenetrability makes them only stop. If two equal bodies meet directly *in vacuo*, they will, by the laws of motion, stop where they meet, and lose all their motion, and remain at rest; unless they be elastic, and receive new motion from their spring. If they have so much elasticity as suffices to make them rebound with $\frac{1}{4}$, or $\frac{1}{2}$, or $\frac{3}{4}$ of the force with which they come together, they will lose $\frac{1}{4}$, or $\frac{1}{2}$, or $\frac{3}{4}$ of their motion. And this may be tried, by letting two equal pendulums fall against one another from equal heights. If the pendulums be of lead, or soft clay, they will lose all, or almost all their motions; if of elastic

originally communicated to matter. And, I think, it may be worth considering, whether by this doctrine of theirs, the *Cartesians* do not more take upon them, than other philosophers, to judge of God's designs. For, if a man be known for very wise, and to have various ways of compassing his several ends; he who, seeing some of those ways have a direct tendency to a rational end, shall conclude that end to be one of the number intended, thereby expresses more respect to that man, than he who should conclude, his end cannot be such; and that he has no other design knowable, except a certain general one. And, indeed, it seems more easy to know, that a particular end, for which an engine is proper, may, among others, be intended by the artificer, tho' never so skilful; than to know negatively, that he can have no other than such a certain end.

And how will a *Cartesian* be sure, that among the many ends he grants God may have proposed to himself, in the production of his mundane creatures, one is not, that we, whom he has made intelligent Beings, and capable of admiring and praising him, should find just cause to do so, for the wisdom and goodness he has display'd in the world? Which attributes we could not well discern, and celebrate, unless we knew that the creatures were made for such uses, and that they are exceedingly well fitted to them. God's immutability is, indeed, alledg'd to prove, that the quantity

" stic bodies, they will lose all but what
" they recover from their elasticity. If
" it be said, that they can lose no motion
" but what they communicate to other
" bodies, the consequence is, that in
" *vacuo* they can lose no motion; but,
" when they meet, must go on, and penetrate one another's dimensions. If three
" equal round vessels be fill'd, the one
" with water, the other with oil, and
" the third with melted pitch; and the
" liquors be stirred about alike, to give
" them a vortical motion; the pitch, by
" its tenacity, will lose its motion quickly;
" the oil, being less tenacious, will
" keep it longer; and the water, being
" less tenacious, will keep it longest; but
" yet will lose it in a short time. Whence
" it is easy to understand, that if many
" contiguous vortices of melted pitch,
" were each of them as large, as some
" suppose to revolve about the sun and
" fix'd stars; yet these, and all their
" parts, would, by their tenacity and stiffness, communicate their motion to one
" another, till they all rested among
" themselves. Vortices of oil, or water,
" or some fluid matter, might continue longer in motion; but, unless that
" matter were void of all tenacity, and attraction of parts, and communication

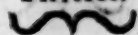
" motion, the motion would constantly
" decay. Seeing, therefore, the variety of motion which we find in the
" world, is always decreasing, there is
" a necessity of conserving and recruiting it by active principles; such as are
" the cause of gravity, by which planets
" and comets keep their motions in their
" orbs, and bodies acquire great motion
" in falling; and the cause of fermentation, by which the heart and blood of
" animals are kept in perpetual motion,
" and heat; the inward parts of the earth
" are constantly warm'd, and, in some
" places, grow very hot; bodies burn and
" shine; mountains take fire; the caverns of the earth are blown up; and
" the sun continues violently hot, and
" lucid, and warms all things by his light.
" For, we meet with very little motion
" in the world, besides what is owing to
" these active principles. And, if it were
" not for these principles, the bodies of
" the earth, planets, comets, sun, and
" all things in them, would grow cold,
" and freeze, and become inactive masses;
" and all putrefaction, generation, vegetation, and life, would cease; and the
" planets and comets would not remain
" in their orbs." *Newton. Optic. p. 372,*

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Final Causes of Natural Things.

of motion never varies; but to me 'tis not evident why God's having particular ends, tho' some of them seem to require a change in his way of acting in natural things, must be more inconsistent with his immutability, than his causing many things, decreed from eternity, to be brought to pass in process of time. And, particularly, it seems not clear, why God may not as well be immutable, tho' he should sometimes vary the quantity of motion in the world; as he is, tho', according to the opinion of most *Cartesians*, he daily creates multitudes of actuating rational souls, to unite them to lifeless human bodies. I say not this, as if I absolutely rejected the *Cartesian* doctrine about the continuance of the same quantity of motion in the whole mass of matter. For whether that be true or not, 'tis no unuseful, or improbable hypothesis; and I have not so much argued against that, as, upon the same grounds they argue for it.

To come then to the thing itself; M. *Des Cartes* objects, that 'tis a presumption in man to pretend he is able to discover the ends that the Omniscient proposed to himself in the making of his creatures. Now I consider, by way of answer, that there are two very different ways, wherein a man may pretend to know the ends of God in his visible works; for he may either pretend to know only some of God's ends in some of his works, or he may pretend to know all his ends. He who presumes to discover God's ends, in this latter sense, will scarce be excused from a high presumption, and no less a degree of folly. But to pretend to know God's ends, in the former sense, is no presumption; and, to take notice of them, rather a duty. For there are some things in nature, so curiously contrived, and so exquisitely fitted for certain operations and uses, that it seems little less than blindness in him who acknowledges a most wise author of things, not to conclude, that, tho' they may, perhaps, have been design'd for other uses, yet they were also design'd for these. Thus he who considers the admirable fabric of the coats, humours, and muscles of the eye, and how excellently all the parts are adapted to compose an organ of vision, can scarce help confessing, that the author of nature intended it should serve the animal, to which it belongs, to see with. The *Epicureans*, indeed, who believe the world to have been produced merely by a casual concurrence of atoms, may have a kind of excuse, which other philosophers, who acknowledge a deity, want. Thus, the very supposition, for instance, that a man's eyes were made by chance, argues, that they need have no relation to a designing agent; and the use that a man makes of them, may be either casual too, or at least an effect of his knowledge, not of nature's. But when, upon the anatomical dissection, and the optical consideration of this part, we find it to be as exquisitely fitted for an organ of sight, as the best artificer could have framed a little engine, purposely design'd for the use of seeing; it sounds very harsh to say, that an artificer, who is too intelligent to do things by chance, or to make a curious piece of work, without knowing to what purpose, should not design it for an use to which 'tis wholly adapted.



'Tis possible, indeed, he may have design'd more uses of it than one; and, perhaps, such as we cannot find out: however, among its several uses, this, to which we see it so admirably adapted, should be thought one. And I see not how it magnifies God's wisdom, or expresses our veneration of it, to exclude from the number of his ends, in framing human eyes, that most obvious and ready use, which, we are sure, is made of them; and which they could not be better fitted for. Thus, suppose a countryman, brought into the garden of a mathematician, should there see one of those curious gnomonic instruments, that shew, at once, the place of the sun in the zodiac, his declination from the equator, the day of the month, the length of the day, &c. it would, indeed, be presumption in him, whilst unacquainted both with mathematics, and the several intentions of the artist, to pretend himself able to discover all the ends for which so curious and elaborate a piece was framed: but, when he sees it furnish'd with a stile, with horary lines, with numbers, and, in short, with all the requisites of a sun-dial; and manifestly perceives the shadow to mark, from time to time, the hour of the day; 'twould be no presumption in him to conclude, it a sun-dial; whatever other uses it was design'd for.

And I demand of those, who will not allow that any natural things are directed to ends knowable by men, whether, if the divine author of them had really design'd them for such ends, the things themselves are not so framed and directed, as in that case they ought to be? And whether the fabric and management of natural things do really countenance, or contradict our supposition?

For my part, I confess, I think it no disparagement to the wisdom of any agent whatsoever, to suppose his productions design'd for such ends, among others, as they are excellently fitted for; unless it appear'd that such ends were unworthy the wise agent. But that cannot be justly said in our case; since 'tis not injurious to the divine author of things, to believe that some of the ends, to which he destined several of his corporeal works, were to exert and communicate his goodness; and to receive from his intelligent creatures an ardent love, a high admiration, and an obsequious gratitude for having display'd so much wisdom and beneficence in them.

And, indeed, I can by no means assent to the assertion of *Des Cartes*, that none of God's ends in his corporeal works are more manifest than others, but that all of them lie equally hid in the abyss of the divine wisdom; for there are many of his creatures, some of whose uses are so manifest and obvious, that the generality of mankind have in all ages, and almost in all countries, acknowledg'd them. And as to what he adds, that, in physics, all things ought to be made out by certain and solid reasons; I answer, first, that I see not how God's designing some of his works for particular uses, amongst others, is inconsistent with the physical accounts of their creation. Thus, a man may give a mechanical reason of the structure of every wheel, and other part of a watch, and of their way of acting upon one another, when rightly set together; and, in short,

PHYSICS. of the contrivance and phenomena of the little machine; tho' he supposes that the artificer design'd it to shew the hours of the day, and tho' he has that use in view, whilst he explains the fabric and operations of the watch. Secondly, I readily admit, that, in physics, we should ground all we deliver upon as solid reasons as possible; but there is no necessity that those reasons should be always *precisely physical*, especially when we are treating not of any *particular* phenomenon, produced according to the course of *nature* established in our system; but of the first and general causes of the world itself: from which causes, I see not why the final uses that appear manifestly to have been design'd, should be excluded. And to me 'tis not very material, whether in physics, or any other science, a thing be prov'd by the peculiar principles of that science; provided it be firmly prov'd by the common grounds of reason. And here let me observe, that the fundamental tenets of *Des Cartes's* own philosophy, are not prov'd by arguments strictly physical; but either by metaphysical ones, the more general dictates of reason, or the particular testimonies of experience. Thus, when, for instance, he truly ascribes to God all the motion that is found in matter, and consequently all the phenomena that occur in the world; he proves it not by a precise physical argument, that God, who is an immaterial agent, is the efficient cause of motion in matter; but only hence, that since motion does not belong to the essence and nature of matter, matter must owe its motion to some other Being: and then 'tis most agreeable to common reason to infer, that since matter cannot move itself, but must be mov'd by some other Being, that Being is immaterial; since, otherwise, some matter must, contrary to the hypothesis, be able to move itself. And when *Des Cartes* attempts to demonstrate, that there is always the same quantity of motion in the universe; and, consequently, that as much motion as one body communicates to another, it loses itself; he proves it by the immutability of God, which is not a strict physical argument, but rather a metaphysical one; as he had before prov'd, that God was the cause of all motion in matter, not by principles peculiar to physics, but by the common grounds of reason.

But tho' *Des Cartes* speaks very dogmatically and universally against mens pretending to know any final cause in natural things; yet I have met with a passage in his writings, where he seems to speak more cautiously, and opposes their opinion, who teach, that God had no other end in making the world, than that of being praised by men. His words are these. " 'Tis self-evident, that we cannot know the ends of God, unless he reveals them to us; and supposing it morally true, that all things were made to the end that we should praise God for them; in which sense it may be said, that the sun was made to give us light; yet it would be very childish and absurd to say, metaphysically, that God, like a proud man, had no other end in making the world, but to be prais'd by men for it; and that the sun, which is vastly larger than the earth, was wholly design'd to give us light, who inhabit but a very small part of the surface of the terraqueous globe." But he, at the same time, delivers two

or three other things, wherein I cannot acquiesce. As, first, that 'tis self-evident, we cannot know the ends of God, unless he himself reveals them to us. For what he here says to be self-evident, is not so to the generality of mankind, or even of philosophers; and, therefore, I think it ought not to be barely asserted, but prov'd. In the next place, he does not shew how we are obliged to praise God for his works, if God had no intentions that we should do so, or that we should discover none of the ends for which he design'd them. If a judicious man should see a book written in some language which he is an utter stranger to, and should know nothing farther of it than that 'twas compos'd by a very intelligent physician; he might, indeed, conclude, that it was not written by chance; but could not, by inspecting the book itself, be convinced, that it was form'd with great skill and kindness, and deserv'd his praise and thanks; since he was unable to know any of the particular ends to which the several chapters of it were design'd; and, consequently, could not see how well they were fitted to answer such ends. What *Des Cartes* says, that it is childish and absurd to think God had created the sun, which is vastly bigger than the earth, only to afford light to a small number of men, is somewhat invidiously propos'd; for there are few eminent writers who confine the utility of the sun directly to its affording light to man: and the small bulk of mankind ought not to make it seem absurd, that God may have had an especial eye to their welfare in framing that bright globe; since that most excellent machine, the human body, appears to be a more admirable thing than the sun: besides, the rational and immortal soul that resides in it, is incomparably more noble than a thousand masses of brute unorganized matter can be justly reputed. And since, in this very discourse, our author confesses that we may know the ends of God's corporeal works, if he reveal them to us; a christian philosopher may be allow'd to think the sun was made, among other purposes, to enlighten the earth, and for the use of man: since the scripture teaches, that not only the sun and moon, but the stars of the firmament, which *Des Cartes*, not improbably, thinks to be so many suns, were made to give light to the earth, and were divided to all the nations that inhabit it. Perhaps it were not rash to add, that we may know some of God's ends in things corporeal, without lessening our veneration of his wisdom; as we know some of them in other matters, of which the scripture furnishes us with a multitude of instances; as, particularly, *Job's* sacrificing for his friends, and the declared uses of the *Urim* and *Thummim*: since God may, if he pleases, declare truths to men, and instruct them by his creatures and actions, as well as by his word. And thus he taught *Noah* by a rain-bow, and *Jonah* by a gourd and a worm; and regulated the encampments of the *Israelites* by the guidance of a cloud, and a fiery pillar. Lastly, *Des Cartes* objects, that those he dissents from, talk as if they look'd upon God as a proud man, who design'd his works only to be praised for them; but I know not whether he herein speaks so cautiously and reverently of God, as he ought, and elsewhere uses to do. For as humility, tho' a virtue in men,

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is extremely remote from being any of God's perfections; so that may be pride in a man, who is but a creature imperfect, dependant, and having nothing of his own, which wou'd be none at all in the creator, who is incapable of vice, and who may, if he pleases, justly propose to himself his own glory for one of his ends, and both require and delight to be prais'd by men for his works; since he is most worthy of all praise, which it is their duty and reasonable service to pay him.

'Tis not without concern, that I find myself obliged thus to oppose some sentiments of *M. Des Cartes*, for whom I have, otherwise, a great esteem; for I am not at all of their mind, who think that author a favourer of atheism, which, to my apprehension, would subvert the very foundation of his own philosophy. But, judging that his doctrine, as generally understood, as to the rejection of final causes from the consideration of philosophers, tends to weaken one of the best, and most successful arguments for the being of a God, whom they ought to admire, praise, and reverence; I think it my duty to prefer an important truth before my respect to any man, how eminent soever, who opposes it; and to consider more the glory of the great author of nature, than the reputation of any of her interpreters.

And to confirm what I have hitherto deliver'd, 'tis remarkable that the excellent contrivance of the great system of the world; and especially the curious fabric of animal bodies, with the uses of their several senses and parts, have been the great motives, which in all ages and nations induced philosophers to acknowledge a Deity; and that the noblest and most intellectual praises have been paid him by the priests of nature; as proceeding from the transcendent admiration rais'd in them by the attentive contemplation of the fabric of the universe, and of the curious structures of living creatures. And, therefore, it seems injurious to God, as well as unwarrantable in itself, to banish from natural philosophy the consideration of final causes; from which, chiefly, I cannot but suppose that God must reap the honour due to those glorious attributes, his wisdom and his goodness. And, I confess, I somewhat wonder, that the *Cartesians*, who have generally, and some of them with skill, maintain'd the existence of a Deity, should endeavour to make men throw away an argument, which the experience of all ages shews to have been the most successful, and in some cases the only prevalent one, to establish, among philosophers, the belief and veneration of God. I know the *Cartesians* say, that their master has demonstrated the existence of a God, by the innate idea men have of a Being infinitely perfect, who left it upon the human mind, as the mark of an artist impress'd upon his work; and that they also ascribe to God the creation of matter out of nothing, and its motion; which sufficiently argue the immensity of his power. But tho' I would by no means weaken the argument drawn from the innate notion of God, since many learned men have acquiesced in it; yet why may we not reasonably think, that God, who has taken care men should acknowledge him, may have also provided for the securing a truth of so great consequence, by stamping characters, or leaving

leaving impressions of his wisdom and goodness, as well externally upon the world, as internally upon the mind? The bare contemplation of the fabric of the world, without considering any part of it as destin'd to certain uses, may still leave men unconvinced that there is any intelligent, wise, and provident author and disposer of things; since we see, the *Aristotelians*, notwithstanding the extent, symmetry, and beauty of the world, generally believed it to have been eternal. And tho' they did not allow it to have been created by God, yet because they asserted that animals, plants, &c. act for ends; they were obliged to acknowledge a provident and powerful Being, that maintain'd and govern'd the universe, which they call'd *Nature*; tho' they often dangerously mistook, by confounding this Being with God himself; and at other times speaking of it as co-ordinate with him; as in that famous axiom of *Aristotle*, "God and nature do nothing in vain." I acknowledge, therefore, that as I set a just value upon the *Cartesian* proof of God's existence; so I see no reason why we should give up any other strong argument, that proves so noble and important a truth; especially since the *Cartesian* way of considering the world, tho' very proper, indeed, to shew the greatness of God's power, does not, like the way I plead for, manifest that of his wisdom and beneficence. For while a *Cartesian* only shews, that God is admirably wise, upon the supposition of his existence; the same thing is manifested in our method, by the effect of a wisdom as well as power, that cannot reasonably be ascribed to any other than a most intelligent and potent Being: so that by this means men may, at once, be brought to acknowledge God, to admire him, and to praise him.

S E C T. II.

BEFORE I proceed to the second question; Whether we may consider final causes in all sorts of bodies, or only in such as are peculiarly qualified? I must divide natural bodies into animate and inanimate. The former term I here take in the larger sense, to comprehend not only animals, but vegetables; tho' I shall not reject the opinion of those, who are unwilling to allow plants such a soul or life, as is confessedly granted to animals.

Whether final causes are to be expected in all, or only in some particular bodies?

Of the inanimate bodies of the universe, the noblest, and those which chiefly deserve to be consider'd on this occasion, are the sun, planets, &c. For when men saw those vast luminous globes, and especially the sun, to move so constantly and regularly about the earth; diffusing thereon both light and heat, and by their various revolutions to produce day and night, summer and winter, and the vicissitudes of seasons; they concluded, that these motions were guided by some divine Being, and design'd for the benefit of man. Whether this be a demonstrative proof, I shall not now debate; but surely it may carry thus much of probability with it, that in case a man shall think the fabric of the celestial bodies was the production of an intelligent and divine agent, the irregular phenomena will not contradict him; since there

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there is nothing in that fabric unworthy a divine author: and the motions and effects of the sun and stars may well allow us to think, that, among other purposes, they were made to illuminate the terrestrial globe, and bring heat and other benefits to the inhabitants of it. So that the contemplation of the heavens, which so manifestly declare the glory of God, may justly excite men both to admire his power and wisdom in them; and to return him thanks and praises for the great advantages we thence receive. On the other hand, it may be said, that in bodies inanimate, whether the portions of matter they consist of, be greater or less; the contrivance is very rarely so exquisite, but that the various motions and occurrences of their parts may, without much improbability, be suspected capable, after many essays, to cast one another into several of those circumvolutions call'd by *Epicurus* *οὐρεσπᾶς*, and by *Des Cartes*, *Vortices*; which being once made, may continue a long time, after the manner explain'd by the latter. But allowing this hypothesis to be possible; when I consider that we are not yet sufficiently acquainted with the true system of the world, nor usually sensible how small a part the terrestrial globe makes of the universe; I am apt to suspect, that men often assign the systematical ends and uses of the celestial bodies upon slender evidence; concluding them made and moved only for the service of the earth, and its inhabitants. And tho' I will not deny, that as we actually receive benefits by the established order and motion of the heavenly bodies, so one of the several uses intended by the author of nature, may particularly regard our species; yet perhaps 'twill not be easy to prove, that some of those bodies and motions are not rather intended for other purposes, than to cast their beams, or shed their influences upon the earth: at least, it seems probable to me, that the situations of the celestial bodies do not afford, by far, so clear and cogent arguments of the wisdom and design of the author of the world*, as do the bodies of animals and plants. And for my part, I think I see more of admirable contrivance in the muscles of a human body, than in what we yet know of the astronomical world: and the eye of a fly, as far as appears to us, seems to be a more curious piece of workmanship, than the body of the sun itself.

* As a noble instance of wisdom and design in the situation of the planets, take an observation of the great Sir *Is. Newton*. "Those planets, says he, have the greater density, *ceteris paribus*, which are placed nearest the sun. Thus *Jupiter* is more dense than *Saturn*, and the earth more dense than *Jupiter*. For it was necessary to place the planets at different distances from the sun, that each might receive a greater or a less degree of his heat, according to its density. If our earth were placed in the orbit of *Saturn*, the water of it would be frozen up; and if in the orbit of

"*Mercury*, 'twould presently exhale in vapour. For the sun's light, to which his heat is proportionable, is seven times more dense in the orbit of *Mercury*, than with us; and I have found, by the thermometer, that water will boil with a heat seven times as great as that of the summer's sun. But the matter of *Mercury* is, doubtless, fitted for heat; and therefore must be denser than that of our earth: since all dense matter requires a greater degree of heat, to perform the operations of nature." *Newton, Princip. p. 372.*

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As for other inanimate bodies, the matter whereof seems unorganiz'd, tho' there be no absurdity in supposing that these were also made for distinct and particular purposes, if not also for human uses; yet most of them are of such easy and unelaborate textures, as to make it seem possible, that various occurrences and jostlings of the parts of the universal matter might, at one time or other, have produced them: since we see, that in some chymical sublimations and crystallizations of mineral and metalline solutions, and other phenomena, where the motions appear not to be particularly guided and directed by an intelligent cause, bodies of as various textures as those are producible.

If it be said, that supposing chance, or any thing else, without the particular guidance of a wise and all-disposing cause, can make a finely-shaped stone, or a metalline substance, growing, as silver sometimes does, in the form of a plant; ought we not to allow, that chance may also make vegetables and animals? I can by no means grant the consequence. For there are some effects so easy, and readily to be produced, that they do not infer any knowledge or intention in their causes; whilst there are others that require such a number and conjunction of conspiring causes, and such a continued series of motions or operations, that 'tis utterly improbable they should be produced without the superintendency of a rational agent, wise and powerful enough to range and dispose the several intervening materials and instruments, in a manner necessary to produce such a remote effect: it will not therefore follow, that, if chance could produce a slight contexture in a few parts of matter, we may safely conclude it able to produce so exquisite and admirable a contrivance, as that of the body of an animal. What does it argue then, if sometimes, in sawing pieces of variegated marble, we happen to meet with the delineations or pictures of towns, woods, and men? For, besides that the delightfulness and rarity of such spectacles inclines the imagination to favour them, and to supply their defects; would any wise man conclude from hence, that a real town or wood, much less numbers of men, should be made by such a fortuitous concurrence of matter? What comparison is there betwixt the workmanship that seems to be express'd in a few irregular lines, drawn upon a plane, with perhaps two or three colours luckily placed, and the great multitude of nerves, veins, arteries, ligaments, tendons, membranes, bones, glands, &c. required to compose a human body; every one of the numerous parts whereof, must have its determinate size, figure, consistence, situation, connexion, &c. and many, or all of them together, conspire to exercise and perform determinate functions and uses? And, indeed, I never saw any inanimate production of nature, or, as they speak, of chance, whose contrivance was comparable to that of the meanest limb of the most despicable animal; nay, there is infinitely more art express'd in the structure of a dog's foot, than in that of the famous clock at Strasburg.

And tho' the paw of a dog be far inferior in its structure to the hand of a man, yet even this is inconsiderable, if compared to the eye; the several parts

Evident marks of design in the structure of the eyes, and other parts, of animals

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parts whereof, how numerous soever, could none of them be spared or alter'd, unless for the worse; as may appear from those many diseases observ'd in that small admirable organ: for each of those diseases consists in this, that the humours, or other parts of the eye, are brought to a state different from that whereto nature had design'd it. 'Twould be tedious to enumerate the several distempers of the eye; wherefore I shall only mention two or three particulars, wherein one would scarce imagine that a small recess from the natural state could bring any considerable or sensible inconvenience. That which we call the pupil, is not a substantial part of the eye, but only an aperture of the uvea; almost perpetually changing its bigness, according to the different degrees of light the eye chances to be expos'd to. And therefore, it should seem, that whilst this hole remains open, it performs its office, by giving entrance to the incident rays of light. And yet I lately saw a woman, who, after a fever, was not able to dilate the *Pupilla* of her eyes, as before; and tho' they were but very little narrower than ordinary, yet she complain'd she had thereby almost lost her sight. The preternatural constriction of the pupil is indeed no frequent distemper; yet physicians have given it a place among the stated diseases of the eye. And, on the other side, tho' a competent wideness of the pupil be requisite to a clear and distinct vision; yet if its dilatation exceeds the due limits, there is thereby produced a distemper worse than the former; because it often almost totally deprives the patient of sight. It may seem also but a slight circumstance, that the transparent coats of the eye should be devoid of colour; and of as little moment, that the cornea should be very smooth, provided it remain transparent: yet when either of these circumstances is wanting, the sight may be greatly vitiated. Thus we see, that in the yellow jaundice, the adventitious colour wherewith the eye is ting'd, makes the patient think he sees many objects yellow, which are of a contrary colour. And I know a gentleman, who having had a small pustule excited and broken upon the cornea, tho' the eye has long been cicatriz'd; yet a very little inequality or depression, that still remains upon the surface of that transparent coat, so affects him, that when he comes into the open fields, or the streets, he for a pretty while thinks he sees objects very glaring, and as many others as men usually do stones at the bottom of clear water: which I impute to the want of uniformity in the refraction of those reflected rays of light, that fall upon the unequal surface of the cornea. To give a further proof, that the eye was made with design, I shall here take notice of an observation or two, that do not occur in the dissection of that part, and are therefore often unobserv'd by anatomists.

I have found in frogs, that, besides those parts of the eye which they have in common with men, dogs, cats, and most other animals, there is a peculiar membrane or cartilage, which is not commonly perceiv'd; wherewith they can at pleasure cover the eye, without too much hindering the sight, because the membrane is both transparent and strong; so that it may pass for a kind of moveable cornea, or occasional safe-guard

to the eye. In furnishing frogs with this strong membrane, the providence of nature seems very conspicuous; for, they being amphibious creatures, design'd to pass their lives in watery places, which, for the most part, abound in plants, endow'd with sharp edges, or points; and the progressive motion of this animal, being not by walking, but by leaping, if his eyes were not provided with such a case, he must either shut them, and so leap blind-fold; or, by leaving them open, must run the risk of having the cornea cut, prick'd, or otherwise offended; whilst this membrane, as was said, is like a kind of spectacle, that covers the eye, without taking away the sight: and as soon as the occasion for it is over, the animal withdraws it into a little cell, where it rests till its use be again required. This membrane becomes visible, by applying the point of a pin, or any such sharp thing, to the eye of a frog, whilst his head is held steady; for to screen his eye, he will presently cover it therewith, and afterwards withdraw it, upon a removal of the suspected danger. And because many birds are destin'd to fly among the branches of trees, and bushes; lest, by this means, the prickles, twigs, leaves, &c. should wound or offend their eyes, nature hath also given them such another kind of horny membrane, as we find in frogs.

'Tis known that men, and most four-footed beasts, and birds, have several muscles belonging to their eyes, by the help of which, they can turn them any way; and so obvert the organ of sense to the object. But nature, not having given that mobility to the eyes of flies, she, in recompence, furnishes them with a multitude of little protuberant parts, finely rang'd upon the convex of their large bulging eyes: so that by means of these numerous little studs, numberless rays of light are reflected from objects placed on either hand, above, or beneath the level of the eye, and conveniently fall upon that organ, to render the objects they come from, visible to the animal; and by the help of a good microscope, and a clear light, some hundreds of these little round protuberances may be discover'd, curiously rang'd, on the convexity of a single eye of an ordinary flesh-fly.

But some may here pretend, that all organs of sight ought to be conform'd to those of men; these being the best, and most perfect. And, indeed, man being justly reputed the most perfect of animals, it is not strange he should presume, that his eyes, and other parts of his body, are the best contriv'd of any to be found in nature; yet we cannot, from hence, safely conclude, that all eyes, which, in other animals, are of different structures from those of man, should, for that reason, be defective. For, first, the admirable wisdom display'd by the author of nature, in fitting the eyes, and other organical parts of animals, to their several uses, and the respective functions we see them exercise, may justly persuade us, that the things whose reasons and uses we do not alike discern, are, nevertheless, most wisely constituted: God having too much knowledge to do any thing unskilfully; and we having too much presumption, if we suppose he had no ends in framing his creatures beyond the reach of our discovery. And, secondly, the eye is not to be consider'd abstractedly, as an instrument of

PHYSICS. vision; but as an instrument belonging to an animal, that is to make use of it in particular circumstances. And, therefore, it ought highly to recommend the wisdom and providence of the great author of things, that he has furnish'd various species of animals, with organs of sight, very differently framed and placed; since this diversity nobly manifests his great providence and knowledge, in having so admirably suited the eyes of all animals, both to the rest of their bodies, and to those parts of the great theatre of the world, on which he design'd they shall live and act. Thus, tho' several beasts, as horses, oxen, &c. have their eyes furnish'd with a seventh muscle, besides the six they share in common with men; we must not conclude, that either the organs of vision are imperfect in men, or that those of these beasts have something superfluous: for they being to feed, for the most part, on the grass of the field; and that they may the better chuse their food, being obliged to turn their eyes, for a long time together, downwards; the seventh muscle excellently serves them for that purpose, by enabling them to continue unwearied by such a posture; whilst man, who has no such necessity of looking assiduously downwards, would be only incumber'd by a seventh muscle.

On the other hand, the deficiency observable in the eyes of some animals, compared to those of man, may be ascribed to the just contrivance of nature, that, on most occasions, declines doing what is unnecessary to the particular ends she aims at in the fabric of a part. Thus moles, being destin'd to live, for the most part, under ground, have their eyes so little, in proportion to their bodies, that 'tis commonly believ'd they have none at all; but, tho' I have found the contrary, yet their eyes are very different from those of other four-footed beasts; which need not be wonder'd at, considering that nature design'd these creatures to live under ground, where sight is of no use; and where large eyes would be more expos'd to danger: besides, their sight, as dim as it is, serves them to perceive when they are no longer under ground; which seems to be the most necessary intelligence they want from their eyes.

'Tis observed, that the organs of vision in a camelion, are of a very uncommon structure; since, to omit other considerable peculiarities, his eyes often move independently on each other; so that, for instance, he can look directly forward with the right eye, and, at the same time, directly backward, towards his tail, with the left; or may turn the pupil of the former strait upwards, whilst the other is turn'd directly downwards. This peculiar power seems to have been granted him by providence, that as he is a very low animal, and destin'd to live, for the most part, in trees and bushes, and there to feed chiefly on flies; he may perceive them, which way soever they chance to come, within the reach of his tongue, which, being of a great length, he suddenly darts out, and therewith catches his prey.

Many fish have the crystalline humour of their eyes almost spherical; and, consequently, much rounder than it is found in man, and other terrestrial animals. And this difference of figure, tho' it would be inconvenient for

for us, is very well accommodated to them ; since they, living in the water, which, as a thicker medium, much more refracts the rays of light than the air, thro' which they pass to our eyes ; 'twas fit their crySTALLINE humour should be of that figure, to refract the rays already refracted by the water, and thereby make them converge, so as to paint the images of objects at the bottom of the eye.

Should a person of curiosity survey and consider the various structures of the organs of vision, in different animals, and compare them with the other parts of the respective animal ; the scene he is design'd to act on, and the uses each of them is to make of his eyes, in the most ordinary circumstances ; he might, doubtless, offer a probable reason of the differences in those organs, which, to a common observer, would seem to be errors, or defects in nature. Thus, tho' the pupil of the eye be oblong in horses, oxen, and some other quadrupeds, as well as in cats ; yet, in the former kinds of animals, it lies transversely from the right side of the eye to the left, whilst, in cats, its situation is perpendicular : for horses and oxen, being usually obliged to find their food on the ground, they the more conveniently receive the images of the grass, &c. in a horizontal view, by having their *Pupilla* transversely placed ; whilst cats, being to live chiefly on rats and mice, which are animals that usually climb, and run upon steep places ; the most commodious situation of their pupil, for discovering and pursuing these objects, is the perpendicular. But, to proceed : the different structures, and situations of the eye, in different animals, wonderfully shew a great variety in the skill of the divine author. And, indeed, if I might presume to guess at any of God's ends, that are not manifest ; I should think that the delightful variety, we may observe, not only in animals themselves, consider'd as entire systems ; but in those parts of them which appear destin'd for the same function ; as particularly, that the organ of vision was design'd, at least, among other ends, to display the great creator's manifold wisdom ; and to shew, that his skill is not confined either to one sort of living engines, or in their parts of the same kind, to the same contrivance ; but is able to make a multitude of surprizing organs, all of them curious, and exquisite in their kind, with regard to their different uses.

To be able to frame clocks and watches, ships, mills, &c. manifests a far greater skill in an artificer, than the power of making but one of those engines, how perfectly soever he contriv'd it. And the same superiority of knowledge would be display'd, by contriving engines of the same kind, or for the same purposes, after very different manners.

Thus weights are of great use and necessity in the famous clock of *Strasburg* ; and therefore it recommends the contrivers of watches, that they give them a very little and portable bulk, which is wanting in the *Strasburg* machine ; and still more, that they can make a clock without weights, and substitute a spring to perform their office.

And thus tho' it seems absolutely necessary, that an animal should be furnish'd with feathers, in order to fly ; yet the wise creator hath shewn, that he is
not

PHYSICS. not confined to apply them for that purpose ; since a flying-fish is able to move a great way in the air. And the *Indies* have lately afforded a sort of flying squirrels, one whereof I saw, alive, at *Whitehall*. And tho' the flight of these is small, yet there is another kind of animal, without feathers, that long continues upon the wing, and that is the bat ; some whereof I have seen but little less than hens : and was assur'd, by a credible eye-witness, that, in the kingdom of *Golconda*, he had seen much bigger.

This consideration is alone sufficient to justify the wisdom of the creator ; who, being a most free, as well as a most wise agent, men ought not to find fault, if he think fit to recommend his wisdom, by displaying it in very different manners : tho' there are many cases wherein the less perfect fabric, or situation, of an eye, or other organical part, may be more convenient than the correspondent organ of man, to obtain the ends for which it was given to an animal design'd to act upon its respective stage, and live by its peculiar provision. Besides, an organical part may, in some animals, be intended for more uses than in others ; and, therefore, require a different structure : as, in moles, the feet are differently framed, or situated, from those of other quadrupeds ; because the chief use they are to make of them, is not to walk upon the ground, but to dig themselves ways under it ; provident nature wisely suiting the fabric of the parts to the uses wherein they were to be employ'd : as, a mechanic employs one contrivance of his wheels, pinions, &c. when he makes a mill to be driven by water ; and another, when it is to be mov'd by the wind. The camelion has a tongue, both peculiarly shaped, and of a length disproportionate to that of his body ; because, as we before observed, he is to take his prey, which are flies, by shooting out that instrument ; and could not often, otherwise, approach very near them, without frightening them away. And, in many cases, where this reflection does not so properly take place, we may observe, there is a wonderful compensation made for that which seems a defect in the parts of an animal, of a particular species, compar'd with the correspondent ones of an animal of some other species. Thus birds, which want teeth to chew their food, are not only furnish'd with hard bills to break it ; and, birds of prey, with crooked ones to tear it ; but, which is more considerable, have crops to prepare and soften it, and very strong muscular stomachs, to digest and grind it : in which work, they are usually assisted by gravel, and little stones, that they are led, by instinct, to swallow ; great quantities whereof are often found in their stomachs.

Chance an imaginary Being.

And let it be here observ'd, that chance is really no natural cause, or agent, but a creature of man's own making. For the things that are done in the corporeal world, are really done by the parts of the universal matter, acting and suffering according to the laws of motion, established by the author of nature. But we men, looking upon some of these parts as directed in their motions by God, or nature, and as disposed to the attainment of certain ends ; if, by the intervention of other causes, that we are not aware of, an effect be produced, very different from that which

we

we supposed was intended; we say that effect was produced by chance: so that chance is indeed but a notion of ours, and signifies no more than that in our apprehensions the physical causes of an effect did not tend to the production of what they have, nevertheless, produced. And therefore I wonder, that the philosophers who preceded *Aristotle*, never treated of chance among natural causes.

And as some stones, of the most curious shapes, have embolden'd many favourers of *Epicurus* to set them in competition with those animals, or parts of animals, from their likeness whereto they have receiv'd their names; we ought to consider, that several learned men have, of late, made it very probable, that these stones were once really the animals, or those parts of them which they resemble, and were afterwards turn'd into stones by the supervention of some petrescent matter, or petrifying cause. And, allowing some of these sorts of stones to be the production of the mineral kingdom; yet it would not clearly follow, that they owe their shapes to chance, since 'tis no absurdity to admit seminal principles in the more elaborate sorts of fossils. However, I think it very injurious to make these productions rival the animals to which they are compared. For the shape, in which alone they and the animals agree, being an external thing, is not worthy to be mention'd, in comparison of that wherein they differ; the rude and slight texture of the best shaped stone being immensely inferior to the internal contrivance of an animal; which must consist of a multitude of parts of a determin'd figure, bulk, situation, &c. as is obvious to those who have seen dissections skilfully made. And 'tis not only in the firm and quiescent parts that this great internal difference between stones, and the animals they resemble, is to be found: there appears a far greater difference between a living animal, and a stone, than any the anatomical knife can shew us betwixt a dead one, and a stone, tho' ever so curiously figured. For there are numberless liquors, spirits, digestions, secretions, coagulations, motions of the whole body, of the limbs and other parts, which are lodg'd and perform'd in a living body, and are, perhaps, more admirable than the structure of the solid and quiescent parts themselves: so that, tho' a stone, in external appearance very like a shell-fish, were made by chance; yet from thence to conclude, that chance may make a living shell-fish, would be to argue worse than he who should pretend, that because an unskilful smith may make a hollow piece of metal like a watch-case, and fill it with some rude stuff, he must, therefore, be able to make a watch; for there is less difference betwixt the skill expressed in making the case of a watch and the movement, than in making a body like a shell, and the internal parts of a real fish: or, that because putrefaction and winds have sometimes made trees hollow, and blown them into the water, where they swim like boats; therefore the like causes may make a regular galley, according to the laws of naval architecture, man it, steer it, excite and guide all its motions to the best advantage, for the preservation, and various uses of the vessel. In short, if chance sometimes does strange things, 'tis in regard to what she herself, not to what nature uses to perform.

And

Final Causes of Natural Things.

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And now, to give my thoughts upon the second question. 1. I think the naturalist may draw arguments from the ends and uses of the parts of living bodies, provided he proceeds herein with due caution. 2. 'Tis my opinion, that the inanimate bodies here below, proceeding not from seminal principles, have but a more slight texture; such as earths, liquors, flints, pebbles; and will not sufficiently warrant reasonings drawn from their supposed ends. 3. It seems to me, that the celestial bodies abundantly declare God's power and greatness, by the immensity of their bulk, and, if the earth stand still, the celerity of their motions; and also argue his wisdom, and general providence, with regard to them: because he has, for so many ages, kept such a variety of vast vortices, or other masses of matter in motions immensely rapid, without permitting them to destroy one another, or lose their regularity. And I see no absurdity in supposing that, among other uses of the sun, and of the stars, the service of man might be intended; tho' I doubt whether, from the bare contemplation of the heavens, and their motions, it can be cogently inferr'd, that the chief end of them all, is to enlighten the earth, and bring benefits to the creatures that live upon it.

Revelation allows us to speak more positively of final causes, than natural philosophy.

Hitherto I suppose the naturalist to discourse merely upon physical grounds; but if revelation be admitted, we may rationally believe more, and speak less doubtfully of the ends of God, than bare philosophy will warrant us to do. For if God is pleas'd to declare any thing to us concerning his intentions in the making of his creatures, we ought to believe it, tho' the consideration of the things themselves did not give us the least suspicion of it; which yet, in our case, they do. The scriptures expressly teach us, that "God made the two great luminaries, the greater to rule the day, and the lesser to rule the night;" that "he made the stars also, and set them in the firmament, or expanse of heaven, to give light upon the earth." And these are reckon'd among the uses of these luminaries: "to divide the day from the night; and to be for signs, and for seasons, and for days and years." And *Moses*, dissuading the *Israelites* from worshipping the sun, the moon, and the stars, says, that "the Lord had imparted them unto all nations under the heaven." And therefore those *Cartesians*, who admit the authority of holy scripture, should not reject the consideration of such final causes as revelation discovers to us; since 'tis certainly no presumption to think we know God's ends, when he himself acquaints us with them: nor to believe that the sun, tho', generally, esteem'd a nobler body than the terrestrial globe, was made, among other purposes, to enlighten it. 'Tis recorded, in the book of *Genesis*, that God's design in making man, was, that he should "subdue the earth, and have dominion over the fish of the sea, and over the fowl of the air, and over the cattle, and over all the earth, and over every living thing that moveth upon the earth." And the same book informs us, that, after the deluge, God "deliver'd all terrestrial beasts, and fowl, and fish, and every moving thing that lives," into the hands of men, and intended that they should eat animals; as, before the flood, he

had

had appointed them all the sorts of wholesom vegetables for their food. And, since God was pleased to appoint, that men should live on these creatures; it cannot be absurd to say, that, among other purposes to which he destin'd the sun, his shining upon the earth was one; since, without his light and heat, men could not provide for themselves; and neither those plants, that men and cattle must feed upon, could grow and ripen; nor, consequently, those animals that were to be their principal food, and serve them for other uses, could be sustain'd, and provided for. Many other texts might be here alledged to the same purpose; but I shall content myself to mention that of the royal prophet, when, speaking of man to his maker, he says, "Thou hast made him a little lower than the angels, and hast crown'd him with glory and honour. Thou madest him to have dominion over the works of thine hands, and hast put all things under his feet."

Indeed, if we consider only that visible part in man, his body; the smallness thereof may make it seem improbable, that portions of the universe, incomparably greater than he, should be intended for his service. But christians will not think this incredible, if they consider man, as he chiefly consists of a rational mind; which proceeds immediately from God, and is capable of knowing him, loving him, and being eternally happy with him. They who despise man, consider'd in this capacity, very little know the worth of a rational soul. But God, who is the best judge in this case, was pleas'd to consider men so much, that it gave *David* cause to admire, as we just now saw; and not only to form them in his image, at their first creation; but, when they had wilfully lost, and forfeited it, he vouchsafed to redeem them by the sufferings and death of his own son, who is incomparably more excellent than the whole world. And 'tis not incredible, that God should have intended many of his other works to be serviceable to man; since, by miraculous operations, he hath, sometimes, suspended the laws of nature; and, sometimes, over-ruled them for his sake: as appears by the flood, by the passage of the *Israelites* thro' the red-sea, and the river of *Jordan*; by the standing-still of the sun and moon, at *Josuah's* command; by the inefficacy of the burning fiery-furnace on *Daniel's* three companions; and, in short, by the stupendous eclipse of the sun, at the crucifixion of the *Messias*.

And we shall the less scruple to admit, that such vast bodies, as the sun and moon may tend to serve mankind, if we consider, 'tis far from being a constant rule, that a thing more excellent cannot be employ'd for the good of a less excellent. Thus an angel was sent to relieve *Hagar* in the wilderness; another had regard to the life of a prophet's ass; and many more were employed on earth, in doing good offices to particular persons: and, of all the angels in general, the epistle to the *Hebrews* informs us, that "they are ministring spirits, sent forth to minister unto them who shall be heirs of salvation."

Final Causes of Natural Things.

S E C T. III.

How inanimate
bodies may act
for ends, where-
with they are un-
acquainted.

PReparatory to the discussion of the third question, "Whether, and in what sense, the acting for ends may be ascribed to an unintelligent, and inanimate body?" it will be necessary to clear the grand difficulty that has before, and ever since the time of *Aristotle*, perplexed those who allow the consideration of final causes in natural philosophy. This difficulty is obvious enough; for much the greater part of bodies being senseless, and, most of them, lifeless too; it seems inconceivable how they should constantly act for ends they are not capable of pre-designing, and, appositely employ means they have no knowledge to make choice of.

Aristotle, who expressly teaches, that nature does nothing in vain; and, rightly judg'd, that the actions of natural agents had a tendency to certain ends; takes no notice of this difficulty, but seems rather to shift it off than resolve it.

But, to consider the difficulty itself, there are two accounts on which the actions of natural agents may be said to tend to a certain end; as, either when the agent has a knowledge of that end, and acts with an intention to obtain it; or, when the action of the proximate agent is directed as it ought, to obtain an end, which, yet, is neither known, nor intended, by that proximate agent; but by a remoter, that is intelligent. In the former of these senses, I cannot admit, that any inanimate body acts for ends; since that pre-supposes the agent both to know the end he is to obtain, and to purpose to obtain it: things whereof inanimate bodies are incapable. And, to fancy with some, that they may have a knowledge, *sui generis*, as they speak; which, tho' confined to the actions proper to a particular kind of body, shall yet suffice to determine them to those actions, is, to offend against that rational, and receiv'd rule in philosophy, that Beings are not to be multiply'd, without there appears a necessity for them; and to introduce a sort of knowledge that seems unintelligible.

It remains, then, that I embrace the second sense, in which 'twas formerly said, natural things may work for an end; tho', in this case too, we must speak somewhat improperly: for the action may more justly be attributed to the remote intelligent, than to the immediate agent, which is but, as it were, the instrument of the other. Now, it appears to me, that the most wise, and powerful author of nature, whose piercing sight is able to penetrate the whole universe, and survey all the parts of it at once; did, originally, frame material things into such a system, and settle among them such laws of motion, as he judged suitable to the ends he propos'd to himself in making the world. And as, by virtue of his vast and boundless intellect that he, at first, employ'd; he was able, not only to see the present state of things he had made, but to foreknow all the effects that particular bodies, peculiarly qualify'd, and acting according to the laws of motion, by him established, would, in particular circumstances, have on one another; so, by the same omniscient power, he

he was able to contrive the whole fabric, and all its parts, in such a manner, that, whilst his general concurrence maintain'd the order established, each part of this great engine should, without either intention, or knowledge, as regularly and constantly act towards the attainment of the respective ends he design'd them for, as if themselves really understood, and industriously prosecuted those ends. Thus, in a well-made clock, the spring, the wheels, the balance, &c. tho' each of them act according to the impulse it receives, and the determination that is given it by the other parts of the engine, without knowing what the neighbouring parts, or what themselves perform; yet their tendencies are so determined, and over-ruled, and their motions so regulated, by the structure of the machine, that the whole could not proceed more conveniently, nor better perform the office of a clock, if they knew they ought all to conspire, and were design'd to make the index truly mark the hours. 'Tis not easy, indeed, to conceive, how one agent should, by so simple an instrument as local motion, direct such a multitude of others, as make up a world, to act with the same regularity, as if each of them went upon its own particular design, and yet all conspire to obey the laws of nature. But if we consider, that this great work is ascribed to an omniscient, and almighty agent, it will not appear incredible; especially since 'tis manifest, that a multitude of bodies act as we have supposed: and that, if we will not ascribe to God the direction, and superintendency of the motions that are manifestly fitted for the attainment of ends, we must ascribe them to nature; which will not lessen, but increase the difficulty. And, upon viewing a great engine, wherein the works of many trades, and a great variety of other motions were perform'd by little puppets, that managed the tools of the several artificers; whilst all of them were set on work by a single spring, which communicated motions, regulated and determined by the particular structure of the little statues, &c. I could not think it impossible, that the great creator should be able, by the motions and structures of matter, to set very many partial, and subordinate, engines a-going. For 'twill not, I hope, be said, that the multitude of these, any thing near surpasses that of those which I saw in the hand of an illiterate tradesman, so far as the narrow knowledge of that artificer is surpassed by the boundless understanding of an omniscient artist. And God's wisdom and skill is more display'd in making so many various bodies act according to their particular designations, while they all conspire to the general ends of the universe; than barely in causing bodies to act, appositely, for ends to themselves unknown. For, if moving bodies be duly disposed, and have a sufficient connection, 'tis not difficult to direct a few of them to the attainment of an end proposed by an understanding agent, tho' unknown to the immediate agents: as, anciently, among the *Jewish* husbandmen, and, at this day, in some parts of the east, the ox, by treading the corn, separates the grain from the straw, as well as the labourer who thrashes it, on purpose to make that separation; and a horse,

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horse or an ass, by going round in a mill, may grind the corn as well as the miller himself.

This doctrine, however, is not inconsistent with the belief of any true miracle; for it supposes the ordinary and settled course of nature to be maintain'd; without at all denying, that the most free and powerful author of nature is able, whenever he thinks fit, to suspend, alter, or contradict those laws of motion, which he, alone, at first establish'd, and which requires his perpetual concurrence to uphold.

S E C T. IV.

*How final causes
are to be consid-
er'd.*

TO come to our last question, "With what cautions, final causes are to be consider'd by the naturalist?" I must observe, that the cases whereto it may relate, are so many and various, that I can only touch upon some few of them.

And to make way for what I am to offer, by a distinction; there are two ways of reasoning from the final causes of natural things, that ought not to be confounded. For sometimes men draw arguments from the use of bodies, that relate to the author of nature, and the general ends he is supposed to have intended in things corporeal: as when from the manifest usefulness of the eye, and all its parts, for vision, 'tis infer'd that the eye was originally framed by a very intelligent Being, with a particular care that animals should be furnish'd with the fittest organ of so necessary a sense. And sometimes, also, men ground arguments upon the supposed ends of things, as to the peculiar nature of the things themselves; and conclude, that this affection of a natural body or part ought to be granted, or that deny'd; because by this, and not by that, or by this more than by that, the end design'd by nature may be best and most conveniently obtain'd. The latter sort of arguments I usually call purely physical; and those of the former may be stiled physico-theological, or, by a shorter name, metaphysical ones.

But in order to be the more clear upon this subject, I shall refer my thoughts of it to the five following propositions.

P R O P. I.

As to the generality of the celestial bodies, it seems unsafe to draw arguments of their nature, on a supposition of particular ends, at least of the human ones, design'd by God in their formation.

*As to the celestial
bodies.*

I am by all means for encouraging the contemplation of the celestial part of the world, and the shining globes that adorn it, and especially the sun and moon, in order to raise our admiration of the stupendous power and wisdom of him, who was able to frame such immense bodies; and, notwithstanding their vast bulk, and scarce conceivable rapidity, keep them for so many ages constant both to the lines and degrees of their motion, without interfering with one another. And, doubtless, we ought to
return

return thanks and praises to the divine goodness, for having so placed the sun and moon, and determin'd the former, or else the earth, to move in particular lines, for the good of men, and other animals. And how disadvantageous would it have been to the inhabitants of the earth, if the luminaries had moved after a different manner? I dare not, however, affirm, that the sun, moon, and other celestial bodies, were made, solely, for the use of man; much less presume to prove one system of the world to be true, and another false; because the former is better fitted to the conveniency of mankind, or the other less suited, or perhaps altogether useless to that end. Thus men sometimes alledge, that the sun ought to be in perpetual motion, to shine upon the earth; because, as they fancy, 'tis more convenient for man, that those distant bodies, than that the earth, which is his habitation, should be kept in motion. But considering things as mere naturalists, it seems not very likely, that a most wise agent should have made such vast bodies as the sun and the fix'd stars, (especially, if we suppose them to move with such a prodigious rapidity, as vulgar astronomers assign them) chiefly to illuminate a little globe, that, without an hyperbole, is but a physical point in comparison of the immense celestial space; while those lights might as well illuminate the earth, if they were a thousand times less than they are; provided they were placed at a proportionable distance from it. And 'twill be very hard to say, what considerable use the terrestrial globe, or its inhabitants, derive from that multitude of celestial spheres which compose the milky way; since each of those stars is so far from being singly able to enlighten the earth, that *Aristotle*, and the generality of philosophers, for many ages, took the whole number of them for a meteor. And what light, or other known advantage, can the earth, or its inhabitants, receive from those many fix'd stars, that the telescope, only, can discover, among the six or seven conspicuous ones of the *Pleiades*; or among those, which the naked eye discovers in the belt or girdle of *Orion* *?

I foresee, it may be said, that these and the like celestial bodies are, at least, thus far useful to man, as to discover to him the power and greatness of the divine maker. And, indeed, tho' perhaps his wisdom appears as great to us men in the structure of a glow-worm, as in the disposition of the small stars, that make up the galaxy; yet the immensity of his power could

* It seems difficult to say, what advantages the earth can receive from the comets; yet late discoveries have led us to conjecture at their use: and that most sagacious philosopher, Sir *Is. Newton*, supposes that one final cause of comets, is, "to recruit the seas, and the moisture in the planets, by a condensation of their vapours, and exhalations thereon. For as seas are absolutely necessary to the constitution of our earth, to the end that the heat of the sun may

"thence raise vapours in great plenty,
"which gathering into clouds, fall down
"in rain, and thereby water the earth,
"and fit it for vegetation; or else being
"condensed by the cold tops of the
"mountains, distil down in springs and
"rivers; so comets seem necessary to
"preserve the waters in the planets, by
"continually repairing, and supplying
"with their exhalations and condensed
"vapours, all that liquor, which being
"wasted in vegetation and putrefaction,
"is

PHYSICS. could not possibly be so well declared by less productions. These arguments, nevertheless, are not purely physical, but of that sort which I call physico-theological, whose inferences relate to the general designs of God in the universe, which I therefore stile cosmical ends; but do not reach to prove any thing about the determinate nature of particular bodies. And since the utmost that philosophy teaches, is, that in general, the good of man was one of the ends design'd by God in framing the world as we see it; there may, by the same omniscient author of nature, be other ends design'd of those telescopical, and other small or remote stars, whose uses to us are doubtful or inconsiderable; to attain which ends, those celestial bodies and motions may be admirably contrived and directed. And we being unable by mere reason to discover what those ends are, tho' we have nothing near so great cause to think there may not be such ends, as the infinite wisdom of God gives us to think there may; 'tis presumptuous to judge of the system of the world, and of the design of vastly remote fix'd stars, by its being greater or less advantageous to us: especially since tho' it were certain, that, among other uses, God intended they should in some sort be serviceable to us; yet he has no way declared in what capacity, or to what degree, they shall be so. And therefore, if they prove serviceable in any measure; that is, so far as we know, all he design'd they should be: and that itself, being an unmerited favour, deserves our humble thanks. And it seems very likely, that God did not design equal advantages to all the parts of the earth, from the present system of the universe; since the countries inhabited by the *Samoids* and *Nova-Zemblans*, and other nations that lie very near the arctic pole, want many conveniencies and advantages enjoy'd by the inhabitants of the temperate zones.

But tho' bare philosophy does not favour this bold opinion, yet I know 'twill be pretended that revelation does. And I readily confess, that the terraqueous globe and its productions, especially the plants and animals 'tis furnish'd with, appear, from the scripture, to have been design'd for the use and benefit of man; who has, therefore, a right to employ any of them he is able to subdue; and that the sun and moon were appointed by God to give light upon the earth, and be useful to all the nations that inhabit it: and that therefore the royal prophet had reason to exclaim,

“ is turn'd into dry earth upon them.
 “ For all vegetables wholly grow from
 “ liquids, and afterwards, in great mea-
 “ sure, change into dry earth by putre-
 “ faction; and a terrestrial part perpetu-
 “ ally falls from putrefied fluids.
 “ Hence the bulk of dry earth is constantly upon the increase, and the
 “ fluids, unless by some means supplied,
 “ must continually decrease, and at length
 “ be exhausted. But the vapours of comets
 “ being continually rarified in the
 “ empty celestial space, are diffused eve-

“ ry way therein, and spread thro' the
 “ whole heavens; afterwards they are
 “ gradually attracted by the planets, from
 “ their principle of gravity, and proba-
 “ bly intermix'd with their atmospheres.
 “ I farther suspect,” says this great philosopher, “ that the spirit, which is the
 “ least, the most subtile, and the best part
 “ of our air, and which is necessary to the
 “ life of all things, comes principally
 “ from the comets.” *Newton. Princip.*
 p. 472, 473.

“ how

“ how manifold are thy works, O Lord! how wisely hast thou made them all!” For he applies these expressions to the terraqueous globe and its inhabitants; as he elsewhere justly says, that “ the heavens declare the glory of God, and the firmament sheweth his handy-work.” But these general declarations, tho’ they properly excite our wonder and thankfulness, yet I fear, are not good topics, from whence to draw such physical conclusions in particular cases, as some learned men venture upon. For I do not remember ’tis any where declared in scripture, that the service of man was the only, or principal use, of all the celestial bodies. And this single consideration should make us very cautious, how we estimate the great system of the world by our conveniencies. And if it be said, that man alone has a rational faculty, whereby to refer the great works of God to the glory of their maker; I answer, that tho’ this has been affirm’d by many, yet I have never found it prov’d. And I somewhat wonder, that divines should, on this occasion, overlook that passage in *Job*, which they generally interpret of the angels. For the question which God there puts to *Job*, may be justly applied to *Adam* himself; “ Where wast thou, when I laid the foundations of the earth? declare, if thou hast understanding. “ When the morning-stars sang together, and all the sons of God shouted for joy?” And, indeed, if we may presume to conjecture at such things, it seems reasonable to me, that God created the angels before the material world, that he might have intelligent Beings to pay him the just tribute of praises for so admirable a spectacle, as that of the rising world, or the beginning and progress of the creation. However, it appears from these words in *Job*, that before man was made, God wanted not intelligent spectators and applauders of his corporeal works. And since the angels are a nobler order of intellectual creatures than men, and no unconcern’d spectators at the works of God; how do we know, that in the system of that part of the heavens which is invisible to us without the help of telescopes, and in the plants, animals, or other furniture, whatever it be, of those particular stars that serve us men, barely for declarations of their maker’s power; such intelligent spirits as angels may not discern as wise designs, and as admirable contrivances, as those manifested in forming and furnishing the earth? And in this case, God will lose none of the glory due to the divine attributes display’d in the fabric of the celestial part of the world, tho’ the fix’d stars should not be principally design’d for the service of man.

But, secondly, ’tis yet more unsafe to form arguments upon the nature of particular inanimate bodies in the sublunary world, from the uses we think them design’d for.

This will be made evident, by considering how little we know of the particular purposes of nature, in those terrestrial bodies, which being in-organical, cannot, by their curious structure, disclose to us the particular ends to which they were ordain’d. And their motions wanting that constancy and regularity of the celestial bodies, the caution given about drawing arguments from the astronomical system, will not, sure, be thought unfit to take place in clays, chalks, stones, &c. whose textures, compared with

And those that are terrestrial.

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with those of living creatures, are very simple, slight, and seldom more curious than may be made artificially, by dissolving stones and metals in chymical menstrua, and afterwards crystallizing the solutions. 'Tis true, revelation speaks rather of God's having destin'd animals and vegetables, than other inanimate bodies, to the service of men; yet there is no absurdity, to conceive, in the general, the same to be one of the ends design'd by the author of nature, in making metals, stones, and those other inanimate parts of the terrestrial globe, that man is able to master and make use of. But 'tis very unlikely, that the internal part of the earth, which may, for ought we know, contain great varieties of fossils, and other creatures, should be made chiefly for the service of men, from whose sight they lie hid; and who will, in all probability, never descend to a thousandth part of the depth requisite to discover them, and do not so much as know what kind of bodies they are. And tho' it will not hence follow, that the terraqueous globe was made by chance, any more than that the other plants were so; because the admirable structure of plants and animals proves the existence and providence of a most wise and powerful author of things, who may justly be supposed to have made nothing in vain, even among the inanimate portions of our globe; yet that those inanimate portions were made for determinate ends, is more easily deduced from the knowledge we have, by other means, of their being produced by a wise author, than from the contemplation of those bodies themselves. And, perhaps, it is worth inquiry, whether some things may not be made, even by a wise agent, not out of a primary intention, but as productions that will naturally follow, upon the establishment and preservation of those grand laws and rules of motion, that were most fit to be settled among things corporeal. And 'tis very possible, that, according to such a general establishment, many parts of the terrestrial globe are so disposed of, as not to be serviceable to men; because the whole mass could not otherwise be so well suited to the general ends of the universe. Thus, tho' the eclipses of the sun and moon be usually unwelcome, and, if astrologers may be credited, often prejudicial to men; yet "the great former of all things" did not think fit to alter the tracts or lines of motion that he assign'd the luminaries, to avoid the eclipses that must yearly ensue upon their moving in such lines. Whence we also learn, that some phenomena may not belong to the primary intention of nature; but are only the necessary consequences and effects of the primitive constitution of the world, and the universal laws of motion.

But if it be here demanded, to what end the deep and hidden parts of the terraqueous globe, and the telescopic stars of the firmament, were made, if not for the use of man? I freely acknowledge, that I cannot tell: and perhaps such an answer may be more expressive of the profound reverence we owe the great author of nature, than their opinion, who would have all these made for the sole use of man.

PROP.

PROP. II.

'Tis often allowable for a naturalist, from the manifest and apposite uses of the parts of animal bodies, to collect some of the particular ends for which the creator design'd them: and in some cases we may, from the known nature and structure of the parts, draw probable conjectures about the particular offices of them.

To obviate mistakes, it must be here observ'd, that I speak only of those ends and uses of the parts of an animal, that relate to the welfare and propagation of the animal itself, and which, therefore, I call animal ends; tho' I do not thereby deny any declaration made in the holy scriptures, that God design'd the entire animals, as well as their parts, to be serviceable many ways to man. This premised, I come to consider distinctly the two parts of the proposition.

And, first, there is no work of nature known to us, wherein the consideration of final causes may so justly take place, as in the structure of animal bodies. For my own part, I confess, that when I assist at a skillful dissection, I cannot but wonder there should be philosophers, who ascribe the admirable contrivance of a human body to blind chance. The Stoic, who in Cicero, ask'd an Epicurean why chance did not make palaces, and erect other buildings, seems to have propos'd a pertinent question. But the most commodious houses are far less curious structures than the human machine: for the materials of a palace are few, in comparison to the parts of an animal body; and their disposition exceeding slight, compared to the curious and elaborate contrivance of the numerous solids and fluids of the human structure: the former whereof, alone, are above some hundreds; yet in every one of these parts, the bulk, figure, consistence, texture, situation, connexion, and aptness for motion, are the most commodious that can possibly be devised; whilst all of them are wonderfully symmetrical, both to one another, and the whole body. And this number of parts is so artificially contrived and set together, that tho' no room be lost, many of them, at the same time, exercise very different motions, while each moves freely, and rather promotes than hinders the motion of others.

Hurts or diseases may shew how excellently all the parts of our bodies are contrived, in order to our welfare. For if even a finger be swell'd, displaced, kept in a wrong posture by contractions, have its continuity violated, its tone chang'd by strains or contusions, its sense or motion taken away, its membranes fretted by sharp humours, or its motions disorder'd by convulsions; we quickly find, how commodiously the parts affected were framed or dispos'd; their natural figure, connexion, tone, &c. being now alter'd.

The eye, to single out again that part for an instance, is so exquisitely adapted for seeing, and so little fitted for almost any other office in the body; and that use is so necessary for the welfare of the animal, that it may well be doubted, whether any considering man can really think it not designed for that use. The six or seven muscles, which move the whole ball

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But for the uses of the several parts of the eye, I refer my reader to *Scheiner's Oculus*, and *Des Cartes's Dioptrics* ; whence it will appear, that, in forming this part, nature not only acted with design, but with so great skill in optics, that a more than ordinary acquaintance with that science is necessary to understand the wisdom of the several contrivances ; which, perhaps, no degree of skill whatever in it, would enable a man to alter for the better.

'Twere tedious to mention other parts of the body, that manifestly appear to have been ordain'd to certain uses. The books of anatomists are full of passages to this purpose : of which I shall only say in general, that tho' what they deliver is sufficient to shew all the parts of the body to be the effects of an intelligent cause ; yet, unless their descriptions, and reflections, be improved by mathematics, mechanics, and chymistry, we shall have but an imperfect notion how intelligent that cause is ; or how much wisdom is display'd in the structure of a human body, and its several parts.

I know 'tis objected, by the *Epicureans*, that the parts of animals were first made, and their uses afterwards discover'd, by the sagacity of men. But this is a sophistical objection. For, first, many of the internal parts perform their functions, without our having any knowledge of their structure, or situation ; so far are they from being applied to such uses by our sagacity. And as for the limbs, and other parts, which we move at pleasure, 'tis true, they cannot be employ'd to their respective uses, till actually form'd ; nevertheless, they might be originally so form'd, as, in due time, to be fit for such uses. And, in effect, we see that a chick is furnish'd with compleat eyes and wings, before it be hatch'd ; tho' whilst inclosed in the egg, it can make no use of them, either to see, or fly. And why was it, do the *Epicureans* think, that nature provided a whole sett of temporary

temporary parts for pregnant females, and animals in the womb ; which, when they are come into a freer state, partly shrink away of themselves, and partly turn to a ligament, fitted no longer for the former, but for a more seasonable use ? And 'tis to be noted, that these umbilical vessels, and the placenta to which they are fasten'd, is of no necessity or use to the female before conception ; and therefore, those temporary parts, appear to have been design'd by nature for the propagation of the species.

And tho' the sagacity of men, may have found out some uses of the particular parts of their bodies, which seem not to have been primarily intended by the author of nature ; yet this does not prove, that those uses were undesign'd : for the prescience, and goodness of God, are sufficient to render it probable, that he who gave man both the limbs of his body, and the endowments of his mind, did both foresee what uses men might, according to their sagacities, and emergencies, make of these parts ; and so contrive the parts, that they should be applicable to such uses.

And, we may now proceed to the latter part of our proposition ; which asserts, that, in some cases, from the known ends of nature, as well as from the structure of the parts, probable conjectures may be form'd about the particular offices of them.

This could not have been seasonably spoken to before ; because the arguments that were founded on the uses of the parts of animals, suppose those parts to have been destin'd to particular uses, knowable by us ; and that the several parts of the body were contrived as wisely, and commodiously, as men are able to devise, in order to the ends of nature : which must always be understood to have united in her designs, the uses of the parts, and the welfare of the whole.

And, indeed, if we consider how admirable a fitness there is in the parts of a human body, to those particular ends we can discover them to have been design'd for ; it seems allowable to conjecture, that such a part was not primarily design'd to such an use, if it is, on the account of its structure, or otherwise, less fitted for it than the constant wisdom of nature seems to require ; especially if there be any other parts, by which the office may be more commodiously performed. And, on the other side, it seems probable, that such a part was destin'd to such an use, if the use itself appear to be necessary, and the part better fitted for it than any other.

Thus, tho' anatomical and optical writers, for many ages, unanimously concluded, the crystalline humour to be the principal seat of vision ; yet the industrious *Scheiner* justly rejects that receiv'd opinion, upon shewing, that it suits not with the skill and providence of nature, that it should be so, since it wants many requisite qualifications for that purpose ; and especially since most of these are to be found in the retina. And, I remember, upon asking our famous *Harvey*, what induced him to think of a circulation of the blood ; he said, that observing the valves in the veins of many parts of the body, so placed, as to give free passage to the blood towards the heart ; but to oppose the passage of the venal blood, the contrary way ; he imagin'd

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Thus, tho' the ancient anatomists, and physicians, believ'd the parts were nourished by the venal blood ; the modern writers teach them to be nourished by the blood, in its passage thro' the arteries. Not that they think the blood, which runs thro' the veins, altogether unfit to supply the parts with that vital liquor ; but because they judge the veins to be less fit for this purpose, than the arteries ; into the latter whereof the blood comes immediately from the left ventricle of the heart, agitated, and spirituous, and, by a brisk impulse, better suited to answer this end.

But the writings of physicians, and anatomists, being filled with instances of this kind, I forbear to mention any more.

P R O P. III.

It is rational, from the manifest fitness of some things, to cosmical, or animal ends, to infer, that they were thereto ordained by an intelligent agent.

Care seems to have been taken, that the body of an animal should be provided not only with all things that are ordinarily necessary, and convenient ; but with some super-abundant provision against accidents. Thus, tho' a man may live, and propagate his species, after the loss of an eye ; yet nature furnishes us with two, that, in case one be destroy'd, the other may suffice for vision. And the like may be said of the ears. In short, nature has furnish'd men with double parts of the same kind, where it is highly useful, and may be permitted, without prejudice, to the rest of the body. And this is the more considerable, because, in other parts, nature appears to husband things so, as to avoid doing what is superfluous. Thus, within the skull, some vessels, that would, in other parts of the body, have double coats, are very thin ; the skull being ordinarily sufficient to defend them from external injuries.

Another argument, that nature acts with design, about animals, may be drawn from what anatomists observe of those parts of the womb, or the fœtus, that are to be found but at certain times, when there is need of them ; and not at others, when they would be useless. Thus, when a woman is with child, the *Vasa umbilicalia* are produced to be canals, either for the blood, or alimental juice, and spirits, that then ought to pass between the womb and the fœtus, by means of the placenta. And tho', as long as the child continues in the womb, these temporary parts continue with him ; yet, as soon as he comes into the world, the umbilical vessels, particularly the two arteries, and the vein, together with the membranes they are wrapp'd up in, with the chorion, and the amnios, that involve the fœtus, are thrown off, as unnecessary, and expell'd in the after-birth ; there remaining only that part of the umbilical vessels that lies

lies within the child's abdomen, between the navel and the liver ; where its use is considerable, tho' new ; for it serves now no longer to convey blood, or an alimantal liquor, to and fro, but degenerates into a ligament.

Thus, also, the *Foramen ovale*, gives passage to the blood from the right ventricle of the heart to the left, that the circulation of it may be maintain'd ; for it cannot in the embryo, as in a born child, pass thro' the vessels of the lungs, from one of the ventricles to the other : whence this contrivance seems to be an expedient that nature employs, till the foetus is excluded ; when that temporary conformation is obliterated. For the child, now breathing the free air, is in a condition to make the blood circulate thro' the pulmonic vessels, according to the primary intention of nature. From which, and the like instances, we may infer, that these temporary parts were framed by a fore-knowing, as well as a designing agent ; who intended they should serve for such a purpose, and then be laid aside : for 'tis utterly improbable, that an undesigning agent should so appositely, and exquisitely, frame scaffolds for a future building, if he did not, before-hand, destine both the one and the other, to concur to the same ultimate effect.

Another argument, for our present purpose, may be drawn from the consideration of what, in animals, is commonly call'd instinct : which, in some cases, more directly regards the welfare of the creature ; in others, the propagation of the species ; and, sometimes again, respects both. The writers of voyages, and natural history, recount strange instances of the instinct observable in certain animals. But we need not lay the stress of our argument upon dubious, or suspected relations ; since what I have met with, in authors of good authority, or receiv'd from the mouths of credible travellers, may serve my present turn ; especially, if we may take the word instinct in a latitude, so as to comprize those untaught methods, and expedients, that are made use of by some animals, to avoid dangers, provide for their future necessities, or to catch their prey.

Surprizing things are related, not only by poets, but by more credible writers, about the sagacity and government of bees ; in point, both of œconomy, and politics. But tho' I shall not build any thing upon suspected authorities, yet, having long kept a transparent hive, and thereby gained the opportunity to make frequent observations of the actions of these little animals ; I confess, I discover'd some things that I did not believe before : which induced me to look upon them, as very fit instances of creatures endow'd with natural instinct and providence. For, 'twere hard for a mathematician, in contriving so many cells as bees make in the area of one of their combs, to husband so little space more skilfully than these animals. And they not only carefully, and seasonably, lay up their honey, to serve them all the winter ; but, curiously, close the particular cells with covers of wax, that keep the included liquor from spilling, and from external injuries. I do not here mention the prognostication of weather that may be made in the morning, by their keeping within their
hives,

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hives, or flying early abroad, to furnish themselves with wax, or honey; or, by their unexpected return before a storm: because, I suspect, that these things are not so much the effects of instinct, as of a tenderness, and quickness of sense; something analogous whereto, may be seen in a good weather-glass; and is, also, to be found in many wounded, and valetudinary persons, who are affected with such beginning alterations in the air, as other men perceive not. But, among the peculiarities to be observ'd in the conduct of bees; 'tis very remarkable, that, after a fight, they take up the dead which lay on the ground, and, as I have observ'd, fly away with them far from their hive.

Another obvious instance of the instinct that nature has given to some despicable insects, may be taken from ants. For, 'tis known, that these little creatures do, in the summer, hoard up grains of corn against the winter. And their sagacity is the more considerable, if it be true, what many learned men affirm, that they eat off the germen of the grains they lay up, lest the moisture of the earth, exposed to the rains, should make them sprout. But, whatever become of this tradition, these insects perform some other actions, greatly resembling those proceeding from sagacity and industry.

The natural skill of spiders, in weaving their webs, that are so fitly contrived, both to catch their prey, and give them immediate notice of its being caught, is a thing which, if it were not familiar, would be look'd upon as admirable. And this skill is not, as some imagine, an effect of imitating their parents; for if the eggs be taken away, and enclosed in a glass; when they come to be hatch'd by the heat of the sun, the little creatures will, immediately, fall to spinning in the glass itself; as was related to me by an eminent mathematician, who made the experiment. And I saw the less reason to distrust it, because having, by an external heat, hatch'd many eggs of silk-worms, in a place where there had not been any of a long time before, nor, probably, ever, till then; yet the worms, produced by these eggs, did, in autumn, of their own accord, climb up to such convenient places as I had prepared for them, and there weave those curious oval prisons, wherein they enclose themselves, and which are unravel'd into extremely fine silk.

But this provident industry is not confined to insects, for 'tis to be found in many of the greater animals; particularly in the beaver; a creature whereof, indeed, many fabulous stories are related: yet sober, and judicious persons, who were either born, or lived in *New-England*, where these animals abound, have assured me, that the beavers, with their sharp teeth, cut pieces of wood, and fit them to their purpose; that, by joining their labours, they lay these together, so as to build themselves strong winter-houses; in which there is, sometimes, a kind of second story for the inhabitants to retire to, when the water chances to overflow; that, for these houses, they chuse a very convenient situation, just by some river, or other water, that can furnish them with fish; and that the hole belonging to each house, is placed just by the water, that they

they may, immediately, flounce into it, and so save themselves, when their houses are attack'd. And, to facilitate their swimming, and the catching of their prey in the water; nature has given them two feet, not made like those of dogs, or cats, or in the manner of their other two; but furnish'd with broad membranes betwixt their toes, like the feet of geese, ducks, and other aquatic animals, that are to use them as oars, to thrust away the water, and facilitate their motions.

The various arts employ'd by animals of different kinds, about the materials, the construction, and the situation of their nests, is usually remarkable, and, sometimes, wonderful. Of this skill, we have many eminent instances; but 'tis particularly remarkable, that, in countries abounding with apes and monkeys, creatures very greedy of birds eggs, there is a sort of bird, whose eggs they, peculiarly, affect, that hang their nests near the end of some long flexible branches, which reach over the water; and, by that means, avoid their enemies, who cannot swim.

In the nests of wasps, which they often, for greater security, make under ground; I have observ'd a very curious, and artificial structure, to conceal, and shelter their young ones, till they are ready to fly.

But the instinct that nature has planted in animals, for their own preservation, is much inferior to that providence she has furnished them with, for the propagation of their species.

There are many remarkable things to be met with in the nests of several birds, both as to the materials, the structure, and the situation of the places wherein they are built. I have seen nests, and, particularly, some made in the *Indies*, which would raise a man's wonder how the birds should find such odd, but commodious materials to build with. There are birds in the east, which make their nests of a white substance, that looks almost like ising-glass, dissoluble in liquors, and so very well tasted, that it makes the chief sauce used in the southern parts of *India*. The structure, also, of the nests of several birds, both as to their figure, magnitude, and accommodations, wherewith they are furnish'd, for warmth and softness, may deserve the applause of mathematicians; especially if it be consider'd, that these little untaught architects had no tools to make their curious buildings with, except their bills and feet. Much more foresight, however, appears in the situation of the place, that some birds make choice of, to build their nests in; as may be observ'd, not only in the pendulous nests of swallows, and the secret ones of some *European* birds; but very conspicuously in the hanging nests that we lately mention'd, to be so oddly placed by some birds, to secure their eggs from apes and monkeys; and by the situation of the nests used for sauce as we just now related, to be found only upon high and steep rocks: which are so fasten'd to the concave parts of them, that look downwards, and, commonly, hang directly over the sea; that there is no getting them, without much trouble, and danger: upon which account, as well as that of their delicacy, they are very dear, even in the *East-Indies*, where they are found.

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The like care to contrive their nests advantageously, and make them in secure places, is visible in insects; as may be observ'd in the subterranean nests of the wasps, formerly mention'd, and in the eggs of snails, which I have sometimes found hid under-ground; and have hatch'd in glasses furnished with the same earth wherein they were found.

If I should here set to view the several effects and arguments of the wonderful providence, that the most wise author of nature exercises about the propagation of animals, by distinguishing them into male and female; by furnishing both sexes with mutual appetites and organs exquisitely adapted to the increase of their kind; by the admirable formation of the fœtus in the womb, without the female's knowing how it is perform'd; by the strange subtilties and courage, that several creatures, either oviparous or viviparous, have, to hide and defend their young; by the full provision that is made for the nourishment of the fœtus, and the welfare of the female after she has brought forth; and the like: I might much enrich and adorn my argument. But instead of pursuing a speculation, that would lead me too far, I shall look back upon the intimation I lately gave, that even those meaner parts of animals, which seem to have been framed with the least care and contrivance, are yet worthy of their author. For tho' the teeth be some of the least elaborate parts of the human body, yet even these afford numerous phenomena applicable to our present purpose: but I shall only transiently consider a few of them.

And first, 'tis remarkable, that tho' when a man comes to his full stature, all the other bones of the body cease to grow, the teeth continue to increase, in length, during his whole life. This growth of the teeth appears not only by their continuing for many years of the same length, but by the unsightly shooting of one tooth, when that which was opposite to it in the other jaw is wanting; whence it has liberty to sprout without opposition. Of this difference in point of growth betwixt the teeth and other bones of the body, what reason can be so properly assign'd, as its being intended to repair the daily waste of substance, proceeding from the frequent attritions of the upper and lower jaw in mastication?

2. Tho' the bones of the body are generally invested with a very thin sensible membrane call'd *Periosteum*; that part of each tooth, which is not cover'd by the gums, wants it; which would be subject to very frequent and painful compressions and lacerations.

3. To enable the teeth to break and make comminutions of the more solid kind of aliment, nature has providently framed them of a closer and harder substance than almost any other bones in the body; tho' these are so numerous, that anatomists reckon above three hundred of them. And I have met with it in authors of good credit, that some mens teeth have been so hard, as, when struck against another fit body, to produce sparks of fire.

4. That these bones, whose use is so great, may themselves be continually nourish'd and supplied; tho' they are set in bone, the wise author of things has admirably contriv'd a cavity on each side of the jaw-bone, wherein

wherein are lodg'd an artery, a vein, and a nerve ; which, thro' lesser cavities, fend their twigs to each particular tooth.

5. As infants are design'd, by nature, to feed, for a considerable time, on milk, they remain long without teeth ; whilst several animals, which are often early reduced to seek aliment, that is neither fluid nor soft, come into the world with teeth already form'd in their jaws.

6. The bony substance appointed for the comminution of the aliment, ought not, for several reasons, to be in either jaw entire, or all of one piece ; and therefore nature has providently made, for that use, a competent number of distinct bones in either jaw. And because men may have occasion to feed often upon very different sorts of aliment ; and because usually the same aliment may require different preparations in the mouth, to facilitate the digestion of it in the stomach ; nature has provided men with two rows of teeth, for the most part equal in number, and corresponding to each other, yet of different shapes, for different uses ; the fore-teeth are broader, and have a kind of edge, to cut the more yielding sort of aliment, whence they are call'd *Incisores* ; others being stronger, and more fitly shaped to tear the tougher food, are term'd *Canini*. There is also a third sort, whose principal office it is to grind the aliment cut or torn by the others : and, for this purpose, they are made much broader, and somewhat flattish, with their upper surfaces uneven and rugged ; that by their knobs and little cavities, they may the better retain, grind, comminute, and mix the aliment ; and for this reason they are call'd *Molares*.

7. And because the operations to be perform'd by the teeth, often require a considerable firmness and strength, partly in the teeth themselves, and partly in the instruments that move the jaw, wherein the lower sett of them is fix'd ; nature has provided the lower, or moveable jaw, with strong muscles, that it may bear forcibly against the upper ; and has not only placed each tooth in a distinct cavity of the jaw-bone, as it were in a close, strong, and deep socket ; but furnish'd the several sorts of teeth with hold-fasts, suitable to the stress, that, by reason of their different offices, is to be laid on them. And therefore the *Incisores* and *Canini* have usually but one root ; whilst the *Molares*, that on many occasions are employ'd to break hard bodies before they can be ground, are furnished with three roots, and often with four, in the upper jaw, whose substance is somewhat softer, and whose grinders serve as so many little anvils for those of the lower to strike or press against.

If it shall be said, that allowing, tho' man is indeed advantageously furnish'd with teeth, yet there are many other animals, some whereof have not near so many teeth, nor so commodiously shaped and placed as his ; others, that are not furnish'd with any teeth at all ; and that several animals have some of their other parts less convenient in their kind, or are not otherwise near so well provided for as they would be, if they were not rather the productions of chance, than of an intelligent and designing agent : I must declare, that many particulars might be farther alledg'd, to shew the final causes of things corporeal, as the consideration of them leads

PHYSICS. to a high veneration of their divine author; and to manifest, that when his providence is deny'd or condemn'd, 'tis for want of due consideration.

But I have already, in great measure, prevented myself in the answering this objection, by obviating some exceptions relating to the eyes of man and other animals. For the considerations that have been alledg'd to that purpose, may, *mutatis mutandis*, be applied to the varieties to be found in the teeth, and other parts, of different kinds of creatures.

'Tis known, that oxen, sheep, &c. are not furnish'd with near so many teeth as men, horses, dogs, &c. But this defect, if it be any, is supplied partly by the power and instinct they have to chew the cud, and thereby make a second attrition of the aliment, that is already greatly soften'd; and partly by the successive cavities, or stomachs, thro' which that aliment is transmitted, and by that means elaborated and fitted for further uses. The beaver, the tortoise, the bee, and the humming-bird, which sucks the exudations of flowers with his little long bill, like the bee, and many other animals, have their mouths, and their ways of preparing their aliment for the stomach, very different from what is observ'd in men; and yet each respectively very convenient; all circumstances consider'd.

These, and the like expedients, are in many animals such as afford no cause of taxing the author of nature, for not having given some of them all the same parts wherewith others are furnish'd; but rather, the providence and wisdom of God, in the contrivance of his visible works, may be as well discover'd by the seeming omission of this or that part, useful to other animals, but unnecessary to those wherein it is not found; as by granting those parts to such animals, whose compleatness or welfare they are necessary or highly conducive to: and therefore 'tis not strange, that he has not given to men, and many sorts of birds, such tough, transparent, and moveable membranes, as frogs are provided with, to defend their eyes from such accidents as the others are not usually expos'd to.

Bats are esteem'd a contemptible sort of creature, yet they may afford us a considerable argument to our present purpose. For we have here an animal that flies like a bird, tho' it wants feathers, and has a fabric quite different from that of birds. And here too, we may observe the compensation that is made for parts that seem either deficient, or less advantageous, than those of the same denomination in other birds; as also the regard which the divine artist appears to have to the symmetry of parts in his animated works, and to their fitness to the places they are to frequent. For the bat being to act sometimes like a bird, and on some occasions like a terrestrial animal, or mouse, he ought to be furnish'd with parts suitable to such different designs: and, therefore, the want of feathers in his wings is supplied with a broad membranous expansion, and a kind of toes furnish'd with articulations. And because this animal was to forbear settling on the ground, otherwise than his occasions required, each of his wings is furnish'd with a strong crook, like the claw of a bird's foot; by the help of which, he can fasten to trees, walls, &c. and there keep himself at what distance he pleases from the ground. And as he is furnish'd with

with teeth, which other birds want, to chew his food, and thereby prepare it for digestion; he needs no crop, or such a strong muscular stomach, as is usually found in birds.

In short, to omit the peculiar structure of other internal parts, wherein the bat differs from other birds; since the female was not, like them, to be oviparous; but, like mice, and other quadrupeds, to bring forth her young ones alive; she is not only furnished with an uterus, fitted for that purpose; but, in regard she does not exclude, together with the fœtus, a competent stock of aliment, to nourish it, till it can shift for it self; the bat is furnished with teats to give suck: and 'tis observ'd, that as she has but two of these, so she brings forth but two young ones at a time; while mice are much more prolific.

The writers of natural history, and travellers, afford numerous instances of the various, and excellent contrivances, that are to be found in animals differing from man, in the fabric of the mouth, and other parts, subservient to the preparation and digestion of their aliment.

But, to be silent, as to the mouths of tortoises, camelions, &c. the hardness of whose gums, with regard to their aliment, supplies the want of teeth, there is an *American* beast, which serves as a notable instance, to manifest how the wise architect can compensate the want of teeth, by the rare structure of the mouth and tongue; and their fitness to seize, and make use of that aliment, which, tho' uncommon for a beast of his bulk, he seems to have been destined to live upon. The animals of this kind are, by *Hernandus*, call'd, *Achoas*, and *Tamendoas*; and, of these, he says, "they are quadrupeds, about the size of lambs, whose bellies almost touch the ground; that they have no teeth at all, but live upon ants; whose hillocks they turn up with the two large claws they have fixed to their fore-feet, and then thrusting out their long rough, cylindrical tongue, they roll it upon as many as they can, and so draw them into their mouth, which is exceeding narrow." *Piso* adds, that "their tongue, which is sometimes two feet in length, lies doubled up in a cavity, between the sides of the lower jaws; which, in order to catch their prey, they thrust into the trunks of trees, and there suffer it to continue, with a moisture upon it, till cover'd with ants; and then suddenly draw them with it into their mouths."

'Tis also to be consider'd, that many things may be useful, in an organical part, besides those whereby its office is primarily and chiefly perform'd. Thus, tho' the eye-lids, and their motions, together with the little glands that belong to them, are not at all necessary to the act of vision; yet they are to the compleatness, and welfare of the eye: as is manifest by the pain and prejudice that organ receives, if the eye-lids be considerably disorder'd.

But, besides those uses of the parts of a human body, which I call anatomical, because they are such as anatomists have discover'd, by mere dissection; there may be several others, which I term chymical, because these parts prepare spirits of several sorts, and, perhaps, perform other

PHYSICS. operations, very important, if not necessary, to the welfare of a living man.

And there may still be other uses, very proper to be consider'd, in some parts of a human body; as the mechanical advantages, for which the various shapes, and structures of different muscles, the seeming irregular fabric of the bones, and, especially, of the processes, and protuberances, are admirably fitted. And, in some parts, too, there are peculiar ones; as for instance, optical ones in the eyes; which must be consider'd, before the design of nature therein, can, without rashness, be censured.

Thus, tho' the figure of the crystalline humour, be much more spherical in most fish, than in men, and other terrestrial animals; yet he who understands the doctrine of refractions, and considers, that fish under water, are to see thro' a far thicker medium than air, will readily acknowledge, that this difference between their eyes, and those of men, is not an imperfection in the former; but, whilst those creatures are in their own element, a great advantage.

And, to be short, I think there are so many sciences, and other parts of knowledge, some of them, perhaps, scarce yet discover'd, that may be requisite to warrant a man in censuring the ends of God, in the bodies of animals; that very few men are qualified to condemn them justly; and those who have knowledge enough to judge right, will not be forward to condemn, but admire them. Thus men may easily be too rash, in thinking a part inartificially framed, upon supposition, that by the anatomical dissection of it, they know all the uses whereto the skill of the divine artist could design it.

And, it will not necessarily follow, that because in some particular bird, or beast, or fish, we are unable to say why this, or that part is not to be found; or why it is otherwise framed, or situated, than that which is analogous thereto in man; it must, therefore, be casually, or undesignedly framed, or placed: since we cannot expect from brute animals, answers to those proper questions about their own bodies, which we can receive from men about theirs. Nay, notwithstanding the great diligence, with which the more curious physicians are obliged to cultivate anatomy, and the frequent opportunities they have to do it, and to inquire of living men about what they observe, and feel, when the natural use of their parts is hinder'd, or perverted; yet we are, to this day, at a loss, as to the true uses of the visible parts of the body; to say nothing of the invisible ones. So that 'tis no wonder, if, in animals, whose fabric we have much less concern to inquire into, and much less opportunity to examine, we, sometimes, find parts, of whose uses and fitness, men are, hitherto, unable to give a satisfactory account. For even in man, himself, tho' there be numerous valves found in his veins, yet, for those many ages, that the true uses of them were unknown, an *Epicurean* physician might have thought he had reason to look upon them as superfluous parts; which, since the circulation of the blood is discover'd, are acknowledg'd of the greatest utility.

And,

And, since God is both a most free, and a most wise agent, it need not seem strange, that he should adorn some animals with parts, or qualities, that are not necessary to their welfare, but that seem design'd for their beauty ; such as the disposition of the camelion to change his colour ; and the lovely green, blue, yellow, and other vivid colours, that adorn some sorts of pigeons, parrots, and the smaller birds ; and especially, that admirable little creature, the humming-bird.

On the other side, God's wisdom sometimes seems to be provident, and solicitous, not to bestow on an animal, or a part of it, more than is necessary for the use whereto either is design'd. Thus the veins are, by anatomists, observ'd to have but one coat, and usually to lie more exposed than the arteries that accompany them, which have stronger, and double coats, because they are to convey a more important liquor ; which liquor, besides that 'tis more agitated, and spirituous, is forcibly impell'd into them, by the strong muscular contraction of the heart. And, 'tis observable, that tho' the nerves usually lie deep, to be kept both safe and warm ; being very liable to be offended both by cold, and the contact of external bodies ; yet, as 'tis necessary that the optic nerve should expand itself into the eye, the membranes which invest that nerve, and other coats of the eye, except the retina, are made vastly more firm than the *Dura*, and the *Pia mater*, whence they proceed ; and tho' exposed to the free air, are less sensible of the cold, than most parts of the body ; and will bear, without danger, several liquors, and other offensive things, whose pungency would put other nerves of the body into convulsive motions.

This conduct makes it seem, as if God, like an excellent writing-master, did, in the great volume of his creatures, intend to bestow on some of them, things rather ornamental, than necessary, as flourishes on the capital letters of nature ; and sometimes to employ characters of different shapes, to stand for the same letters ; and sometimes, also, to employ abbreviations, to express, in short, that which might be very justifiable, had it been more fully delineated.

And, if we allow, as 'tis very probable, that God design'd, by the great variety of his works, to display to his intelligent creatures, the great abundance of his wisdom ; 'tis obvious, that much of the variety, observable in the analogous parts of animals, may be very conducive to so comprehensive a view ; whereto the beauty of some creatures, and parts, as well as their more necessary, or convenient structure, may be subservient : especially if the innocent delight of man be also intended ; as it seems to be, in the curious colours, and shapes of several flowers ; in the melodious music of birds ; and, in the vivid, and curiously variegated colours of the feathers of several winged animals ; particularly those that make up the peacock's train.

'Tis worth our observing, that we are not near such competent judges of wisdom, as of justice and veracity : which are estimated by eternal and fixed bounds, or rules, very intelligible to a moderate understanding. But, as for wisdom, the more profound it is, the less we are able to look thro'

PHYSICS. thro' it, penetrate to the bottom of it, and judge knowingly of its actions. And therefore, tho' we may safely conclude, that God acts wisely, when he does any thing that has an admirable tendency to those ends, we justly suppose him to have design'd; yet we cannot safely conclude in the negative, that this, or that, is unwise, because we do not discern a wise tendency therein. For so wise an agent may have other designs than we know of, and further aims than we can discern, or suspect; and may have at hand, or furnish himself with such means to compass his ends, and that even by the co-operation of what we think useless, or improper, as are far above the reach of our conjectures; and without the knowledge of which, we rashly censure the wisdom of his proceedings.

In the double horizontal dial, formerly mention'd, it would be rash of any to condemn, or despise, the various lines they find traced upon that useful instrument, because they see they are not necessary to shew the hour of the day; since the mathematician, who drew those lines, may be well supposed to have more ends than one, or two, in making the instrument; and not to have drawn them by chance, or unskilfully, tho' the inconsiderate censurers do not know for what other, or farther purposes, the artist may have design'd them.

Suppose some *Indian* fisher-man, unacquainted with *European* arts and affairs, should come on board a man of war, under sail; he would quickly perceive, by the use made of the tackling, that this floating building was very artificially contrived; yet, if he should fix his eyes upon one of the guns, or the anchors, and perceive that no use was made, or likely to be made of them, in sailing, he would be strongly tempted to think, that those heavy masses were useless clogs, and burdens to the vessel. But if he were told the necessity, and usefulness of the guns, for defence; and of the anchors, to stay the ship in storms; he would presently alter his mind, and confess, that he had blamed the contrivers for that, which nothing but his ignorance kept him from commending.

I have dwelt the longer on this third proposition, because I think it a duty, that our reason owes to its author, to endeavour to vindicate his manifold wisdom, in this libertine age; wherein too many, who have more wit than philosophy, labour upon *Epicurean*, and some even upon *Cartesian* principles, to depreciate the wisdom of God; whilst others presume to censure his contrivances, in the bodies of animals.

I had also a desire, that the reader should not barely observe the wisdom of God, but actually be convinc'd of it. To which purpose, in my opinion, 'tis very conducive, if not necessary, besides general notions, to observe, with attention, some particular instances of the divine skill, wherein it is conspicuously display'd. 'Tis true, that in the idea of a Being infinitely perfect, boundless wisdom is one of the attributes included: but, for my part, I am of opinion, that this general and indefinite idea of the divine wisdom, will not give us so great a veneration for it, as may be produced in our minds, by knowing, and considering the admirable

nable contrivances of the particular productions thereof; and their exquisite fitness for those ends and uses, to which they appear to have been design'd.

P R O P. IV.

We ought not to be hasty in concluding upon the particular use of a thing, or the motive which induced the author of nature to frame it in a peculiar manner.

It has been already shewn, that some parts are so excellently, and manifestly fitted for a certain use, and so much better adapted to that, than to any other; as to make it seem perverse, to doubt of its being design'd thereto. But the like cannot be said, in general, of all the parts of the body; especially of the internal. There are many uses, either necessary, or highly conducive to the welfare of the animal, that have no part so much more conspicuously fitted for them than another, as to leave it easy to determine the true, and primary offices thereof; especially, with so much certainty, as thereon to ground philosophical inferences. And, of this difficulty, I conceive, there may be four reasons; tho' they do not all occur in each particular case.

And, first, the whole animal itself, that we consider, is a part of the universe; and, therefore, cannot well be supposed to have been framed, and furnish'd with the parts it consists of, merely for its own sake. And, when we say, that all its parts are contriv'd to the best advantage to the animal; I conceive, this is to be so understood, in a limited sense; that the parts are excellently framed for the welfare of the animal, as far as that welfare is consistent with the general ends of the author of nature, in the constitution, and government of the universe; which ends, because they relate to the whole world, I call cosmical. It has not, indeed, been prov'd, that none of these cosmical ends are discoverable by us: however, to discover them all, is no easy task. Yet, it seems presumptuous to suppose, that the welfare of particular animals is any farther design'd, and provided for, than will consist with the cosmical ends of the universe, and the course of God's general providence; to which his particular providence ought, in reason, to be subordinate. And, tho' it seems great rashness in men, to determine positively, and exclusively of others, what ends the omniscient creator proposed to himself, in giving to the world its present frame; yet, as far as I can hitherto discern, I see nothing more likely to have been one grand motive of so great a variety, as we observe in his corporeal works, especially in animals, than that he might, by so many very different contrivances as are to be met with in the structure of men, quadrupeds, birds, fish, reptils, &c. exercise and display his manifold wisdom. As man, even upon account of his body, is acknowledg'd the most perfect of animals; if God had barely design'd to give every creature the most advantageous structure, it seems, that he should have made no other animals than men. But, then, there could not have been that diversity of contrivance among living automata, which so greatly recom-

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PHYSICS. mends the wisdom of him, who could frame so many, and so differing animals; tho' not all equally perfect, yet all admirably furnish'd for those purposes to which he design'd them. It does not, therefore, argue any want of providence, that he has not furnish'd man with wings, fish with feet, and birds with fins and scales; because these parts would have been either superfluous, or burdensom, or unsuitable to his design of fitting some animals to live on the earth, and others in water: and if he design'd any to inhabit both, he furnishes them with parts of a peculiar structure; as was formerly noted of the beaver. Were it not for this consideration, it would be hard to assign the reason why vegetables are not the food of all animals; but that some should be carnivorous, and furnish'd with appetites, and organs, to devour others, and live, as birds and beasts of prey, upon the weaker. And 'twill be hard to shew, why, even in animals of the same kind, the safety of some should be so much better provided for than that of others; as we see some ants, and glow-worms, furnish'd with wings, and others not. And, even in our own species, those of the female sex are not so happily framed, in order to their own welfare, as males; since the uterus, &c. which are not necessary to the good of individuals, but to the propagation of the species, subject that tender sex to a sett of diseases peculiar to them. So that men may, sometimes, mistake, when they peremptorily conclude, that this, or that part of an animal, must, or cannot have been framed for such a particular use; without considering the cosmical, primary, and over-ruling-ends, that may have been design'd by nature, in the construction of the whole animal.

2dly, Men sometimes, erroneously, conclude, that such an office cannot belong to such a part, because they think it is not so commodiously framed for it, as might be wish'd; without considering, whether the structure they propose, would not, in some other as considerable respect, oppose the welfare of the animal; or, whether it would be consistent with the other uses, design'd by nature, in that sort of creature. For, in the living works of so excellent an architect as nature; it must not be expected, that any particular end should be prosecuted, to the prejudice of the whole: but it must rather be suppos'd, that she aims, not only at particular expedients, but universal symmetry; while she excellently fits the several parts for their respective offices; tho' only so far as a due regard to the design, and welfare of the whole, will permit. Thus, as we before observ'd, tho' man be allow'd the most perfectly framed of any animal in the world; yet his body is not made the model for nature to form the correspondent parts of other animals by. The lungs of a dog, a bird, a frog, a viper, &c. are of a structure very different from those of man. He is not furnish'd with so many stomachs as an ox, or a sheep; because nature intended not that he should ruminate like them. And tho' his gall be lodg'd in a peculiar bag, within the liver; yet 'twere rash to say, that the secretion of the bile is none of the uses of those livers, wherein such a cystis is not to be met with: since, in some animals, as

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in horses and pigeons, that bitter humour is not usually, as in man, collected into one bag: and tho' it be so in vipers; yet, as far as I have observ'd, the containing cystis does not at all touch the liver.

3dly, 'Tis difficult to determine the true and primary use of a part; because nature often fits one part for several uses.

And, 4thly, it sometimes increases the difficulty, that nature may compass the same ends by several means; each of them sufficiently commodious. I join these two observations together, because, in effect, they often concur to render it hard to determine the true use of a part. Neither does nature constantly employ only one part to perform a particular office; but the design'd effect is, sometimes, produced by a series of successive operations, to which several parts may differently contribute.

Neither the mechanism of a human body, nor of very considerable parts thereof, is to be assign'd from the bare structure of the visible matter, whether solid, or fluid, to be found in the vessels, and cavities of a dead body, skilfully dissected. For the body of a living man is a very compounded engine, many of whose functions are perform'd, not by the blood, and other visible fluids, barely as they are liquors; but, partly, by their circulation, and other motions; and, partly, by a very agile, and invisible, sort of fluids, call'd spirits animal and vital; partly, perhaps, by little springy particles; by somewhat that may be call'd the vital portion of the air; and, by things analogous to local ferments: the important operations of all which, cease with life; and the agents themselves are not to be discern'd in a dead carcass. So that, besides those manifest uses, which the visible fabric of the engine may suggest to an anatomist; there may be chymical uses of some parts, that serve for the elaboration of spirits, and other fluids: which uses, as we formerly observ'd, are not suggested to the anatomist, by the inspection of the structure of the parts; but, to discern them, may require no ordinary skill in chymical principles, and operations.

Such considerations as these, lead me to think it exceeding difficult to determine, with any certainty, the principal use of many particular parts; especially, if other uses be excluded. It is not enough to secure us, that we understand the chief function, and end, of a part, to know, that it is contrived for such a purpose; since this fitness hinders not, but that the primary use of the part may be another more conducive to the general welfare of the animal, or the cosmical ends of nature. And it ought not to seem strange, that some pieces of workmanship, consisting of many parts, all of them curiously contrived, should, by one learned man, be guess'd to be design'd for this use; and, by others, for that; while both of them may be worthy of the artificer.

When some very politic prince does a great thing, without declaring why; the guesses of the statesmen are often very different; tho' none of them ascribe to him a design unbecoming a wise man. So, when a learned author expresses himself ambiguously; one reader interprets his words to this sense; and another, to that: yet both of the senses pitch'd on,

PHYSICS. may fairly agree with the context, and the main scope of the writer. This I say, because I would, by no means, disparage the wisdom of nature, by proposing the difficulties I have hitherto mentioned; tho', I confess, that, upon the account of them, and some others; I look upon many of the arguments that several authors draw from final causes, but as conjectures. And, in many cases, I allow what is suggested to me, upon the supposition of the intended uses of particular parts, rather as proper hints to excite, and direct, a more thorough inquiry, than as safe grounds to build physical conclusions on.

P R O P. V.

The naturalist should not suffer the search, or discovery, of final causes, to make him undervalue, or neglect, the inquiry after their efficient.

'Tis true, to inquire for what purpose nature would have particular effects produced, is a curiosity worthy of a rational creature. But this is not the proper task of a naturalist; whose work is not so much to discover the manner wherein, as for what reason particular effects are produced. Thus, an ignorant rustic, in *England*, knows something of a watch, if he is able to tell you, that 'tis an instrument made to measure time; which is more than every *American* savage would be able to say; and more than those civiliz'd *Chinese* knew, who took the first watch they saw for a living creature: yet the *English* rustic, who knows no more of a watch, than that 'twas made to shew the hour of the day, understands but very little of the nature of that machine. And, as the two things men aim at in physics, are to understand after what manner nature produces the phenomenon we contemplate; and, in case it be imitable by us, how we may, if occasion requires, produce the like effect, or come near it; these ends cannot be attain'd by the bare knowledge of the final causes of things, nor of the general efficient. But, to answer those intentions, we must know the particular efficient, with the manner, and progress, of their operating; and what dispositions they either find, or produce in the matter they work on. Thus, he who would thoroughly understand the nature of a watch, must not rest satisfy'd with knowing, in general, that a man made it for such uses; but he must, particularly, know of what materials the spring, the wheels, the chain, and the balance are made; he must know the number of the wheels, their magnitude, shape, situation, and connexion, in the engine; and after what manner one part moves another, in the whole series of motions; from the expansive endeavour of the spring, to the revolutions of the index that points the hours. And much more must a mechanic know all this, if he means to make a watch himself, or to give sufficient instructions for it to be made. In short, the neglect of efficient causes, would render philosophy useless; but the studious search after them, will not prejudice the contemplation of final causes. For the wise author of nature has so excellently contriv'd the universe, that the more clearly, and particularly, we discern how suitable the means are to the ends

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ends to be obtain'd by them; the more plainly we discern the admirable wisdom of the author of things; who is "wonderful in counsel, and excellent in working." Nor will the sufficiency of the intermediate causes make it needless to admit a first, and supreme cause; since that order of things, by virtue whereof these means become sufficient to such ends, must have been, at first, instituted by an intelligent cause. And if it be irrational to ascribe the excellent fabric of the universe, and the actions that have manifest tendencies to determinate useful ends, to so blind a cause as chance; it will be rather more irrational to ascribe to chance, the first formation of the universe; of which the present state of things is but the natural consequence, or effect. For it may, indeed, be plausibly said, that, in the present state of things, the several parts of the universe are, by the contrivance of the whole, determin'd, and thereby qualify'd to obtain their ends; but it cannot be, rationally, pretended, that, at the first formation of the world, there was a sufficiency in the senseless materials of it, without any particular guidance of a wise superintendent, to frame bodies which are so excellently contriv'd, and fitted to their respective ends.

Upon the whole, it appears, that all consideration of final causes, is not to be banish'd from natural philosophy; but that 'tis rather allowable*, and, in some cases, commendable, to argue, from the manifest uses of things, that the author of nature pre-ordain'd them; that the sun, and moon, with the other celestial bodies, excellently declare the power, wisdom, and glory of God; and were, some of them, among other purposes, made to be serviceable to man; that, from the suppos'd ends of inanimate bodies, whether celestial, or sublunary, 'tis very unsafe to draw arguments, to prove the particular nature of those bodies, or the true system of the universe; that, as to animals, and the more perfect sort of vegetables, 'tis warrantable to say, particular parts were pre-ordained to particular uses, relating to the welfare of the animal, or

* Final causes are allowable in philosophy; and Sir Isaac Newton himself, scruples not to assign them in the structure and contrivance of the universe. And of this we have a remarkable example in his doctrine of comets. "Comets, (says that admirable philosopher) by reason of the great number of them, the vast distance of their aphelia from the sun, and the considerable stay they make there, must necessarily receive some disturbance from their mutual gravitations to each other. Whence their eccentricities, and times of revolution, will sometimes be a little increas'd, and

" at others, diminish'd. We are not, therefore, to expect, that the same comet shall always exactly return in the same orbit, at the same period of time.—— And hence appears the reason why comets move not in the zodiac, as the planets do; but stray from it, in various motions, to all the parts of the heavens: for, in their aphelia, where their motion is slowest, they ought to be at the greatest distance from one another, that their mutual attractions may be the less. *Newton. Princip. p. 480.*

PHYSICS. plant itself, or to its species; but that such arguments may easily deceive, if those who form them are not very cautious to avoid mistaking, among the various ends that nature may have in the contrivance of an animal body, and the various ways which she may successfully take to compass the same ends; and lastly, it appears, that a philosopher, nevertheless, must not let the search, or knowledge of final causes, make him neglect a careful inquiry after efficient.



THINGS

T H I N G S

A B O V E

R E A S O N,

C O N S I D E R ' D.

S E C T. I.

IT appears to me, that, among the objects of our reason, there are *Things above reason, of three kinds.* some, whose nature we cannot comprehend; others, whose attributes, or actions, we cannot reconcile to the subject; and, lastly, others, that we cannot conceive how they should consist with some acknowledged truth: and these may be call'd things above reason. By which I here understand, not such things as are false, and absurd; but such as, tho' the understanding sees sufficient cause to assent to, yet finds itself reduced to this assent, with a remarkable, and peculiar, disadvantage. And this disadvantage, usually, proceeds either from the nature of the thing proposed; which is such, that we cannot, sufficiently, comprehend it; or, from our being unable to conceive the manner of its existence, and operation; or, because it involves some notion, or proposition, that we see not how to reconcile with some other, which, we are persuaded, is true. The first of these three sorts of things, may be call'd incomprehensible; the second, inexplicable; and the third, unsociable.

The first consists of those things, whose nature is not distinctly, and adequately, comprehensible by us; to which, perhaps, we may refer all such intellectual Beings, if any be granted, as are, by nature, of an higher order than human souls; at least, we may refer to this head the great author of nature. *Incomprehensible.*

The second sort of things above reason, consists of such as, tho' we do not deny to exist, yet we cannot clearly, and satisfactorily, conceive how they can be what we acknowledge they are: as, how matter can be infinitely divisible; and how there should be such an incommensurability betwixt the side, and diagonal, of a square, that the smallest line cannot, adequately, measure them both. *Inexplicable.* The

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Unsociable.

The third sort, are things incumber'd with difficulties, and objections, that cannot directly, and satisfactorily, be removed by those who acquiesce in the receiv'd rules of inferior sciences, and reason but at the common rate; such objects of contemplation having something belonging to them, that appears irreconcilable with some very manifest, or acknowledged truths.

Thus, that man has free will, at least with regard to civil matters, is the general confession of the species: all the laws that forbid, and punish, murder, adultery, theft, and other crimes, being founded upon a supposition, that men have a power to forbear committing them: and the sense men have of being possessed of this power over their own actions, is great enough to make malefactors acknowledge their punishments just; being condemned by their own consciences, as well as by their judges. Yet the generality of mankind ascribe to God an infallible prescience of human actions. But how a certain fore-knowledge can be had of contingent things, and such as depend upon the free-will of man, is that which we are unable clearly to comprehend. And, doubtless, 'tis hard to conceive, how an infinitely perfect Being should want prescience; or, that our will should want the liberty we almost perpetually exercise.

Again, geometricians teach the divisibility of quantity *ad infinitum*, to be mathematically demonstrable. Suppose, then, a strait line, of three feet long, divided into two parts, the one double to the other; now a line of two feet is divisible into infinite parts, or it is not; if you say it is not, you contradict a demonstration; if you say it is, you must confess, either that the line of one foot is divisible into as many parts as the line of two feet; or else, that the infinite parts into which the line of one foot is granted to be divisible, is exceeded in number by the parts into which the line of two feet is divisible; and, consequently, that the line of two feet has a multitude of parts, greater than infinite. Which reasoning may let us see, that we may be reduced, either to reject inferences fairly drawn from manifested, or granted truths, or to admit conclusions that appear absurd; if we will have all the common rules, whereby we judge of other things, to be applicable to infinities. Hence it is clear, in the general, that there may be things which surpass our reason; at least, so far, as not to be judg'd of by the measures and rules used in judging of ordinary occurrences: for which reason, I shall call them privileg'd things.

Privileg'd things what?

The imperfection of the human mind.

But, to manifest the imperfections of our reason, with regard to privileg'd things, we need not have recourse to the divine nature: a mathematical demonstration may serve the turn. For, suppose a great circle, divided into its 360 degrees; and suppose any number of strait lines, drawn from the several assignable parts of some one of these degrees, to the centre; 'tis manifest, that the degrees being equal, as many lines may be drawn from any other; and so from every one of the rest.

Then, suppose a circular arch, equal to the assumed degree, to be further bent into the circumference of a little circle, concentrical with the great

great one; it follows, from the nature of a circle, that the semi-diameters of it, how many soever they be, can no where touch one another, but in the centre. Whence 'tis evident, that all the lines, drawn from the circumference to the centre of the greater circle, must pass thro' different points of the circumference of the smaller; and, consequently, that as many lines as can, even mentally, be drawn from the several points of the circumference of the great circle to the common centre of both circles, must all pass thro' different points of the little circle; and thereby divide it into as many parts as the greater circle is divided into. Here, then, the circumference of the lesser circle presents us with a curve line, which was not possibly divisible into more parts than an arch of one degree, or the 360th part of the circumference of the greater circle; yet, without being lengthen'd, it becomes divisible into as many parts as the whole circumference of the same greater circle. And, tho' we should suppose the circumference of the internal circle not to exceed one inch; and that of the exterior circle to exceed the circumference of the terrestrial globe, or *Orbis magnus* itself; still the demonstration would hold.

And, surely, 'tis very agreeable, both to the nature of God, and to that of man, that such things as these should prove true; for we mistake, and flatter human nature too much, when we think our understanding so unlimited, both in point of capacity, and of extent; and so free, and unprepossess'd, as many philosophers seem to suppose. For, whatever our self-love may incline us to imagine, we are really but created, and finite Beings, and come into the world but such as it pleas'd the most free author of nature to make us. And, from this dependance, and limitation of our natures, it follows, not only that we may be born with certain impressions, appetites, or tendencies of mind; but also, that the means given us to employ in the search, or judging of truth, are only proportionable to the design of our make; and, therefore, may, probably, be supposed not able to reach all kinds of truths; many of which may be unnecessary for us to know here: and some may be reserv'd, to render us sensible of the imperfections of our natures, and to make us aspire to that condition, wherein our faculties shall be much enlarged, and heighten'd. It seems not, therefore, unreasonable to think, both that God has so limited our faculties, that, in our present state, there should be some objects beyond the comprehension of our understandings, or, that some of his creatures should not be able perfectly to understand some others; and yet, that he has given us light enough to perceive, that we cannot attain to a clear, and full knowledge of them.

It may here, indeed, be objected, that tho' the instances given, have not been hitherto clear'd by the light of reason, yet, 'tis probable, they may be so hereafter; considering how great a progress is, from time to time, made in the discoveries of nature. I answer, that I imagine, what future discoveries are made, will chiefly concern those things which either we are ignorant of, for want of a competent history of nature; or mistake, thro' erroneous prepossessions, or for want of freedom, and attention, in our specu-

PHYSICS. *Speculations.* But as to all metaphysical difficulties, wherein neither matters of fact, nor the hypotheses of subordinate parts of learning are of force, we have little to expect. And, however it be as to other abstruse objects; I am very apt to think, there are some things relating to the infinite Being, which will still remain incomprehensible, even to philosophical understandings. I can scarce hope to see those obstacles surmounted, that proceed not from any personal infirmity, or evitable faults, but from the limited nature of the human mind. Besides, as mankind may hereafter explain some of those grand difficulties, which have hitherto perplex'd philosophers; so their inquiries may, possibly, lead them to discover new difficulties more perplexing than the first. For, even amongst the things wherewith we are already acquainted, there are many which we think we know, only because we never, with due attention, try'd whether we can frame such ideas of them, as are clear, and worthy for a rational mind to acquiesce in. This appears from the great intricacy that considering men find in the notions commonly receiv'd of space, time, and motion, &c. and the difficulties of framing clear, and satisfactory apprehensions, even of such obvious things. We see also, that the angle of contact, the doctrine of asymptotes, and that of surd numbers, and incommensurable lines, all which give no concern to common accountants and surveyors, perplex the greatest mathematicians. Hence the growing curiosity of mankind is not more likely to solve some difficulties, than to raise others; which may prove more insuperable than they.

The motion of a coach-wheel is so obvious, and seems so plain a thing, that the coach-man never looks upon it with wonder; yet after *Aristotle* had taken notice of the difficulty that occur'd about it, this trivial phenomenon has perplex'd even famous mathematicians, and continues yet to do so: there being some circumstances in the progressive motion and rotation of the circumference of a wheel, and its nave; or of two points assign'd, the one in the former, and the other in the latter, that have appear'd too subtle for modern writers.

After what manner human reason acts.

And here we may observe, that reason operates according to certain ideas, axioms, and propositions, wherewith, as by rules and measures, it conceives, estimates, and judges of things. And, indeed, when we say, that a thing is consonant or repugnant to reason, we mean usually, that it is either immediately or mediately deducible from, or at least consistent with, or contradictory to one or other of these standard-rules or notions. But if these rules and notions be such as are drawn only from finite things, or are agreeable but to those; they may prove useless or deceitful, when we go to stretch them beyond their measure, and apply them to infinities.

To illustrate and confirm this notion. All the things that we naturally do or can know, may be divided into such as we may acquire without a medium, and such as we cannot attain to, but by the intervention of a medium, or by a discursive act. To the first belong such notions as are supposed to be connate; as, that "Two contradictories cannot be both true;" "The whole is greater than a part thereof;" "Every whole number is either

either even or odd ;" &c. as also, those other truths, that are assented to upon their own account, without needing any medium to prove them ; because that, as soon as by clear terms, or fit examples, they are plainly proposed to the understanding, they discover themselves to be true so manifestly by their own light, that they want no proposition to make the understanding acquiesce in them. Of this kind are some of *Euclid's* axioms ; for instance, " If to equal things equals be added, the totals will be equal."

To the second sort of things knowable by us, belong all that we acquire by the act of reasoning ; wherein, by means of propositions or mediums, we deduce one thing from another ; or conclude, affirmatively or negatively, one thing of another. This being supposed, and we being conscious to ourselves that we are not the authors of our own nature ; all the experience we have hitherto had, leads us to think, that the measures suggested to us, either by sensations, the results of sensible observation, or the other instruments of knowledge, are such as fully reach but to finite things ; and, therefore, are not safely applicable to others. And many of those principles that we think very general, may be only gradual notions of truth ; and but limited and respective, not absolute and universal.

And tho' perfect syllogisms be counted the best and most regular forms, that our reasonings can assume ; yet even the laws of these are grounded on the doctrine of proportions ; for even between things equal, there may be a proportion. Upon this ground, I suppose, it is, that mathematical demonstrations have been publicly proposed of the grand syllogistical rules. And, in consequence hereof, geometers tell us, there is no proportion between a finite line and an infinite ; because the former can never be so often taken, as to exceed the latter ; which, according to *Euclid's* definition of proportion, it should be able to do. Since then, the understanding operates but by the notions and truths 'tis furnish'd with, and since these are its instruments, by proportion to which, it takes measures, and makes judgments of other things ; such instruments may be too disproportionate to some objects, to be securely employ'd to determine several particulars about them. Thus, the eye being an instrument which the understanding employs to estimate distances, we cannot by that, safely take the breadth of the ocean ; because our sight reaches not far enough, to discover the extent of so vast an object. And the common instruments of surveyors, that would serve to measure the height of a house or a steeple, or even of a mountain, cannot enable them to take the distance of the moon. But when astronomers take, by supposition, a line that reaches from the surface to the centre of the earth, tho' by the help of this, and the parallax, they may tolerably measure the distance of the moon ; yet, with all their great industry, they cannot, by the same way, with any tolerable accuracy, measure the distance of the stars ; the semi-diameter of the earth bearing no sensible proportion to that of so vast a sphere, as makes their parallax vanish : it being all one to sense, whether, at so great a remove, a star be observ'd from the centre, or from the surface of the earth. Thus when I

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'Tis evident, that some substance or other, whether God, the world, or matter, never had a beginning; that is, has been for ever. But when we speak of an eternity *à parte ante*, as they call it, we do not speak of a thing whereof we have no conception at all; as will appear to a considering person: yet the general notion we have, is such, that when we come attentively to examine it, by the same ways whereby we judge of almost all other things, the understanding is confounded: for we must conceive, that the time efflux'd since the first man began to live, bears no greater proportion to the duration of God, or of matter, than a moment. And as there are some things, whose nature and consequences puzzle our faculties; there are others, whereof tho' we have a notion, yet the *Modus operandi* is beyond our comprehension. I do not mean only the true and certain *Modus operandi*, but even an intelligible one. Thus, tho' many, especially *Cartesians*, and that upon a philosophical account, assert that God created the world; yet, how a substance could be made out of nothing, I fear, we cannot conceive.

And

And tho' all philosophers, very few excepted, believe that God made the world, out of pre-existent matter; yet how he could make it, but by locally moving the parts of the matter it was to consist of; and how an incorporeal substance can move a body, which it may pass thro' without resistance, will be found hard to explain. If it be said, that the soul being an immaterial substance, can nevertheless move the limbs of the human body, rightly disposed; I answer, it does not appear that the rational soul gives any motion to the parts of the body, but only guides or regulates that which she finds in them already.

Thus then, by making observations of such things as are the proper objects of our faculties, and by drawing just consequences from such observations, and from our other knowledge, we may come to be certain that some things exist, and so have general and dark ideas of them; when, at the same time, we are at a loss to conceive how they can be such, or how they can operate and perform what they do; supposing the truth and sufficiency of some other things we are convinc'd of. To be short, negative apprehensions we may have of some privileged things, and positive, but indistinct apprehensions of others; and that is enough, in some sort, to make us understand ourselves, and one another, when we speak of them: tho' when we sufficiently consider what we say, we may find, that our words are not accompany'd with clear, distinct, and symmetrical conceptions of those abstruse and perplexing things we speak of. And since we find, by experience, that we are unable sufficiently to comprehend things, which by clear and just consequences may be proved to exist; this strongly argues, that some of our conceptions may be of things whereto somewhat belongs, that transcends our reason, and surpasses our comprehension. So that when natural philosophy had taught men to believe God to be an infinitely perfect Being, we ought not to say, that they had no idea of such a Being, because they had not a clear and adequate one. And since *Aristotle* professedly discoursed of infinity, and cites the ancients philosophers for having done so before him; and since *Democritus*, *Epicurus*, *Gassendus*, and others, maintain either that the world is boundless, or that space is not finite in extent, or that the world consists of atoms, infinite in number; we must not suppose, they said they knew not what, as they must have done if they wrote without ideas of the things they treated of: tho' we may justly say, that the subject being infinite, the ideas they framed of it, could not be comprehensive and accurate. Thus, when the eye looks into a deep sea, tho' it may pierce a little way into it; yet when it would go deeper, it only discovers somewhat dark and indistinct, which affects the organ so differently from what other more genuine objects do, that we thence easily discern our sight fails us in the way, before it arrives at the bottom; and consequently, that there may be many things conceal'd there, which our sight is unable to reach. But if we really find there are things which our reason cannot comprehend, then, whether the account I have given, why our faculties prove insufficient for these things, be good or not; there must be some true account or other of that insufficiency.

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ciency. And some things must appear to us so sublime and abstruse, that we are unable to comprehend them, and to discern so much as the reason why they cannot be comprehended by us.

Upon the whole, then, we may reasonably suppose, that the great author of nature so framed man, as to have furnish'd his intellective faculty with a light, whereby it can, not only estimate the power of a multitude of other things, but also judge of its own nature and power, and discern some of the limits, beyond which it cannot safely exercise its act of judging and defining. But the rational soul does not only pass judgment of the things without her, but about herself, and what passes within her: she searches out, and contemplates her own spirituality, and union with the body. The intellect judges wherein its own nature consists, and whether or no itself be a distinct faculty from the will. And to come yet closer, logic and metaphysics are the works of the human mind; which, by framing those disciplines, manifests that it not only judges of reasoning, but of the very principles and laws thereof; teaching what things are necessary to obtain evidence and certainty, and what kind of mediums they are, from whence we must not expect any demonstrative arguments relating to a subject. Thus, if we compare the bodily eye with the understanding, which is the eye of the mind; we must allow this difference, that the intellect is as well a looking-glass as a sensory; since it does not only see other things, but itself too, and can discern its own blemishes, bad conformation, or whatever other infirmities it labours under. The soul, therefore, when duly excited, is furnish'd with a light that may enable her to judge, even of many of those original notions, by which she judges of other things. In short, the soul, upon trial, may find, by an inward sense, that some things surpass her force; as, a blind man being set to lift up a rock, would quickly find it too unwieldy to be managed by him: and the utmost exercise of his strength, would but convince him of the insufficiency of it to surmount so great a weight. We do not then pretend, that the eye of the mind should see invisibles; but only, that it shall discern the limits of that sphere of activity, within which nature hath bounded it; and consequently, that some objects are disproportionate to it. *Aristotle* himself says, that the eye sees both light and darkness; which expression, tho' somewhat odd, may be defended: since, tho' darkness is a privation, not a Being, it cannot properly be the object of sight; yet it may be perceived by means of the eye, from the very different affection which that organ suffers, when impress'd by luminous objects, and when it is made useless to us by darkness.

Whether men
may, with ju-
stice, discourse of
things above
reason?

But it may be ask'd, how can we justify our discoursing, at all, of things transcending reason?

The better to clear this matter, I must make some distinctions of the notions or conceptions of the mind; and, for brevity sake, give names to those I have now occasion to employ. I consider then, that whether the conceptions or ideas we have of things, be simple or compounded, they may be distinguish'd into such as are particular or distinct, and such as are only

only general, dark, and confused, or indistinct. When a navigator to unknown countries, first gets a sight of land, tho' he may be satisfied that it is land, yet he has but a very dark and confused picture of it made in his eye, and cannot descry whether the shore be rocky, or what creeks or harbours it may have in it; much less, whether the coast be well inhabited; and if it be, what kind of buildings it has: all which he may plainly and distinctly see, upon going on shore. And of some things we have an adequate, of others but an inadequate conception: thus, if we suppose the same navigator should look towards the main sea, tho' he might see a great way distinctly, yet at length it would appear so dark and confusedly to him, that at the verge of the sensible horizon, his sight would make him judge the sea and sky came together; he would, however, conclude, that the utmost part of the sea he could descry, was but a part of the ocean, which may, for ought he knows, reach to a vast extent beyond the visible horizon. To our confused, and often, also, to our inadequate conceptions, belong many of those that may be call'd negative; which we employ, when we speak of privations or negations, as blindness, ignorance, death, &c. We have a positive idea of things that are square and round, black and white; and, in short, of other things, whose shapes and colours make them the objects of our sight. But when we say, for instance, that a spirit or an atom are invisible; those words are attended with a negative conception, which is commonly but dark and confused, because 'tis indefinite, and removes, or lays aside those marks, by which we clearly perceive and distinguish visible substances: as, when we say, that such a thing is impossible, we have some kind of conception of what we speak; but 'tis a very obscure and indistinct one, at best; exhibiting only a general and very confused representation of some ways, whereby one might think the thing likely to be effected, if it were at all performable; and accompanied with a perception of the insufficiency of those ways. There is yet another difference in the notions we have of things, which, tho' not usually observ'd, is too important to be here omitted. Of some things we have a knowledge, that for want of a fitter term, may be call'd primary or direct; and of others, the knowledge we have, is acquired but by inferring it from some more known or clearer truth; and so may be call'd *inferred* or *illative* knowledge. Thus, when a geometrician defines to me an hyperbola, I quickly gain a clear and distinct idea of it: but when he proves to me, that this hyperbola may have such a relation to a strait line, which he calls an asymptote, that this line being continued, still comes nearer and nearer to the prolong'd side of the hyperbola, and yet, how far soever both be drawn, will never come to touch it; his subtle demonstrations present me with an *inferred* or *illative* truth, at which we arriv'd not, but by the help of a train of reasonings; and on which if we exercise our imagination, we shall find this factitious truth, if we may so call it, accompanied but with a very dim and confused idea. To these distinctions we may add, lastly, what belongs chiefly to the notions we have of true or false propositions, that of our conceptions of things, some are symmetrical, or every way consistent; by which I mean, those which have

have these two qualifications; the one, that all the parts are consistent among themselves; the other, that the entire idea is consistent with all other truths: and some are chimerical, or asymmetrical; by which I understand those that are either self-destructive, by the contrariety of the parts themselves, whereof they are made up, as if one should talk of a triangular square, or a sun-shiny night; or being extravagant, lead to some manifest absurdity that may be justly inferr'd from them; or into inextricable difficulties; or else involve a real repugnancy to some acknowledg'd truth, or rule of reason.

Now the mind of man is so framed, that, when duly instructed, it can perceive a want of light in itself for some purposes; or of clearness, and completeness, in the best ideas it is able to frame of some things; and, on this account, can so far take notice of the extent, and imperfection of its own faculties, as to discern, that some objects are disproportionate to it. As, when we attentively consider the dimensions of space, or those of the universe; we may, by trial, find, that we cannot conceive them so great, but that they may be yet greater; or may exceed the bounds, how remote soever, which our former conception presumed to assign them. Thus, when the eye looks upon the main sea, we easily grow sensible, that, how much soever we can discover of it, yet our sight falls far short of the extent of that vast object. And 'tis by the sense which the mind has of its own limits, and imperfections, on certain occasions, that we may estimate what things ought not, and what ought to be look'd upon as things above reason; for, by that term, I mean not such things as our rational faculty cannot at all reach to, or has no kind of perception of: for, of such things, we cannot, in particular, either think, or speak, like men: but my meaning is this, that the rational soul, being conscious of her own actions, and feeling that she knows several sorts of things truly, and clearly; and, thereby, justly concluding them to be within the compass of her faculties; when she contemplates some few things, which seem to be of another order, she is convinced, that, however she strains her power, she has no such ideas, or perceptions, of them, as she has, or may have, of those objects, which are not disproportionate to her faculties.

Again, the nature of the mind is such, that its faculty of drawing consequences from known truths, is of greater extent, than its power of framing clear, and distinct, ideas of things; so that, by suitable, or successive inferences, it may attain to a clear conviction, that some things exist, of whose nature, and properties, it can frame no clear, and satisfactory conceptions. And, that men should be better able to infer propositions about several things, than to penetrate into their nature, is the less strange, because 'tis oftentimes sufficient for our uses, to know that such things exist; tho' that knowledge be not accompany'd with a clear and distinct idea; and because the rules, as this, for example, "whatever is produced, must have a cause," are often clear, and easy, that enable the mind to infer conclusions about things, whose nature is very abstruse.

To apply, then, these notions to the three sorts of things that I have represented, as being, in some sense, above reason. There are objects of so immense, and peculiar a nature, that, by an easy view of the mind, without subtle, and laborious inquiry, the soul discerns, and, as it were, feels the object to be disproportionate to her powers; and, accordingly, if she tries, she quickly finds herself unable to frame conceptions thereof fit to be acquiesced in: and this sort of objects, I, upon that account, call inconceivable, or supra-intellectual.

But when, by attentively considering the attributes, and operations of a thing, we, sometimes, find, that it hath a property belonging to it; or performs somewhat, which, by reflecting on the Beings, and ways of working that we know already, we cannot discern to be reducible to them, or derivable from them; we then conclude this property, or this operation, to be inexplicable; that is, such as cannot so much as, in a general way, be intelligibly accounted for: and this makes the second sort of our things above reason. But this is not all; for the rational soul, that is already furnish'd with primitive ideas, and rules, of true and false; when she comes to examine certain things, and make successive inferences about them, she finds, that she cannot avoid admitting some consequences as true, and good, which she is not able to reconcile to some other manifest truth, or acknowledg'd proposition. And other truths are so harmonious, that there is no disagreement between any two of them. The heterogeneous truths, I speak of, appear not symmetrical with the rest of the body of truths; and we see not how we can, at once, embrace these, and the rest, without admitting the grand absurdity, which subverts the very foundation of our reasonings, that contradictions may be true. Thus, in the controversy about the endless divisibility of a straight line; as 'tis manifest, that a line of three feet, for instance, is thrice as long as a line of one foot, so that the shorter line is but a third part of the longer; it should follow, that a part of a line would contain as many parts as the whole, since each of them is divisible into infinite parts; which seems repugnant to common sense; and, to contradict one of those axioms in *Euclid*, whereon geometry itself is built. Upon which account, I have ventur'd to call this third sort of things above reason, asymmetrical, or unfociable; of which eminent instances are afforded us, by those controversies, wherein, which side soever of the question you take, you will be unable, directly and truly, to answer the objections that may be urg'd to shew, that you contradict some primitive, or other acknowledg'd truth.

These are some of the considerations, by which I have been induced to distinguish the things that, to me, seem to over-match our reason, into three kinds. Of those I stile inconceivable, our ideas are such, that a moderate attention suffices to make the mind sensible she wants either light, or extent enough, to have a clear and full comprehension of them; those things I have called inexplicable, are such as we cannot perceive to depend upon the ideas wherewith we are furnish'd;
and

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and to resemble, in their manner of working, any of the agents, whose nature we are acquainted with; and, lastly, those things which I have named unfociable, are such as have notions belonging to them; or have conclusions deducible from them, that appear either disagreeable to our primitive ideas, or, when driven home, inconsistent with the manifest rules we are furnish'd with, to judge of true and false.

But, by sorting things above reason, into three kinds, I do not deny that 'tis possible one object may, in different regards, be referr'd to more than one of these sorts; as may sufficiently appear in that noblest of objects, God; who, when he made the world out of nothing; or, when he discerns the most secret thoughts and intentions of the mind; or, when he unites an immaterial spirit to a human body, and maintains, perhaps, for very many years, that matchless union, with all the wonderful conditions he has annex'd to it; he supplies us with instances of things that are inexplicable: for such operations are not reducible to any of the ways of working known to us, since our minds can but modify themselves by various manners of thinking. And, as for things without us, all that one body can do to another, by acting on it, is to communicate motion to it; and thereby produce in it the natural consequences of such motion; in all which, there is no action like any of those I just now ascribed to God. And since the prescience of those future events, which we call contingent, being a perfection, is not to be deny'd to God; and since this seems irreconcilable to the freedom of human actions, it will afford us an instance of truths, whose consistency, and whose symmetry with the body of other truths, our reason cannot discern; and which, therefore, ought to be referr'd to that sort of things above reason, which I call unfociable.

I come now to the third sort of these things, that mention'd under the name of incomprehensible, or supra-intellectual; which title, whether it belongs to any other object, or no, certainly belongs to God, whose nature comprehending all perfections, in their utmost possible degrees, is not likely to be comprehensible by our minds, who wholly want several of those perfections, and have but moderate shares of the rest. We are, indeed, born with, or, at least, have a power, and several occasions to frame an idea of a Being infinitely perfect; and, by this idea, we may sufficiently discriminate the origin of it, God, from all other objects whatsoever: but, when we come to consider attentively, and minutely, what is contain'd in the notion of omnipotence, omniscience, eternity, and those other attributes that are all united in that great affluence of perfections, God; we may be sure to find, that our faculties are exceedingly surmounted by the vastness and glory of that unlimited, and unparallel'd object; about which, as we can discover that it exists, and that it possesses all the perfection we can conceive; so we may, at the same time, discern, that it must have degrees of perfection, which, because of the inferiority of our nature, we are not able to comprehend. Yet this discovery of God's incomprehensibility, may be made, without subtle inquiries, and without trains of consequences, tho' not without attention, by a direct view of the mind; which finds

finds itself, upon trial, as unable, fully to measure the divine perfections, as the dimensions of space, which we can conceive to be greater and greater, without ever being able to determine any extent beyond whose limits they cannot reach.

'Tis indeed, therefore, arrogance to talk of infinite, or of privileg'd things, with the same confidence, or to pretend to do it with the same clearness, wherewith knowing men may speak of things, unquestionably within the compass of our understandings: but this need not hinder us, nor doth disable us from speaking rationally of privileg'd things themselves. For all allowable notions, are not of the same sort, or order; and if none were to be admitted but those that enable us to comprehend the object, that is, which give us a clear and distinct knowledge of all that it contains, or that belongs to it; I must confess, we have no good notions of privileg'd things in particular. I must add, that I fear we have few or none, even of many things, that we think ourselves very knowing in. And when we speak of things, as being above reason, tho' we have no clear, distinct, and adequate idea of them; yet we may have a general, confused, and inadequate notion of them; which may suffice to make us distinguish their respective objects from all else, and from one another; as may be observ'd in several ideas that are negatively framed; such as those we have of invisible, incomprehensible; and in others, which I formerly call'd *inferred*, because they accompany the remote inferences whereby one truth is concluded from another; as when geometricians infer from some propositions in *Euclid*, that any strait line may be divided farther and farther, without stop. For, of this, and some other propositions, about privileg'd things, we are not quite destitute of allowable notions, as may appear by some admirable speculations of mathematicians, about the affections of surd numbers, and incommensurable magnitudes, of some of which we have no such clear and symmetrical conceptions, as we have of many other things that are of a nearer, and more intelligible order. I shall not, therefore, scruple to acknowledge, that by my own experience, the confessions of others, and by their unsuccessful attempts, I am induced to think, that God, who is a most free agent, having been pleased to make intelligent Beings, may, perhaps, have made them of different ranks, or orders, whereof men may not be the principal; and that, whether there be such orders, or no, he hath, at least, made us men of a limited nature, in general, and of a bounded capacity; and accordingly, hath furnish'd man either with certain innate ideas, or models, and principles; or, with a faculty, or power, and disposition, easily to frame them, as it meets with occasions to excite them. But, because God intended the mind of man of a limited capacity, his understanding is so constituted, that the in-bred, or easily acquired ideas, and primitive axioms, wherewith it is furnish'd; and by relation, or analogy whereto, it judges of all other notions and propositions, do not extend to all knowable objects whatsoever; but reach only to such as have a sufficient affinity, or bear some proportion to those primary ideas, and rules of truth, which are sufficient, if duly improv'd,

PHYSICS. to help us to attain, tho' not the perfect knowledge of truths of the highest order, yet the competent knowledge of as much truth, as God thought fit to allow our minds, in their present state of union with our bodies.

And, indeed, I see no reason to repine at the limits which the author of nature assigns to human knowledge. For the number of privileg'd things, is altogether inconsiderable, in comparison of the multitude of others, to which our knowledge may be improv'd to reach; and which it far more concerns us well to know, than it doth to resolve puzzling questions, about things incomprehensible; there being, within the compass of those truths, enough to employ, and reward our diligence, without straining and tiring our reason about objects that transcend it. Yet, even about these, some inquiries may be allowed; for an object that is, on account of some of its properties, privileg'd, may have several others belonging to it, that do not surpass our reason; and whose knowledge may, therefore, be attain'd by the due employment of it. Thus we usefully study the nature of bodies, which make up the object of natural philosophy; tho' the true notion of body, in general, be a thing so difficult to frame, that the best of our modern philosophers can, by no means, agree about it. This I do not wonder at, because, if we pursue the notion of body to the utmost, 'twill lead us to the perplexing controversy *de compositione continui*; and there the understanding will be left in the dark. Thus surveyors, carpenters, architects, and many others, know several properties of the square figure, that are of great use to them in their respective employments; tho' this, that its side, and diagonal, are incommensurable, be unknown to most of them; and if they were told of it, and should prosecute the speculation, 'twould involve them in exceeding great, and, probably, insuperable difficulties.

And, even about privileg'd things, our inquiries, if discreetly manag'd, may not only be allowable, but, sometimes, profitable. For, of such subjects, a studious search may bring us to know more than we did, tho' not so much as we would, nor enough to be acquiesced in. This may, probably, teach us to know both the objects better, and ourselves the better too; by giving us such a sensible discovery of the insufficiency of our understandings, to comprehend all sorts of things, as may be very useful, tho' not pleasing; and may richly reward the pains that ended in so instructive a disappointment. Thus in the noblest instance that can be given, the contemplation of God himself, tho' he has so order'd all things, that 'tis scarce possible for us to be destitute of an idea of him; yet when we come, with a sufficient application of mind, to pry into the wonderful attributes of this most singular, and adorable Being, we are sure to find ourselves unable to comprehend so unbounded an object. This, however, ought not to discourage us from so noble a study, since we are allowed the great contentment and honour to make further and further discoveries of the most excellent of objects, by that very immensity of his perfections that renders it impossible for us to reach to the bounds of his excellency, or rather to discover that it has any bounds at all.

S E C T. II.

I Proceed, in the next place, to offer some rules and directions, whereby to regulate, and estimate the reasonings we meet with, concerning things above reason. Rules for judging of things above reason.

And, first, I would observe, that as to privileg'd subjects, we should not admit any affirmative assertion, without such proofs as are sufficient in their kind. The first rule.

For, 'tis not reasonable to give assent to any thing, as a truth, without a sufficient ground for that assent. And tho' it may well be granted in the general, that a thing, which surpasses our reason, may have belonging to it some property that is also above reason; yet we are not, in particular, to believe, that this, or that affection belongs to it, without particular and competent proof. For, since about a privileg'd thing, as well as about any other, propositions may be framed, and often are so, contrary to one another; to assent to both, were certainly to believe one falsity, if not two. And if we will assent but to one, we must either judge at adventures, or allow ourselves to examine the mediums of probation, employ'd on both sides; and thereupon judge why one of the propositions is to be assented to, and the other rejected. This manly freedom must be allow'd; without which, our understandings were liable to be impos'd on, in matters of the highest concern: for, there scarce ever did, or, I fear, ever will, want some men, who, either out of ignorance, and passive delusion, self-confidence, or design, take upon them, with great boldness, to affirm what they please about privileg'd subjects: and, when they are oppos'd in their extravagancies, by reasonings they cannot answer, they urge, that these things, being above reason, are not to be judg'd of by it. But, of such men as these, I usually demand, whether their own assent to the things they would have us believe, be grounded upon some rational argument, or not. If they say, 'tis not, they are fools to believe it themselves; and I should add to the number, if, after this acknowledgment, I believ'd them. But, if they say, it is, I desire them to produce their argument; for, since 'tis framed by a human understanding, the force of it may, also, be comprehended, and judg'd of by a human understanding: and 'tis to no purpose to say, the subject surpasses human reason; for, if it do so indeed, it will surpass theirs, as well as mine, and so leave us upon even terms. And, let the thing assented to, be what it will, the assent itself ought to be founded upon a sufficient reason; and, consequently, upon one that is intelligible to the human understanding, that is wrought on by it.

The positive proofs requir'd for an assertion, about a privileg'd thing, must, we say, be sufficient in their kind; but we are not to expect rigid demonstrations of such assertions: for, since 'tis manifest, that there are many truths, such as historical and political ones, that, by the nature of the things, are not capable of mathematical, or metaphysical demonstra-

PHYSICS: tions, yet, being really truths, have a just title to our assent; it must be acknowledg'd, that a rational assent may be founded upon proofs that reach not to rigid demonstrations: it being sufficient, that they are strong enough to make a wise man acquiesce in them. And therefore, if any things can be made out to be reveal'd by God, concerning his own nature, actions, or decrees, we ought to receive them; because, of some of those things, as his prescience, mercy, &c. we can have no better proofs; and of others, as, what he did before our world was made, and what he will do with us after we are dead, we can have no other considerable proofs at all. There is no reason to think, that because an object surpasses the human understanding, it must, therefore, surpass the divine intellect itself. Even in the things that are transacted in the mind of man, I may learn from another, who is not my superior, what I can by no means attain to know, unless he be pleas'd to discover it to me.

Intelligibility to the human understanding, seems no more necessary to the truth, or existence of a thing, than that visibility, to a human eye, should be necessary to the existence of an atom, or of a corpuscle of air, or of the effluvia of a load-stone, or the fragrant exhalations of amber-greece, or musk. The natural incapacity of a child, to understand the abstruse properties of parabolas, hyperbolas, and the incommensurable lines of a square, hinders not those figures from existing, or their properties from being true and demonstrable. And, tho' we do admit some privileg'd things, yet there is no necessity that we should be debarr'd from all knowledge of those sublime objects, in which there are many things, whereof we must confess ourselves ignorant. The ancient geometricians knew very well what a rectangular triangle was, when they conceiv'd it to be a figure consisting of three strait lines, two of which, comprize a right angle; tho' probably, for a great while, they did not know all its chief properties; since, for ought appears, before *Pythagoras*, it was not known that the square of the hypotenuse, is equal to the squares of both the other sides taken together; and much more likely it is, that they were not able to solve those difficulties which attend the endless divisibility of lines inferable from that equality.

But every thing hard to be understood, or contrary to the common rules of probability, has not a right to pass for privileg'd; * for then, the paradoxes about surd quantities, the duplicate proportion, and several othersurprising doctrines, capable of mathematical demonstrations, would be

* " Let it be propos'd (says the ingenious Mr. *Hawkebee*) to represent the figure of an object, placed behind an opake body, upon the contrary side of that opake body; and this without the help of optic-glasses, or any foreign adventitious lights: perhaps the solution might be thought impossible; or the very terms of the problem absurd and contradicto-

ry. For the body, on which the figure is to be seen, must be opake by the hypothesis, and the object placed on the contrary side to that whereon 'tis seen; so that either the light must be transmitted thro' this body, and then 'tis not opake, contrary to the supposition; or else the light must not be transmitted, and then no figure could

" be

be privileg'd. Nor are all those worthy of this title, that are by many propos'd and embrac'd as philosophical mysteries; such as substantial forms, which are really only scholastic chimeras. But tho' I shall not presume, positively, to set down the discriminating bounds, and signs of privileg'd things; yet most, if not all of them, are either primary in their kind, as God himself, and the things whose nature flows immediately from him; or else, things that, if thoroughly inspected, necessarily involve the consideration of some kind of *Infinitum*; or else, are such, that tho' in some principal questions about them, one side must be taken, both sides are encumber'd with absurdities, or scarce superable difficulties. These being the usual marks that belong to privileg'd things, their number cannot be very great; and therefore, we shall not, by allowing them, want objects whereon to exercise our faculties.

2dly, We should not be hasty to frame negatives about privileg'd things; or to reject explications, or propositions of them, as if they were absurd, or impossible. *A second rule.*

We observe, that even in natural things, 'tis very unsafe to affirm, or reject opinions, before men have a competent historical information of what belongs to the subject they take upon them to judge of. And therefore, it must, in reason, be thought much more unwary to be forward to resolve upon negative propositions, about things which, since we ourselves acknowledge to be above the reach of human reason, 'twill become us, at least to forbear a rude, and insulting way of rejecting the opinions of learned men, who dissent from us, about such things; for the sublimity of the subject may render mistakes the more excusable, because difficult to be avoided; and our own sharing in the inability of penetrating such abstruse things, should keep us from being over-confident, that we, also, may not be mistaken; and incline us to tolerate other men's opinions, about matters wherein ourselves have only opinion, not science.

But, I must not be understood to speak against all framing of negative propositions, about privileg'd things; my design being to dissuade from doing it hastily: for, sometimes, 'tis much more easy and safe, to deny things,

" be seen; for all distributions of light,
" by optical artifices, are excluded.
" The thing, however, is plain matter of
" fact;" as he shews, by lining a globe
of glass, in part, with melted sealing-wax,
common sulphur, or melted pitch, and giving it a circular motion; for when his hand was then applied to the lined part, he distinctly, and perfectly saw the shape and figure thereof, upon the concave surface of the wax, &c. within. Hence that Gentleman thinks it may be useful to observe, " that many odd effects, and appearances, against the possibility where-

" of, men may seem to argue very plausibly, and to find downright absurdities, and contradictions in, may yet be brought about by the genuine force of nature, acting in convenient circumstances, upon proper and suitable bodies;" and therefore, that " we should not, upon such occasions, proceed to conclude too peremptorily, what may, or may not be done; and think that every difficulty, or apparent impossibility to us, is a real one to nature." See *Hauksb. Experiment. p. 167.—171. & p. 268.*

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PHYSICS.

than to affirm them to belong to a subject that surpasses our reason. And, the observation may be of use, especially in two cases; one, when the negative, we assert, is grounded not upon axioms taken from the usual course of nature, or upon propositions dubious, or remote from the first principles of knowledge; but upon either universal, or metaphysical axioms; or else upon truths manifestly flowing from some clear, tho' inadequate notion, we have of the nature of the things we treat. The other case is, when we have a clear and sufficient proof by revelation, or otherwise, of the positive attributes of the things we contemplate; for then we may safely deny of that subject, any other thing that is really inconsistent with the positive attribute. Upon which account, tho' we do not fully comprehend what God is, yet knowing by the clear light of nature, that he is a Being intelligent, and infinitely perfect, we may safely deny, against *Epicurus*, *Vorstius*, and *Mr. Hobbs*, that he is a corporeal substance; as also that he is mortal, or corruptible.

A third rule for
judging of
things above
reason.

3dly, A matter of fact, or other proposition about privileg'd things, being prov'd by arguments competent in their kind, we ought not to deny it merely because we cannot explain, or, perhaps, so much as conceive the *Modus* of it.

I have observ'd a want of clearness in several discourses, where the term *Modus* has been employ'd. Sometimes we would deny by it, so much as a possibility that one thing can belong to, or be truly said of another; as when we say, we understand not how one creature can create another, or how there can be a line that is neither strait nor crooked, or a finite whole number that is neither even nor odd. But most commonly we mean, by our not understanding the *Modus* of a thing, that we do not clearly and distinctly conceive after what manner the property, or other attribute, of a subject belongs to it, or performs its operations. The first kind may be call'd a possible *Modus*, and the other an actual *Modus*. Now in both the foregoing acceptations of the term, we may find instances fit for our present purpose. For we cannot imagine, how a short line, or other finite quantity, should be endlessly divisible; or, on the contrary, how infinite parts should make but a finite total: and yet geometry constrains us to admit that it is so. But tho' there be few instances of this kind, yet of the other sort of our ignorance of the *Modus* of things, there may be found more instances than we could wish there were; for even in natural and corporeal things, the eager disputes of the best philosophers, and the ingenuous confession of the most judicious and moderate, sufficiently manifest, that as yet we know not the manner of operation, whereby several bodies perform, what we well know they bring to pass. And not to enter into those nice and tedious disputes, about the cause of the cohesion of the parts of matter in the smallest, most principal, and most primary bodies; perhaps the way whereby the rational soul exercises any power over the human body, and the way whereby the understanding and the will act upon one another, have not yet been intelligibly explain'd by any. The like I may say of the phenomena of the memory; especially in those who
are

are eminent for that faculty. 'Tis hard to conceive, how in so narrow a compass as part of a human brain, there should be so many thousand distinct cells or impressions, as are requisite to harbour the characters or signatures of several languages, each of them consisting of many thousand different words; besides the images or models of so many thousand faces, schemes, buildings, and other sensible objects, and the ideas of so many thousand notions and thoughts, and the distinct traces of multitudes of other things: and how all these shall, in so small a space, have such deep and lasting impressions made for them, and be oftentimes lodg'd so exactly in the order wherein they were at first committed to the memory, that upon a sudden command of the will, or a slight casual hint, a whole sett of words, things, and circumstances, will in a trice, as it were, start up, and present themselves, even in the very series, order, and manner, wherein they long before were ranged. And I doubt not, that besides those abstruse things, about the *Modus* of which, the more candid philosophers have confess'd their ignorance, there would others have been taken notice of, if we did but as fully and impartially inquire into the nature of all the things we think we know. And considering the yet depending disputes between philosophers and mathematicians, about the nature of place and motion, which are things obvious and familiar to us; one might, without other inducements, be inclined to think, we should find many difficulties in many other subjects, wherein we do not now take notice of any, if we particularly studied their nature; and that our acquiescing in what we have learn'd about many things, proceeds not from our greater knowledge of their nature, but from our having exercised less curiosity and attention in considering it. And if in things corporeal, that are the familiar objects of our senses, we are often reduced to confess our ignorance of the *Modus* of their existing, or operating; it will not be denied, that to a Being wholly unapproachable by our senses, natural philosophy may be allow'd to ascribe some things, whose *Modus* is not attainable by our understanding: as the divine prescience, which, as 'twere impious to deny, so, I fear, 'tis impossible to explain, as to the *Modus* of it.

And, perhaps, upon the same grounds, 'tis as inconceivable, that God should see our thoughts, as how he can know our outward actions; for since we have no way of discerning the particular motions of mens bodies, but by some of our senses, especially our sight; and since those sensations themselves necessarily require organs duly constituted, that is, made up of several parts, framed, and join'd together after a determinate manner; I see not how we can explain the perception of visible objects, without an eye, or so much as any corporeal organ or substance; especially, since 'tis very justly asserted, that the deity is not united to any portion of matter, as the human soul is to the human body.

We do not, however, deny to God either the power of moving matter, or that of perceiving all its motions; for which there is some positive proof, competent in its kind. For if there be an effect, that we discern must proceed from such a cause or agent, we may conclude, that such a cause there

PHYSICS. there is; tho' we do not particularly conceive how, or by what operation, 'tis able to produce the acknowledg'd effect. Thus, tho' a man, otherwise of good judgment, being wholly a stranger to mathematics, cannot conceive how a skilful astronomer should, many years before-hand, foretel eclipses to a day and hour, and perhaps to a few minutes; yet when the success, as it often happens, verifies such predictions, he will be satisfied that the maker of them had the skill to foreknow the things foretold in them. And so the generality of learned men among us, who are not much acquainted with that part of navigation, which some moderns call *Limn-euretica*, or the art of steering to harbours, cannot well conceive how a ship that is, for instance, in the vast *Atlantic* ocean, above a thousand miles from any shore, should be so directed, as to arrive just at a little harbour, not cannon-shot over; which perhaps neither the pilot, nor any other in the ship, ever saw. And yet, as little as we distinctly conceive how such an art of finding ports can be framed, we scruple not to allow there is such an one, because navigators to the *East* and *West-Indies* could not, without it, find the remote ports they are bound for.

There is a second sort of positive proofs, consisting of those consequences that are clearly and justly drawn from any manifest, acknowledg'd, or already demonstrated truth. To this belong several mathematical propositions and corollaries; which, tho' being nakedly proposed, they seem incredible to the generality of learned men, and sometimes to mathematicians themselves, are yet fully assented to, because they clearly follow from either manifest or demonstrated truths. Thus, many cannot conceive how 'tis possible there may be millions of circles, whose circumferences shall each of them come nearer and nearer to another, and to a strait line assign'd; and yet none of them touch, and much less cut either any other circle, or that line, but in the same point: and yet this is one of those odd propositions that geometricians have rightly deduced, as corollaries from a proposition of *Euclid*. And, tho' we cannot clearly conceive, how two lines, that, at their remotest ends, are but little distant from each other, should perpetually incline to each other, without ever meeting; yet geometricians, that is, the most rigid reasoners we know of, have been compell'd to admit this in the conchoid of *Nicomedes*.

A fourth rule.

4thly, When we treat of privileg'd subjects, we are not bound to suppose every thing false, that seems to oppose some receiv'd dictate of reason.

For, it being evident, that a great part of the dictates of reason are negative, and that negative propositions usually spring from the repugnancy we judge some things have to a positive dictate of reason; if those positive dictates contain but gradual and limited truths, and come to be unduly extended to privileg'd subjects, it may very possibly happen, that a thing, really true, will appear false, if judg'd of by its agreement to one of those limited, and but respective dictates. 'Tis also clear, that, in natural philosophy, the usual ground on which we reject many things, is, that we judge them unintelligible. And, I censure not the practice in general, but
think

think it may easily mislead us, when extended to things that apparently transcend our reason, as, for ought yet appears, some of the *modus's*, even of things corporeal, are found to do. And, we sometimes think we have made compleat enumerations of the several ways of the existence of an attribute in a subject, or of the operation of one thing upon another, when, indeed, we have over-look'd one or other of them; and, perhaps, that which we have thus omitted, may be the true one; tho', possibly, no attention, and diligence of ours, could, in some cases, have serv'd the turn; the *Modus* inquired after, being not conceivable by us, tho' it may be by a higher than a human intellect.

The school-philosophers, for many ages, in the catalogues they made of the means of a body's working upon another at a distance, did not think of the true ways by which odours and sounds are communicated to us; and therefore had recourse to certain unintelligible things, which they call'd intentional species. But the moderns acknowledge, that odours are communicated by effluvia exhaled from the odorous body, and fitted to affect our nostrils; and that sounds are transmitted to the ear, by the undulating motion which the air is put into by the impulse of the vibrating, or otherwise agitated parts of the sonorous body.

And supposing the reasonable soul to be an immaterial substance; tho', men think they have sufficiently express'd the ways of determining the motion of a body, by saying, that the determination must be either in the line wherein the impellent made it move, or in the line wherein it was determin'd to move by the situation of the resisting body; yet the motions of the animal spirits, if not also some other internal parts of the body, may, the body being duly dispos'd, be determin'd by the human will; which is a way quite different from the other. And how the power of determining the motion of a body, without any power to impart motion to that body, should belong to an immaterial creature, which has no corporeal parts to resist the free passage of a body, and thereby change the line of its motion; is not yet, nor, perhaps, ever will be, in this life, clearly conceived by men: tho' there is no doubt, that he who endow'd the soul with this attribute or power, perfectly understands both how it exists in the soul, and how the soul, by exerting it, operates on the body.

I am, however, in no wise against rejecting opinions that are found contrary to those rules of reason, at the framing of which, the things in question were duly consider'd: but in cases not thought on, when such rules were devis'd, we are not always bound to submit to be judg'd by them; and to maintain an opinion unconformable to such a rule, may be not to oppose a genuine and absolute dictate of reason, but to rectify one that is erroneously thought so; by shewing that the rule is express'd in more general and indefinite terms than it ought to have been. And, doubtless, of two opinions, that is the most rational, which is most agreeable to those rules of reason, which are framed upon the fullest information. 'Tis easy to see, that in the rule I propose, very few of the cases occurring in ordinary discourse, or even in that of philosophers, will be at all concern'd.

PHYSICS. cern'd. And in those few cafes, wherein I intend the rule should take place, I obviate inconveniencies by a double caution. The first, by supposing that the opinion, which claims an exemption from the common rules, it not an arbitrary or precarious tenet, but sufficiently made out by proper arguments: and the second, by declaring that 'tis not to contradict right reason, but bad reasoners, to give limitation to rules which have been too hastily framed and conceived, in too general terms, by men who either were not competently inform'd of the variety of particulars, when they took upon them to make analyses and enumerations; or else presumed to infer, that a thing was not, because they did not understand the *Modus* of its existence or operation. And I have often thought, that the causes of the great clamour that is made against some men, for not obsequiously submitting to what some others call the rules of reason, are, that men do not sufficiently understand the nature of things, and of themselves; but entertain too narrow conceptions of the former, and too high an opinion of the latter.

Reason, what? Reason is often taken for a sett of notions and propositions, employ'd and acquiesced in by this or that sort of reasoners, who receive names from this or that particular discipline, as astronomy, chymistry, optics; of whose receiv'd doctrines they are supposed to be intirely maintainers. But it is also, with at least as much propriety, used to signify the rational faculty itself, furnish'd with the light that accompanies it, when it is rightly disposed and inform'd. In the first of these two senses, it seems but equitable to allow, that some things ought not to be judg'd by all the same rules employ'd to judge other things by; for some of these rules were framed upon a slight consideration of common and familiar things, either by the vulgar, or by men who, for want of skill or application of mind, did not critically regard the distinct natures of things; and yet presumed to settle rules, which other mens inadvertence or laziness has made them receive for certain dictates of reason: whereas, other natures should have been then consider'd, as well as those; but not having been so, the rules I speak of, are not always proper and safe, when applied to these overlook'd natures. Thus, successive Beings, as time and local motion, in some cafes require to be estimated by other measures than substances, whether material or incorporeal. And so, likewise, the more nice metaphysicians, especially among the moderns, have thought themselves obliged to discourse of modus's, relations, privations, extrinsecal denominations, &c. in a very different way from that which belongs to bodies and spirits; tho' the unskilful are still apt to confound all these subjects, by applying to them, indiscriminately, the same rules, or, as they imagine, dictates of reason.

The generality of philosophers, after *Aristotle*, conceive place to be the immovable and immediately contiguous concave surface of the ambient body; so that 'tis a kind of vessel, which every way contains the body lodg'd in it; but with this difference, that a vessel is a kind of moveable place; as, when a bottle of wine is carried from the cellar to the table:

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but place an immoveable vessel, or a vessel consider'd as immoveable. Now supposing, with *Aristotle*, and the generality of philosophers, the plenitude of the world; it may be truly said, that all plants, animals, minerals, stars, and other bodies, are each of them in such an *Aristotelian* place; whence it has been usually affirm'd, that what is in no place, is not at all: yet it appears not, how the outermost heaven can be properly said to be in a place; since these philosophers, asserting the world to be finite, must grant there is no ambient body without it, to contain it. And if the outermost heaven should be impell'd, by the power of God, in a strait line this way or that way, there would ensue a motion without change of place; for the outermost heaven was in none before, and does not, by its progression, come to be contained by a new ambient body. And in this case, even according to those modern favourers of *Aristotle*, who approve *Des Cartes's* definition of local motion, the world may be said to move, without changing place; for it does not pass from the neighbourhood of some bodies to that of others; since comprizing all bodies, and yet being bounded, there is no body for it to leave behind, nor any beyond, for it to approach: and therefore, tho' every particular body in the universe is naturally capable of local motion, yet the universe itself is not; and tho' every particular body in the world has some determinate figure, yet the world itself, if it be, according to the *Cartesians*, indefinite, has none.

Aristotle, and the philosophers since his time, have generally admitted the division, establish'd by him, of all Beings into substance and accident; and accommodated their rules to one of them, or to both. But *Gassendus*, and his followers, have introduced a third sort of Beings, as not being either substances or accidents. Of this kind of things, they make place or space to be; for they will not allow it to be a substance, because it is neither body nor spirit; but only somewhat that has a capacity to receive or contain bodies; and would subsist, tho' God should annihilate all the substances he has created. And, for the same reason, it is not to be call'd an accident, since that necessarily requires a substance to reside in; whereas, in case of the annihilation of the world itself, and consequently all the substances that compose it, their place or space would still remain, and be capable of admitting a new world of the same extent; if God should be pleased to create it: whence *Gassendus* wittily infers, that bodies are rather accidental in respect of space, than space in respect of bodies. But he who shall, with an heedful and unprejudiced eye, survey the several hypotheses, or systems, maintain'd by the different sects of philosophers, may find, that tho' the instances will not be all of them the same, yet there are none of these systems, wherein there may not be observ'd something or other, whereto every one of the rules, which reach to the other subjects treated of in that philosophy, cannot safely be applied. And, indeed, the mind of man being, naturally, far more desirous to know much, than to take the pains requisite to examine whether it does so or not; is very apt to think, that any small number of things it has not distinctly consider'd, must be of the same nature and condition with the rest, that it judges of

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the same kind. For by thus easily attaining to the knowledge of things, the mind gratifies, at once, both its vanity, and its laziness; looking upon these conclusions, as marks of the excellency of its rational faculty; whilst they rather proceed from a want of the due exercise of it.

But, since there is no progress *ad infinitum* in the *Criteria* of truth; and since our faculties are the best instruments that God has given us, to discover, and to examine it by; I think, a clear light, or evidence of perception, shining in the understanding, affords us the greatest assurance we can have, in a natural way, of the truth of the judgments we pass upon things. And, it is not by induction, but by evidence, that we know nothing but truth follows from truth. By which it appears, that the innate light of the rational faculty, is more primary than the very rules of reasoning; since, by that light, we judge even of this axiom, which is itself the grand principle of reasoning by inference. And, as the understanding is usually look'd upon to be the eye of the mind, there is this analogy between them, that there are some things which the eye may discern mechanically, or by the help of instruments; as when it judges a line to be straight, by the application of a ruler to it, or to be perpendicular, by the help of a plumb-line; or a circle to be perfect, by the help of a pair of compasses: but there are other things which the eye perceives and judges of immediately, by intuition, and without the help of organs, or instruments; as when, by the bare evidence of the perception, it knows, that this colour is red, and that blue; that snow is white, not black; and a coal, black, not white. For, thus there are some things, which the intellect usually judges of in a kind of organical way; that is, by the help of certain rules, or hypotheses, such as are a great part of the theorems, and conclusions in philosophy, and divinity; but there are others, which it knows without the help of these rules, more immediately, and, as it were, intuitively, by evidence, or perception; as that "two contradictory propositions cannot both be true;" that "from truth, nothing but truth can justly be deduced," &c. 'Tis also, upon this evidence of perception, that we receive, with an undoubted assent, many primitive ideas and notions; such as those of extended substance, or body; divisibility, or local motion, a straight line, a circle, &c.

And, it seems to me, that the internal light, which the author of nature has set up in the mind of man, qualifies him, if he makes a right use of it, not only to apply the instruments of knowledge, but also to frame, and to examine them. For, by the help of this light, the understanding is enabled to look about, and both to consider a-part, and compare together, the natures of all kind of things; without being necessitated to employ, in its speculations, the rules, or dictates of any particular science, or discipline; being sufficiently assisted by its own light, and those axioms and notions that are of a general nature, and perpetual truths; and so, of a higher order than the dictates or rules of any particular, or subordinate science, or art. And, by these means, the understanding may perceive the imperfection and falsity of such rules, or theorems, as those men who look no higher,

higher, nor further than their own particular science, or art, embrace for certain and unquestionable. Thus, philosophers observing, that they could frame a clear notion of a thing, without considering, whether it were actually in being, or not; or even when they supposed, that 'twas not actually in being; as we can frame a clear conception of a rose in winter, when there are none to be found growing; and have a notion of a myriagon, tho', very likely, there is no such figure really existing in the world; men have generally concluded, that the essence of things is different, and separable from their existence: yet, when we consider, that God is a Being infinitely perfect; and, that actual existence, being a perfection, must belong to him; we may, by the same light of reason, that dictated essence and existence to be two separable things in all other Beings, discern, that they must be inseparable in God; and, consequently, that the fore-mention'd rule, tho' more general than almost any other, is not absolutely universal, but must be limited by the light of reason. And thus, also, philosophers considering, that not only all sorts of bodies, but the immaterial souls of men, are endow'd with qualities, which are accidents, have included it in the very notion of a substance, to be the subject of accidents. But the free intellect, finding in itself a notion of an absolutely perfect, and, therefore, existent Being; and considering that to be the subject of accidents, is not a thing agreeable to the highest perfection possible, it concludes, that in God there are no accidents. And this conclusion has been embraced as a part, not only of christian, but of natural theology; and maintain'd, by several philosophers themselves, upon metaphysical, and other merely rational grounds. In short, the native light of the mind may enable a man, who will make a free, and industrious use of it, both to pass a right judgment of the extent of those very dictates, that are commonly taken for rules of reason; and to frame others on purpose for privileg'd things.

5thly, Where privileg'd things are concern'd, we are not bound to re- *A fifth rule.*
ject, as false, whatever we know not how to reconcile with something that is true.

I think, it will not be doubted, that there are, or may be conceiv'd, strait lines, whereof one is a hundred, or a thousand times longer than another; 'tis also generally granted, that a longer line consists of, or may afford more parts, than a shorter: and, lastly, 'tis generally acknowledg'd, that no number can be greater than infinite. Now, I should gladly see these propositions reconciled to the demonstrations of geometricians, about the endless divisibility of all strait lines; whence they deduce, that tho' they be very unequal among themselves, yet the shortest of them contains, or may afford, infinite parts. I am far from affirming, that one truth can really contradict another; yet, I think, that which is but gradual, or limited truth, may, in some few cases, be irreconcilable, by us, to an absolute and universal truth. We may distinguish those propositions, we call true, into axioms metaphysical, or universal, that hold in all cases, without reserve; and axioms collected, or emergent; by which, I mean such

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as result from comparing together many particulars, that agree in something common to them all. And some of these, tho' they be so general, that, in the usual subjects of our reasoning, they admit of no exceptions, yet may not be absolutely, and without limitation, true. Of this we have an instance, even in that axiom which, almost, all natural philosophers have supposed, and built on, that something cannot be made out of nothing; which, tho' at least, one of the highest, or gradual truths, may yet not be universally true; since, for ought we know, God, who is acknowledg'd a Being infinitely perfect, may possess, and have exercis'd the power of creating. And, in such cases as this, not to be able to reconcile a truth, concerning a privileg'd thing, with a proposition that generally passes for true, will not presently oblige us to reject either proposition, as false; but, sometimes, without destroying either, only to give one of them a due limitation, and restrain it to those sorts of things, on which 'twas at first grounded; and to which 'twas, because of man's ignorance, or inconsiderateness, not at first confined. And, if the miracles vouch'd for any religion, be any of them granted true, it cannot well be denied, that physical propositions are but limited, and such as I call collected truths; being gather'd from the settled phenomena of nature, and liable to this limitation, or exception, that they hold, only where the irresistible power of God, or some other supernatural agent, is not interpos'd, to alter the course of nature.

Now, the reason why we judge things are repugnant, being, that the notions, or ideas we have of them, seem to us inconsistent; if either of these notions be wrong framed, or be judg'd of, by an unfit rule, we may think those propositions to be contradictory that really are not. Thus those used to employ their imaginations about things, which are the proper objects of the intellect, are apt to pronounce others to be inconceivable, only because they find them unimaginable; as if the fancy, and the intellect, were faculties of the same extent. Upon which account, some have so grossly erred, as to deny all immaterial substances; and chose, rather, so far to degrade the Deity itself, as to impute to it a corporeal nature, than to allow any thing to have a being, that is not comprehensible by their imagination; which themselves acknowledge to be but a corporeal faculty. But, besides this mistake of things repugnant, which arises from the misapplication, or mismanagement of our discerning faculties, there may be another, that proceeds from the imperfection, and limitation of our understanding; which being unable to judge of privileg'd things, as it does of other objects, may, sometimes, be unable to discover the reconcileableness, which a more illuminated, and penetrating faculty, may discern. This may be illustrated, by what usually happens at sea, where there is a free prospect; when, looking towards the main, the sky, and the waters, seem to meet at the edge of the sensible horizon; tho', indeed, they are as far distant, as the heavens from the earth. On the other hand, if you skillfully mix together, the dry and fine powder of orpiment, and that of indigo, you will produce a green colour, as is known to painters; where-
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in the eye takes notice but of an uniform mixture, in which it distinguishes neither blue, nor yellow; but if you look on this mixture thro' a microscope, the former colour disappears, and you will plainly see, instead of it, blue and yellow grains of the powder, distinct from one another. These instances may serve to shew the weakness of our visive faculty; and the latter of them teaches, that a thing may appear one and different, as 'tis look'd upon by a more or less discerning eye. But an instance, more to our present purpose, is afforded by yellow diamonds, which, because of their colour, the generality of goldsmiths take to be counterfeit gems; tho' very skilful lapidaries will, by sure signs, discover, and acknowledge them to be true diamonds. Whence we learn, that a more skilful judge may discern an agreement in things that almost all other men think they see manifestly to be of different natures.

I have, also, several times observed, that men judge two things to be irreconcilable, not only when they are both of them represented to the understanding in the form of propositions, but when one of them is only a notion, or a current definition. For several of these notions contain in them a proposition, or are equivalent to it: as, when a circle is defined to be a figure contain'd by a line, all whose parts are equally distant from a point in the middle; this definition contains an affirmation of the essential property of a circle; which, by the generality of geometricians, is, therefore, distinguish'd from that conic section they call an ellipsis; tho' that be also a figure terminated by one curve line.

But, if a man has not genuine, and adequate notions of the things he judges of; he may confidently, and even upon very probable grounds, take things to be inconsistent, that, in reality, are not so. If an ordinary mathematician should hear one man say, that such a figure is an ellipsis, and another affirm it to be a circle; he would think their assertions to be inconsistent, having his mind prepossessed with an ellipsis being a conic section, whose properties must, therefore, he supposes, be very different from those of a circle; whereas, such wary geometricians, as the learned *Dr. Wallis*, will tell him, that the vulgar notions of conic sections are not adequate to the figures producible by them. 'Tis true, when a right cone is cut quite thro' by an inclining plane, the figure produced by the section, agrees well with the receiv'd notion of an ellipsis, in which the diameters are of an unequal length; yet if the plane cut the cone parallel to the basis, that conic section will be a true circle, having all its diameters equal.

'Tis, indeed, an uncommon, and unheeded account, but such an one upon which I have observ'd, not only logicians, but philosophers themselves, to err about judging things reconcilable, or inconsistent; that if a man be not sufficiently acquainted with the nature of either of the two things under consideration, he may think there is a contradiction between them, when there is none to a superior understanding. For, taking it for granted, that he knows one thing for a truth; if some other thing be affirm'd to be so, which he has not skill enough to see how to reconcile to the

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take for a truth; provided the distrust'd proposition be such as we would acquiesce in, if we could reconcile it to the supposed truth.

Upon the whole, it appears, that when two propositions are laid down, one whereof is made evident to us by experience, or by reason, acting within its own jurisdiction, or compass; and the other sufficiently prov'd, by being mathematically demonstrated, or duly attested by divine revelation; we ought not to reject either of these propositions as no truth, merely because we do not yet know how to reconcile them: but we should rather think, that the collected proposition is only a gradual, or limited truth; or else consider, that we knowing but so imperfectly as we do, the particular natures of privileged subjects, there may, possibly, be a superior intellect, able to discern an agreement between what is deliver'd about that subject, and the affirmation which seems repugnant to it; tho' we are not quick-sighted enough to perceive this agreement.

Nor will this doctrine, if duly limited to the subjects wherein alone I would have it admitted, expose us to have falsities imposed on us at the pleasure of bold, and dictating men. For, if neither of the things be privileged, but both in the jurisdiction of ordinary reason; I require, that the propositions framed about them, be estimated according to the common dictates of reason. And, even in cases where one of the propositions is about a privileged thing; I do not at all think fit, that it should be receiv'd, in spite of its being repugnant to the gradual truth deliver'd in the other; unless it can, by some other argument, sufficient in its kind, be prov'd to be true. Thus, tho' men know not how to reconcile the liberty of their own will with the infallible knowledge that God has of those actions that flow from it; yet they have unanimously judg'd it reasonable to believe both free will, and prescience: the former, because they felt it in themselves; and the latter, because the fore-knowledge of things, being manifestly a perfection, ought not to be deny'd to God, whom they look'd upon as a Being supremely perfect; and because some actions, and events, that they all judg'd to flow from mens free will, were, as the generality believ'd, foretold by prophetic oracles. But, since we have scarce any way of discovering a falsity, but by its being repugnant to somewhat that is true; to deny, that, in cases within the jurisdiction of ordinary reason, the repugnancy of a proposition to any manifest truth ought to sway our judgments, were to deprive us of the most useful criterion to distinguish between truth and falsehood.

Lastly, in privileged things, we ought not always to condemn that opinion which is liable to ill consequences, and encumber'd with great inconveniences, provided the positive proofs of it be sufficient in their kind.

The sixth and last rule for judging of things above reason.

That this rule may be the more easily admitted, I shall, separately, suggest three things, which, I desire, may be, afterwards, consider'd all together.

First, clear positive proofs, proportionate to the nature of things, are genuine, and proper motives to induce the understanding to assent to a

PHYSICS. proposition as true; so that 'tis not always necessary to the evidence, and firmness of an assent, that the understanding takes notice of the consequences that may be drawn from it, or the difficulties wherewith it may be encumber'd. This is plain in those assents which, of all others, merely natural at least, are, by knowing men, thought to be the most undoubted, and the best grounded; I mean, the assents that are given to the truth of geometrical demonstrations: yet *Euclid*, for instance, in all his elements of geometry, in some of which surprizing paradoxes are deliver'd, contents himself to demonstrate his assertions in a mathematical way; and does not answer, or take notice of any one objection. And the geometers of our days think they may safely receive his propositions upon the strength of demonstrations annexed to them, without knowing, or troubling themselves with the subtilties employ'd by the sceptic *Sextus Empiricus*, or others of that sect, in their writings against the mathematicians, and all assertors of assured knowledge.

The second thing I would offer, is, that we have shewn there are some things which our imperfect understandings either cannot, or, at least, do not perfectly comprehend; and that nevertheless men have not refrain'd from presuming to dogmatize, and frame notions and rules about such things, as if they understood them very well. Whence it must needs come to pass, that if they were mistaken, as in things so abstruse 'tis very likely they often were; those who judge by the rules they laid down, must conceive the propositions opposite to their mistakes to be liable to very great, if not insuperable difficulties and objections.

Thirdly, as we need not wonder that privileg'd things, which are usually so sublime, as to have been out of the view of those who framed the rules whereby we judge of other things, should be thought liable to great objections by them who judge of all things only by those rules; so we should not require or expect more evidence of a truth relating to such things, than that there are for it such sufficient positive reasons, as notwithstanding objections and inconveniencies, make it, upon the whole, worthy to be embraced.

Euclid, indeed, besides that more satisfactory way of direct probation, which perhaps he might have oftener employ'd than he did, has sometimes, where he thought it needful, had recourse to a way of arguing to an absurdity. But in these cases, he never goes out of the discipline he treats of; and confining himself to arguments drawn from quantity, he urges nothing as absurd, but what is undeniably repugnant to some truth he had already demonstrated, or to those clear and undisputed definitions, axioms, or *Postulata*, which he supposes to have been already granted by those he would convince. But tho' he thus argues, to prove that his readers cannot contradict him without contradicting themselves; yet we find not that he was at all solicitous to clear those difficulties, that a man so quick-sighted could not but know some of his theorems were attended with; but he is contented to demonstrate the incommensurability of the
side

side and diagonal of a square, without troubling himself to take notice of *PHYSICS.* the seeming absurdities that attend the endless divisibility of a line, which would follow from what he demonstrated.

'Tis true, about some privileg'd things there are, and about some others there may be, contradictory opinions maintain'd. Now, as both of these cannot be true, one of them must be so; as, tho' it be hotly disputed whether quantity be endlessly divisible, yet certainly it either must, or must not be divisible without end: and, as was formerly observ'd, which side soever you take, the inconveniencies will be exceeding great; and, perhaps, there will lie objections against it scarce to be directly answer'd. And since one of the two opposite opinions must be true, it will not always be necessary that an opinion must be false, which is encumber'd with great difficulties, or liable to puzzling objections. And, therefore, if the positive proofs on one side be clear and cogent, tho' there be perplexing difficulties objected by the other; the truth ought not, for their sake, to be rejected: because such difficulties proceeding, usually, either from notions that men presume to frame about things above their reach, or from rules that were not made for such points as are in dispute; the objections are not to be judg'd so well founded, as is that acknowledg'd principle in reasoning, "from truth nothing but truth can be justly infer'd."

I confess, I have always thought it reasonable, in such cases, to compare as well the positive proofs of one opinion with those of the other, as the objections that are urg'd on either side; and thence make my estimate, upon the whole, tho' with a peculiar regard to that opinion which has a great advantage in point of positive arguments; because those are, unquestionably, the proper inducements to assent. And, then, the objections may well enough be suspected to proceed from the abstruse nature of privileged things, and the great narrowness of the rules whereby men usually judge of things. For we may have a sufficiently clear proof, that a thing is, whilst we have no satisfactory conception of its manner of existing, or operating; our infer'd knowledge being clearer, and extending farther, than our intuitive, or apprehensive knowledge.

But, even about things that we cannot sufficiently understand, we may, in some cases, exercise our reason, in answering objections that are thought unanswerable, because not directly so. For we may, sometimes, shew, by framing in another case a like argument, which the adversary must confess does not conclude well; that neither does the argument, containing his objection, conclude right.

However, we must not expect to be able, as to privileged things, and the propositions that may be framed about them, to resolve all difficulties, and answer all objections; since we can never directly answer those which require for their solution a perfect comprehension of what is infinite. As a man cannot well answer the objections that may be made against the *Antipodes*, the doctrine of eclipses, that of the different phases of the moon, and of the long days and nights of some months near the

PHYSICS. poles, the theory of the planets, &c. unless he understand the nature of the sphere, and some other principles of cosmography, and astronomy. So that where privileged things are concern'd, clear and positive arguments ought to be of great weight, in favour of the opinion they conclude for; even when, on the contrary side, we may discourse ourselves into such difficulties, as perplex, and, perchance, puzzle our limited understandings.



THE

T H E

Philosophical Difficulties

Relating to the

RESURRECTION,

CONSIDER'D.

THE question, I here design to consider, is, Whether to believe the resurrection of the dead, which the christian religion teaches, be to believe an impossibility? *Preliminary observations.*

I do not pretend, that the resurrection is a thing knowable, or directly provable, by the mere light of nature; nor that it may possibly be effected by mere physical agents: so that, treating of the possibility of the general resurrection, I here take it for granted, that God has been pleased to promise and declare, that there shall be one; and that it shall be effected, not according to the ordinary course of nature, but by his own immediate power.

It must also be observed, that the resurrection taught by the christian religion, is not here meant, in such a latitude, as to comprize all that any particular church, or sect of christians, much less what any private writer hath taught about it; but only what is plainly deliver'd as to this point, in the scripture.

I must further premise, that 'tis no easy matter to determine what is absolutely necessary, and but sufficient to make a portion of matter, consider'd at different times, or places, fit to be reputed the same body. That the generality of men, in vulgar discourse, allow themselves a great latitude, in this case, will be easily granted, by him who observes the receiv'd forms of speaking. Thus *Rome* is said to be the same city, tho' it hath been often taken, and ruin'd by the *Barbarians*, and others. Thus, an university is said to be the same, tho' some colleges fall to ruin, and new ones are built; and, tho' once in an age, all the persons who compos'd it, are succeeded by others. Thus, the *Thames* is said to be the same river, that it was in the time of our fore-fathers; tho' indeed, the water, *Identity: the difficulties of conceiving it.*
that

PHYSICS. that now runs under *London-bridge*, is not the same that ran there an hour ago, and is quite other than that which will run there an hour hence. And, so the flame of a candle, is said to be the same, for many hours together, tho' it, indeed, be every minute a new body; and tho' the kindled particles that compose it, at any time assign'd, are continually putting off the form of flame, and are repair'd by a succession of the like.

Nor is it by the vulgar only, that the notion of identity has been hard to be obtain'd; for, it seems, that even the ancient philosophers have been puzzled about it; witness their disputes, whether the ship of *Theseus* were the same, after it had been so far repair'd, from time to time, to preserve it as a monument, that scarce any plank remain'd of the former timber. And, even, in metaphysics, I think it no easy task, to establish a true and adequate notion of identity; and clearly to determine, what is the true principle of individuation. And, this is not surprizing; for, almost every man, who thinks, conceives in his mind, this, or that quality, relation, or aggregate of qualities, to be that which is essential to a certain body, and proper to give it a peculiar denomination; whereby it comes to pass, that, as one man chiefly respects this thing, and another that, in a body that bears such a name; so one may easily look upon a body as the same, because it retains what he chiefly consider'd in it; whilst another thinks it to be chang'd, because it has lost that which he thought was the denominating quality, or attribute. Thus philosophers, and physicians disagree about water, and ice; some taking the latter to be but the former disguis'd; because they are both of them cold, and simple bodies; and the latter easily reducible to the former, by being freed from the excessive adventitious degree of coldness; whilst others, looking upon fluidity, as essential to water, think ice, upon account of its solidity, to be a distinct species of body. And so the *Peripatetics*, and chymists often disagree about the ashes and *Calces* of burnt bodies; the first referring them to earth, because of their permanency, and fixedness; and many chymists taking them to be bodies *sui generis*, because common ashes usually contain a caustic salt; whereas earth ought to be insipid. And, the like may be said of some wood-ashes, and lime-stone, and even coral; which when well calcin'd, and fresh, have a pungent taste: besides, some of them, that are insipid, may be easily reduced into metals, as the *Calces* of lead and copper.

These difficulties, about the notion of identity, being observ'd, will render it less strange, that, among the ancient *Hebrews* and *Greeks*, whose languages were so remote, in several respects from ours, the familiar expressions, employ'd about the sameness of a body, should not be so precise, as those who maintain the resurrection, in the most rigid sense, would have them.

In the next place, I observe, we may, agreeably with scripture, suppose, that a comparatively small quantity of the matter of a body, being increas'd, either by assimilation, or other convenient apposition of parts, may bear the name of the former body; as a large crop of corn, arising from a comparatively small quantity of seed sown.

And,

And, here it may be proper to mention an experiment, made by two acquaintances of mine, men of great veracity and judgment; who both assured me, that having sown in a garden, some ashes of a plant, like our *English* red poppy, they, sooner than was expected, produced certain plants, larger and fairer than any of that kind, which had been seen in those parts. Now, this seems to argue, that in the saline and earthy, that is, the fix'd particles of a vegetable, that has been dissipated, and destroyed by the violence of the fire, there might remain a plastic power, enabling them to contrive dispos'd matter so, as to re-produce such a body as was formerly destroy'd. But, to this plastic power, residing in any portion of the destroy'd body itself, it will not, perhaps, be necessary to have recourse, in our present undertaking; since an external, and omnipotent agent, can, without it, perform all that I need contend for.

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To come, then, to a more close consideration of those difficulties, which are said to demonstrate the impossibility of the resurrection.

The first objection against the resurrection.

'Tis said, when a man is once really dead, many parts of his body will, according to the course of nature, resolve themselves into multitudes of steams, that wander in the air; and the remaining parts, which are either liquid, or soft, undergo so great a corruption, and change, that 'tis not possible, so many scatter'd corpuscles should be again brought together, and re-united, after the same manner wherein they existed in a human body, whilst it was yet alive. And, say they, much more impossible is it, to effect this re-union, if the body have been, as it often happens, devour'd by wild beasts; or fish; since, in this case, tho' the scatter'd corpuscles of the carcass might be recover'd, as particles of matter; yet, having already pass'd into the substance of other animals, they are quite transmuted by the new form of the beast, or fish, that devour'd them; and of which they now make a substantial part.

Yet, far more impossible will this renewal be, if we put the case, that the body was devour'd by *Cannibals*; for then, the same flesh, belonging successively to two different persons, 'tis impossible that both should have it restored to them at once; or, that any footsteps should remain of the relation it had to the first possessor.

But, in answer to this grand objection, I have several things to offer.

Answer'd.

And, 1st, A human body is not like a statue of brass, or marble, that may continue, as to sense, whole ages, in a permanent state; but is in a perpetual flux, or changing condition; since it grows, in all its parts, and all its dimensions, from a corpuscle no bigger than an insect, to the full stature of a man; which could not happen but by a constant apposition, and assimilation of new parts, to the primitive ones, of the little embryo. And since men, as other animals, grow but to a certain degree, and till a certain age, and therefore must discharge a great part of what they eat and drink by insensible perspiration, which *Sauvartius's* statical experiments, and mine, assure me to be scarce credibly great, as to men, and some other animals, both hot and cold; it will follow, that in no very great compass of

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of time, a large part of the substance of a human body, must be changed. Yet 'tis considerable, that the bones are of a stable and lasting texture; as I found, not only by some chymical trials, but by the skulls and other bones of men, whom history records to have been kill'd an exceeding long time ago.

2dly, There is no determinate bulk, or size, necessary to make a human body pass for the same; and a very small portion of matter, will sometimes, serve the turn. Thus an embryo, for instance, in the womb, a new-born child, a man at his full stature, and a decrepit, aged person, notwithstanding the vast difference of their sizes, are still reputed to be the same person; as is evident by the custom of crowning kings and emperors in the mothers womb; and by putting malefactors to death in their old age, for crimes committed in their youth. And, if a very tall, and unwieldy fat man, should, as it sometimes happens, be reduced by a consumption, almost to a skeleton; yet none would deny that this wasted man, were the same with him that had once so vast a body.

3dly, A body may either consist of, or abound with such corpuscles, as may be variously associated with those of other bodies, and exceedingly disguised by the mixtures, yet retain their own nature: and of this we have various instances in metals. Thus gold, for example, when dissolv'd in *Aqua regia*, passes for a liquor; and, when dextrously coagulated, appears a salt, or vitriol; by another alteration, I have made it part of the fuel of a flame; being dextrously conjoin'd to another mineral, it may be reduced to glass; well precipitated with mercury, it makes a glorious transparent powder; precipitated with spirit of urine, or oil of tartar *per deliquium*, it makes a fulminating calx, that goes off very easily, yet is far stronger than gun-powder; precipitated with another certain alkali, the fire turns it to a fix'd and purple calx. But notwithstanding all these, and various other disguises, the gold retains its nature, as may be prov'd by chymical operations, especially by reductions. And mercury, is of a more changeable nature than gold; sometimes putting on the form of a vapour; sometimes appearing in that of an almost insipid water; sometimes assuming the form of a red powder; sometimes that of a white, or yellow one; of a crystalline salt; of a malleable metal; and of what not? Yet, all these are various dresses of the same quick-silver, which a skilful artist may easily make it put off, and appear again in its native shape*.

And,

* If gross bodies, and light, be mutually convertible into each other, as Sir I. Newton seems to think they are; what transmutations may there not be, in the compass of nature and art? Nature, as Sir Isaac observes, seems delighted with transmutations: "Water (says he) she changes into vapour; which is a sort of air; and, by cold, into ice; which is a hard, pellucid,

" fusible, brittle stone; and this stone re-
 " turns into water by heat; and vapour
 " returns into water by cold. Earth, by
 " heat, becomes fire; and, by cold, re-
 " turns into earth. Dense bodies, by fer-
 " mentation, rarify into several sorts of
 " air; and this air, by fermentation, and
 " sometimes without it, returns into dense
 " bodies. Mercury sometimes appears in
 " the

And, tho' it be true, that instances of the permanence of corpuscles undergoing successive disguises, may be much easier found among metals and minerals than vegetables and animals; yet there are some to be met with among these too: for, not to mention *Hippocrates's* affirmation, about purging a child with the milk of an animal, that had taken elaterium, I once, in *Savoy*, observ'd all the butter, that was made in some places, during the spring season, tasted very much of a certain weed, which, at that time, abounds in the fields there. And, considering how many elaborate alterations the rank corpuscles of this weed must have undergone, in the various digestions in the cow's stomach, heart, udder, &c. and that afterwards, two separations, at least, were made; the one of the cream from the rest of the milk; and the other of the unctuous parts of the cream, from the serum; it will scarce be deny'd, that vegetable corpuscles may, by association, pass thro' various disguises, without losing their nature; especially since the essential attributes of such corpuscles, may remain undestroy'd, tho no sensible quality survive, to make proof of it, as is afforded by our example in the offensive taste. And, besides what we commonly observe on the sea-coast, of the fishy taste of those sea-birds, that feed only upon fish; I purposely inquired of an observing man, who lived upon a part of the *Irish* coast, where the custom is to fat their hogs with a sort of shell-fish, wherewith that place very much abounds, about the taste of their pork; and he assured me, that the flesh had so strong and rank a savour of the fish, that strangers could not endure to eat it. There is a certain fruit in *America*, well known to our *English* planters, which many of them call the prickly-pear, whose very red juice, being eaten with the pulp of the fruit, of which it is a part, passes thro' the various strainers, and digestions of the body, so unalter'd, as to render the urine red enough to persuade those, who are unacquainted with this property, that they make bloody urine; as I have been several times assured, by unsuspected eye-witnesses. But, more odd is that, related by a learned man, who spent several years upon the *Dutch* and *English* plantations, in the *Charibbe* islands; when speaking of

" the form of a fluid metal, and sometimes
 " in the form of a hard, brittle metal;
 " sometimes in the form of a corrosive,
 " pellucid salt, called sublimate; some-
 " times in the form of a tasteless, pellu-
 " cid, volatile white earth, call'd, *Mer-*
 " *curius dulcis*; or, in that of a red, opaque,
 " volatile earth, call'd, cinnabar; or, in
 " that of a red, or white precipitate; or,
 " in that of a fluid salt: and distillation
 " turns it into vapour; and being agita-
 " ted in *vacuo*, it shines like fire. And,
 " after all these changes, it returns into its
 " first form of mercury. Eggs grow from
 " insensible magnitudes, and change into
 " animals; tadpoles into frogs; and worms

" into flies. All birds, beasts, and fishes,
 " insects, trees, and vegetables, with their
 " several parts, grow out of water, and wa-
 " try tinctures, and salts; and, by putrefa-
 " ction, return again into watry substan-
 " ces. Water, standing a few days, in
 " the open air, yields a tincture, which
 " (like that of malt) by standing longer,
 " yields a sediment, and a spirit; but,
 " before putrefaction, is fit nourishment
 " for animals and vegetables. And, a-
 " mong such various and strange trans-
 " mutations, why may not nature change
 " bodies into light, and light into bodies?"
Newton. Optic. p. 349, 350.

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Having thus shewn, that the particles of a body, may retain their nature, under various disguizes, I proceed to add, that they may be stripp'd of those disguizes; or, to speak without a metaphor, be extricated from those compositions wherein they are disguised; and, that, sometimes, by such ways, as those who are strangers to the nicer operations of nature, would never have thought on; nor will not, perhaps, judge probable, when proposed.

Tho' vitrification be look'd upon, by chymists, as the ultimate action of the fire, and the most powerful way of making inseparable conjunctions of bodies; yet, even out of glass of lead, for instance, made of sand, and the ashes of a metal; (tho' the transmutation seems so great, that the dark and flexible metal, is turn'd into a very transparent, and brittle mass) we have recover'd opaque, and malleable lead. And, tho' there be several ways, besides precipitations, of divorcing substances, that seem strictly, if not inseparably, united; yet, by precipitation alone, if a man have the skill to chuse proper precipitants, several separations may be easily and thoroughly made, that every one would not think of: for, 'tis not necessary, that, in all precipitations, as is observ'd in most of the vulgar ones, the precipitant should, indeed, make a separation of the dissolv'd body, from the mass or bulk of that liquor, or other adjunct, whereto 'twas before united, and not be able to perform this, without associating its own corpuscles with those of the body it should rescue, and so making, in some sense, a new and farther composition. That some bodies may precipitate others, without uniting themselves with them, is easily prov'd by the experiment of refiners in separating silver from copper; for the mixture being dissolv'd in *Aqua fortis*, if the solution be afterwards diluted, by adding fifteen or twenty times as much common water, and you put into this liquor a copper-plate, you shall quickly see the silver begin to adhere to the plate; not in the form of calx, as when gold is precipitated to make *Aurum fulminans*, or tin-glass, to make a fine white powder for a fucus; but in the form of a shining metalline substance, that needs no farther reduction, to be employ'd as good silver. And, by a proper precipitant, I have also, in a trice, reduced a large quantity of well-disguized mercury, into running quick-silver. And, if one can well appropriate the precipitants to the bodies they are to recover, very slight, and unpromising agents, may perform great matters in a short time.

If you let a piece of camphire lie a while upon oil of vitriol, shaking them now and then, it will be so corroded by the oil, as totally to disappear therein, without retaining so much as its smell, or without any manifest sign of there being camphire in that mixture; yet that a vegetable substance,

substance, thus swallow'd up and changed by one of the most fretting and destroying substances known in the world, should not only retain the essential qualities of its nature, but be restorable to its obvious and sensible ones in a minute, and that by so unpromising a medium, as common water, will appear by pouring the solution into a large proportion of that fluid, to whose upper part there will immediately rise white, brittle, strong-scented, and inflammable camphire, as it was before.

'Tis here a principal consideration, that all bodies being but parcels of the universal matter mechanically different, they may successively put on forms, in a way of circulation, till they return to their original form, whence they first began; by having only their mechanical properties alter'd.

That all bodies agree in one common matter, the schools themselves allow; making what they call the *Materia prima* to be the common basis of them all; and their specific differences to spring from their particular forms: and since the true notion of body consists either alone in its extension, or in that and impenetrability together, it will follow, that the differences which make the varieties of bodies we see, must not proceed from the nature of mere matter, of which we have but one uniform conception; but from certain attributes, such as motion, size, position, &c. that we call mechanical affections. Hence a determinate proportion of matter being given, if we suppose that an intelligent and otherwise duly qualify'd agent were to watch this portion of matter in its whole progress thro' the various forms it is made to put on, till it come to the end of its course or series of changes; and that this intelligent agent should lay hold of this portion of matter, cloth'd in its ultimate form; and, extricating it from any other parcels of matter wherewith it may be mix'd, make it exchange its last mechanical properties for those which it had when this agent first began to watch it: in such a case, I say, this portion of matter, how many changes and disguises soever it may have undergone in the mean time, will return to be what it was; and if it were before part of another body to be reproduced, it will become capable of having the same relation to it, that formerly it had.

Thus, suppose a man to cut a large sphere of soft wax into two equal parts, and of the one to make cones, cylinders, rings, screws, &c. and kneading the other with paste, make an appearance of cakes, vermicelli, wafers, biskets, &c. 'tis plain, that one may, by dissolution, and other ways, separate the wax from the paste, and reduce it in a mould to the same hemisphere of wax it was before; and so we may destroy all that made the other part of the wax pass for cones, cylinders, rings, &c. and reduce it in a mould to one distinct hemisphere, fit to be re-conjoin'd to the other; and so to recompose such a sphere of wax, as they constituted before the separation was made. And to view precipitate, carefully prepared *per se*, one would think that art had here made a body extremely different from common mercury; this being consistent, like a powder, very red in colour, and purgative, and for the most part vomitive in operation, tho' given but in the quantity of four or five grams: yet if you but urge this powder with a due heat; by putting the component particles into a new and fit motion,

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you may re-unite them together, so as to re-produce the same running mercury you had before the precipitate *per se* was made of it.

4thly, But the christian doctrine doth not ascribe the resurrection to nature, or any created agent, but to the peculiar and immediate operation of God; who has declared, that he will raise the dead. Wherefore, when I mention chymical ways of recovering bodies from their various disguizes, I am far from desiring, that such ways should be thought the only ones, or the best that can possibly be employ'd to such an end. For, as the generality of men, without excepting philosophers themselves, would not have believed or thought, that by easy chymical ways, bodies, which are reputed to have pass'd into quite another nature, should be reduced or restored to their former condition; so, till chymistry, and other parts of natural philosophy, be more thoroughly understood, and farther promoted, 'tis probable, that we can scarce now imagine, what expedients to re-produce bodies, a further discovery of the mysteries of art and nature may lead us to. And much less can our slender knowledge determine, what means, even of physical ones, the most wise author of nature is able to employ, to bring the resurrection to pass; since 'tis a part of the imperfection of inferior natures, to have but an imperfect apprehension of the powers of one that is incomparably superior to them. And even among ourselves, a child, who is endow'd with a reasonable soul, cannot conceive how a geometrician should measure inaccessible heights and distances; much less, how a cosmographer can determine the whole compass of the earth and sea; or an astronomer shew how far 'tis from hence to the moon; and tell many years before-hand, what day and hour, and to what degree, she will be eclipsed. And, indeed, in the *Indies*, not only children, but rational men, could not perceive how 'twas possible for the *Europeans* to converse with one another, by the help of a piece of paper, at a hundred miles distance; and in a moment produce thunder and lightning, and kill men a great way off, as they saw done by guns; and much less, how they should foretel an eclipse of the moon, as *Columbus* did, to his great advantage: which things made the *Indians*, even the chiefest of them, look upon the *Spaniards* as persons of more than human nature. Now, among those who have a true notion of a Deity, a Being both omnipotent and omniscient, that he can do all, and more than all, that is possible to be perform'd by any way of disposing of matter and motion, is a truth that will be readily acknowledg'd; since he was able at first to produce the world, and contrive some part of the universal matter of it, into the bodies of the first man and woman. And that his power extends to the re-union of a soul and body, which have been separated by death, we may learn from the experiments God has been pleas'd to give of it, both in the old testament and the new; especially in the raising *Lazarus* and *Christ* again to life: of the latter of which, particularly, we have proofs strong enough to satisfy any unprejudiced person, who desires but competent arguments to convince him.

Since then a human body is not so confined to a determinate bulk, but that the same soul being united to a portion of duly-organiz'd matter, is said to constitute the same man, notwithstanding the vast differences of big-
 neiss

ness that there may be, at several times, between the portions of matter whereto the human soul is united: since a considerable part of the human body consists of bones, which are bodies of a very determinate nature, and not apt to be destroy'd by the operation either of earth or fire: since of the less stable, and especially the fluid parts of a human body, there is a far greater expence made, by insensible transpiration, than even philosophers would imagine: since the small particles of a resolved body may retain their own nature, under various alterations and disguizes, of which 'tis possible they may be afterwards stript: since, without making a human body cease to be the same, it may be repair'd and augmented by the adaptation of fitly-disposed matter to that which pre-existed in it: since these things are so, why should it be impossible, that a most intelligent agent, whose omnipotence extends to all that is not truly contradictory to the nature of things, or to his own, should be able so to order and watch the particles of a human body, that of those remaining in the bones, of those that plentifully fly away by insensible transpiration, and of those that are otherwise disposed of upon their resolution, a competent number may be preserved or retrieved; so that stripping them of their disguizes, or extricating them from other parts of matter, to which they may happen to be conjoin'd, he may re-unite them betwixt themselves, and, if need be, with particles of matter fit to be intervoven with them; and thereby restore or reproduce a body, which being united with the former soul, may, in a sense agreeable to the expressions of scripture, re-compose the same man, whose soul and body were formerly disjoin'd by death.

sibly, Hitherto we have taken the doctrine of the resurrection in a more strict and literal sense, because I would shew, that even according to that, the difficulties of answering what is mention'd against the possibility of it, are not insuperable; tho' it would much facilitate the defence and explanation of so abstruse a thing, to allow, that as the human soul is the form of man, so that whatever duly-organiz'd portion of matter is thereto united, it therewith constitutes the same man; the import of the resurrection is fulfill'd in this, that after death there shall be another state, wherein the soul shall no longer persevere in its separate condition, but be again united, not to an ethereal, or the like fluid matter, but to such a substance, as may, with tolerable propriety of speech, be call'd a human body.

They who assent to this account of the possibility of the resurrection of the same bodies, will, I presume, be much more easily induced to admit the possibility of the qualifications the christian religion ascribes to the glorified bodies of the raised saints. For, supposing the truth of the scripture, we may observe, that the power of God has already extended itself to the performance of such things, as import as much as we need infer; sometimes by suspending the natural actions of bodies upon one another, and sometimes by endowing human and other bodies with preternatural qualities. And indeed lightness, or rather agility, indifference to gravity and levity, incorruption, transparency, opacity, figure, colour, &c. being but mechanical affections of matter; it cannot seem incredible, that the most free and powerful

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powerful author of those laws of nature, according to which, all the phenomena of qualities are regulated, may, as he thinks fit, introduce, establish, or change them, in any assign'd portion of matter, and consequently in that whereof a human body consists. Thus tho' iron be a body above eight times specifically heavier than water; yet in the case of *Elisba's* helve, its native gravity was render'd ineffectual, and it ascended from the bottom to the top of the water: and the gravitation of *St. Peter's* body was suspended, whilst his master enabled him to walk on the sea. Thus the operation of the most active body in nature, flame, was suspended in *Nebuchadnezzar's* fiery furnace, whilst *Daniel's* three companions walk'd unhurt therein. And thus the body of our Saviour, after his resurrection, tho' it retain'd the very impressions that the nails of the cross had made in his hands and feet, and the wound of the spear in his side, as the history of the gospel assures us; was endow'd with far nobler qualities, than before its death. And, as the apostle tells us, that this great change of schematism in the saints bodies will be effected by the irresistible power of Christ; we shall not scruple to allow of such an effect from such an agent, if we consider how much the bare slight mechanical alteration of the texture of a body, may change its sensible qualities for the better. Thus, without any visible addition, I have several times changed dark and opaque lead, into finely-colour'd transparent glass, of a less specific gravity; and thick fetid smoke, into a bright and scentless flame.



THE

The Christian Virtuoso

Proposition I shall here endeavour to establish, is, that a man may be a virtuoso, or experimental philosopher, without forfeiting his christianity.

And first, 'tis certain, that a due course of experimental philosophy, greatly conduces to settle in the mind, a firm persuasion of the existence, and several of the chief attributes of God; which persuasion is, in the order of things, the first principle of that natural religion, we allow as the very foundation of reveal'd religion, in general.

That the consideration of the vastness, beauty, and regular motions of the heavenly bodies, the excellent structure of animals and plants, besides a multitude of other phenomena of nature, and the subserviency of most of these to man, may justly induce him, as a rational creature, to conclude, this vast, beautiful, orderly, and admirable system of things, which we call the world, was framed by an author supremely powerful, wise, and good, can scarce be deny'd by an intelligent and unprejudiced person.

And this is strongly confirm'd by experience, which witnesseth, that in almost all ages and countries, the generality of philosophers, and contemplative men, were persuaded of the existence of a Deity, from the consideration of the phenomena of the universe; whose fabric, and conduct, they rationally concluded, could not be justly ascribed, either to chance, or to any other cause than a divine Being.

But, tho' God hath manifested himself, even to such as consider things but superficially, by stamping upon several of the more obvious parts of his works, such conspicuous impressions of his attributes, that a moderate degree of understanding, and attention, may suffice to make men acknowledge his being; yet this assent is greatly inferior to the belief which the same objects are fitted to produce in him, who with care and skill considers them. For, the works of God are so worthy of their author, that, besides the impresses of his wisdom and goodness, left, as it were, upon their surfaces; there are a great many more curious and excellent tokens, and effects of divine artifice, in the hidden and innermost recesses of them: and these are not to be discover'd by the slight glances of the

lazy,

Experiment at philosophy leads to religion, in general.

By discovering the existence of God.

PHYSICS. lazy, and the ignorant; but require, the most attentive and prying inspection of curious, and well qualify'd minds. And, sometimes, in one creature there may be great numbers of admirable things, that escape a vulgar eye, which yet are clearly discernible by that of a true naturalist; who brings with him, besides a more than common curiosity, and attention, a competent knowledge of anatomy, optics, cosmography, mechanics, and chymistry. In short, God has couch'd so many things in his visible works, that the clearer light a man uses, the more he may discover of their unobvious beauty, and exactness; and the more fully, and distinctly discern those qualities that lie more concealed. And the more wonderful things he discovers in the works of nature, the more corroborating proofs he meets with to establish, and enforce the argument drawn from the universe, and its parts, to demonstrate, that there is a God: a proposition of so vast a weight, and importance, that it ought to endear every thing to us, that is able to confirm it, and afford us new motives to acknowledge, and adore, the divine author of things.

Now, with regard to this matter, we may confidently say, that experimental philosophy has a great advantage of the scholastic. For, in the *Peripatetic* schools, where things are ascribed to certain substantial forms, and real qualities; the accounts of nature's works may be easily given in a few words, general enough to be applicable on almost all occasions. But these uninstrusive terms neither oblige, nor conduct a man to deeper searches into the structure of things, their manner of being produced, and of operating upon one another; and, consequently, are very insufficient to disclose the exquisite wisdom which the omniscient maker has expressed in the peculiar fabrics of bodies, and the well regulated motions of them, or of their constituent parts. From the discernment of which things, nevertheless, it is, that there arises, by way of result, in the philosophic mind of an intelligent contemplator, a strong conviction of the being of a divine artificer, and a just acknowledgment of his admirable wisdom. To be told, that an eye is the organ of sight, and that this is perform'd by that faculty of the mind, which, from its function, is call'd visive, will give a man but a sorry account of the instrument, and manner of vision itself, or of the knowledge of the artificer who form'd the eye. And he who can take up with this easy theory of vision, will not think it necessary to bestow pains to dissect the eyes of animals, or study the writings of mathematicians, to understand the doctrine of it; and, accordingly, will have but mean thoughts of the contrivance of the organ, and the skill of the artificer, in comparison of the ideas that will be suggested of both to him, who, being thoroughly skill'd in anatomy, and optics, by their help takes asunder the several coats, humours, and muscles, of which that exquisite dioptrical instrument consists: and, having separately consider'd the figure, size, consistence, texture, transparency, or opacity, situation, and connexion of each of them, and how they are all adjust'd in the whole eye, shall discover, by the laws of optics, how admirably this little organ is fitted to receive the incident rays of light, and dis-

dispose them in the best manner possible, to give a lively representation of the numberless objects of sight.

'Tis easy to say, in general terms, that the world is wisely framed ; but, I doubt, it often happens, that men confess the creatures are wisely made, rather because they, upon other grounds, believe God to be a wise agent, than because so slight an account as the school-philosophy gives of particular creatures, convinces them of any divine wisdom in the creator. And tho' I am willing to grant, that some impressions of God's wisdom are so conspicuous, that even, as was said before, a superficial philosopher may thence infer, that the author of such works must be a wise agent ; yet, how wise an agent he has in those works expressed himself to be, none but an experimental philosopher can well discern. And 'tis not by a slight survey, but by a diligent, and skilful scrutiny, of the works of God, that a man must be, by a rational, and affective conviction, engaged to acknowledge, that the author of nature " is wonderful in counsel, and " excellent in working."

2. After the existence of the Deity, the next grand principle of natural religion is, the immortality of the rational soul ; the genuine consequence whereof is, the belief, and expectation, of a future, and everlasting state. For this important truth, many arguments may be alledged, to persuade a sober, and well-disposed man to embrace it : but, to convince a learned adversary, the strongest argument that the light of nature supplies us with, seems to be that afforded by real philosophy. For this teacheth us to form true, and distinct notions of the body, and the mind ; and thereby manifests so great a difference in their essential attributes, that shews the same thing cannot be both. This it makes out more distinctly, by enumerating several faculties, and functions, of the rational soul ; such as, to understand, and that so as to form conceptions of abstract things, of universals, of immaterial spirits, and even of that infinitely perfect one, God himself : and also, to conceive, and demonstrate, that there are incommensurable lines, and surd numbers ; to form chains of reasoning, and draw both cogent, and concatenated inferences about these things ; to express intellectual notions, *pro re nata*, by words, or instituted signs, to other men ; to exercise free-will about many things ; and to make reflections on its own acts, both of understanding, and will. For these, and the like prerogatives, that are peculiar to the human mind, and superior to any thing that belongs to the outward senses, or to the imagination itself, manifest, that the rational soul is a Being of an higher order than corporeal ; and, consequently, that the seat of these spiritual faculties, and the source of these operations, is a substance, that, being in its own nature distinct from the body, is not naturally subject to perish with it.

And, with regard to this truth, our virtuoso hath an advantage of a mere school-philosopher. For, being acquainted with the true and real causes of putrefaction, and other physical kinds of corruption ; and thereby discerning, that the things which destroy bodies, are the avolation, or other recess, of some necessary parts, and such a depraving transposition

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of the component portions of matter, as is altogether unsuitable to the structure, and mechanical modification, essential to a body of that species, or kind, whereto it belongs; he plainly perceives, that these causes of destruction can have no place in the rational soul; which being an immaterial spirit, and, consequently, a substance not really divisible, can have no parts expell'd, or transpos'd; and so being exempted from the physical causes of corruption, that destroy bodies, it ought to last always. And, being a rational creature, endow'd with internal principles of acting, as appears in free-will, it ought to live for ever, unless it please God to annihilate it; which we have no reason to suppose he will do. On the other hand, the modern *Peripatetics* maintain substantial forms, by some of them, stiled *Semi-substantia*; to which, in apes, elephants, and others, that pass for ingenious animals, they ascribe some such faculties, and functions, as seem to differ only in degree from those of the rational soul; but, how innocent soever their intentions may be, their doctrine tends greatly to weaken the chief physical way of proof, from whence the immortality of man's mind is justly inferr'd. For since, according to the *Peripatetics*, substantial forms are, as they speak, educed out of the power, or potentiality, of the matter; and so depend upon it, not only as to action, but as to being, that they cannot at all subsist without it; but when the particular body, as an herb, a stone, or a bird, is destroy'd, they perish with it; I think they give great advantage to atheists, and cavillers, to oppose the immortality of the mind.

For if to an ape, or other brute animal, there belongs a Being more noble than matter, that can actuate, and inform it, and make itself the architect of its own mansion, tho' so admirable as that of an ape, or an elephant; if this Being can, in the body it hath framed, perform all the functions of a vegetable soul; and, besides those, see, hear, taste, smell, imagine, infer, remember, love, hate, fear, hope, expect, &c. and yet be a mortal thing, and perish with the body; 'twill not be difficult for those enemies of religion, who are willing to think the soul mortal, to fancy, that human minds are only a more noble, but not less mortal kind of substantial forms: as, amongst sensitive souls themselves, which they acknowledge to be equally mortal, there is a great disparity in degree; that of a monkey, for instance, being very far superior to that of an oyster.

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3. A third chief principle of natural religion, and, consequently, of reveal'd religion, which supposes the former as its foundation, is a belief of the divine providence. And, in this grand article, as well as in the two foregoing, a man may be much confirm'd by experimental philosophy; both as it affords him positive inducements to acknowledge the article, and as it shews the great improbability of the two principal grounds, on one or other of which is founded the denial of God's providence.

A virtuoso, who, by manifold and curious experiments, searches deep into the nature of things, has great, and peculiar advantages, to discover, and observe, the excellent fabric of the world, as 'tis an immense aggregate of the several creatures that compose it; and to take notice in its

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particular parts, especially those that are animated, of such exquisite contrivances, and such admirable co-ordinations, and subordinations, in reference to each other, as lie hid from those who are not both attentive, and skilful. When our virtuoso contemplates the vastness, scarce conceivable swiftness, and yet constant regularity, of the various motions of the sun, moon, and other celestial lights; when he considers how the magnetism of the earth makes its poles constantly point the same way, notwithstanding its various motions; how, by daily turning about its own centre in four and twenty hours, it receives as much light, and benefit from the sun, and all the glorious constellations of the firmament, as if they, with all the vast heavenly region they belong to, mov'd about it in the same time; how, by its situation among them, it enjoys the regular vicissitudes of day and night, summer and winter, &c. how the several parts of the sublunary world are mutually subservient to one another, and most of them serviceable to man; how excellently the bodies of animals are contrived; what various, and suitable provision is made for different animals; how admirable, and astonishing a process is gone through in the formation of the fœtus; how various animals are endow'd with strange instincts, whose effects, sometimes, seem much to surpass those of reason itself: when, I say, a philosopher duly reflects on these things, and many others of the like import, he will think it highly rational to infer from them these three conclusions.

First, that a machine immense, beautiful, well contrived, in a word, so admirable as the world, cannot have been the effect of mere chance, or the tumultuous jostlings, and fortuitous concourse of atoms; but must have been produced by a cause, exceedingly powerful, wise and beneficent.

Secondly, that this most powerful author, and contriver of the world, hath not abandon'd a work so worthy of him, but still maintains, and preserves it; so regulating the stupendously swift motions of the great globes, and other vast masses of the mundane matter, that they do not, by any great irregularity, disorder the grand system of the universe, and reduce it to a chaos, or confused state of things.

Thirdly, that as it is not above the ability of the divine author, though a single Being, to preserve, and govern all his visible works, how great and numerous soever; so he thinks it not below his dignity, and majesty, to extend his care, and beneficence to particular bodies, and even to the meanest creatures; providing, not only for the nourishment, but for the propagation of spiders, and ants themselves. And, indeed, since the truth of this assertion, that God governs the world he has made, would appear by the constancy, and regularity, and astonishingly rapid motions of the vast celestial bodies, and by the long trains of as admirable artifices employ'd in the propagation of various sorts of animals; I see not why it should be deny'd, that God's providence may reach to his particular works here below, especially to the noblest of them, man; since most of those who deny this, as derogatory to God's majesty, and happiness, acknow-
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ledge, that, at the first formation of things, the great author of them must not only have extended his care to the grand system of the universe in general; but allow'd it to descend so low, as to contrive all the minute, and various parts, not only of greater, and more perfect animals, as elephants, whales, and men; but such small, and abject ones, as flies, ants, mites, &c. which being manifestly propagated by eggs, laid by the female, cannot reasonably be thought the offspring of putrefaction. Whence I gather, as from matter of fact, that to be concern'd for the welfare, even of particular animals, as it is agreeable to God's wisdom, and exuberant beneficence; so it is not truly derogatory to his adorable greatness, and majesty.

And, since man is the noblest of God's visible works; since very many of them seem made for his use; since, even as an animal, he is wonderfully made, and curiously, or artificially wrought; and since God has both given him a rational mind, and endow'd it with an intellect, whereby he can contemplate the works of nature, and by them acquire a conviction of the existence, and several attributes, of their supremely perfect author; and, lastly, since God hath planted notions, and principles in the mind of man, fit to make him sensible, that he ought to adore his maker, as the most perfect of Beings, the supreme Lord, and governor of the world; natural reason dictates to him, that he ought to express the sentiments he has for this divine Being, by a veneration of his excellencies; by gratitude for his benefits; by humiliation, in view of his greatness, and majesty; by an awe of his justice; by reliance on his power, and goodness; and, in short, by those several acts of natural religion, that reason shews to be suitable, and, therefore, due to those several divine attributes of his, which it has led us to acknowledge.

And here I shall add, that, from the *Cartesian* principles, a double argument may be drawn for divine providence.

For, first, according to the *Cartesians*, local motion, which is the grand principle of all action among things corporeal, is adventitious to matter; and was, originally, produced in it, and is still every moment continu'd, and preserv'd, immediately by God: whence it may be inferr'd, that he concurs to the actions of each particular physical agent; and, consequently, that his providence reaches to all, and every one of them.

And, secondly, the same *Cartesians* believe the rational soul to be an immaterial substance, really distinct, and separable from the body. Whence I infer, that the divine providence extends to every particular man; since, whenever an embryo, or little human body form'd in the womb, is, by being duly organized, fitted to receive a rational mind, God is pleas'd to create one, and unite it to that body. In which transaction, there seems to me a necessity of a direct, and particular intervention of the divine power; since I understand not, by what physical charm, or spell, an immaterial substance can be allured into this or that particular embryo, of many that are, at the same time, fitted to receive a human soul; nor by what merely mechanical ties, or bond, an immaterial substance can be durably

durably join'd, and united, with a corporeal one, in which it finds no parts, that it has organs to take hold of, and to which it can furnish no parts to be fasten'd upon by them. No better can I conceive, how a mere body can produce pain, pleasure, &c. by its own mere action, or endeavour to act on an immaterial spirit. No will the force of all this reasoning be eluded, by saying, with some deists, that, after the first formation of the universe, all things are brought to pass by the settled laws of nature. For tho' this be confidently, and, not without colour, pretended; yet, I confess, it does not satisfy me. For, not to mention the insuperable difficulty there is, to give an account of the first formation of things, which many deists will not ascribe to God; nor that the laws of motion, without which the present state, and course of the world, could never be maintain'd, did not necessarily spring from the nature of matter, but depended upon the will of the divine author of things; I look upon a law, as a moral, not a physical cause; as being, indeed, but a notional thing, according to which, an intelligent and free agent is bound to regulate its actions. But inanimate bodies are utterly incapable of understanding what a law is, or what it enjoins, or when they act conformably, or unconformably to it; and, therefore, the actions of inanimate bodies, which cannot incite, or moderate their own actions, are produced by real power, not by laws; tho' the agents, if intelligent, may regulate the exertions of their power by settled rules.

4. I have taken notice of two other accounts, upon which the experimental knowledge of God's works may, in a well-disposed mind, conduce to establish the belief of his providence.

And, first, when our virtuoso sees with how many, and how various, and how admirable structures, instincts, and other contrivances, the wise artificer hath furnished even brutes, and plants to acquire, and assimilate their food; to defend, or otherwise secure themselves from hostile things; to maintain their lives, and propagate their species; it will very much conduce to persuade him, that so wise an agent, who has at command so many differing, and excellent methods, and instruments, to accomplish what he designs; and, often, actually employs them for the preservation, and welfare of beasts, and even of plants, can never want means to compass his most wise, and just ends, with relation to mankind; being able, by ways that we should never dream of, to execute his purposes, and fulfil his promises.

Secondly, when we duly consider the very different ends to which many of God's particular works, especially those that are animated, seem design'd, with respect both to their own welfare, and the utility of man; and how exquisitely the great creator has been pleased to supply them with means admirably fitted to attain these respective ends; we cannot but think it highly probable, that so wise, and so bountiful a Being, has never left his noblest visible creature, man, unfurnished with means to procure his own welfare, and obtain his true end, if he be not wanting to himself. And, since man is endow'd with reason, which may convince him

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him, what neither a plant, nor a brute animal is capable of knowing, that God is both his maker and continual benefactor; since his reason likewise teacheth him, that upon both those accounts, besides others, God may justly expect and require worship and obedience from him; since also the same rational faculty may persuade him, that it well becomes the majesty and wisdom of God, as the sovereign governour of the world, to give a law to man, who is a rational creature, capable of understanding and obeying it, and thereby glorifying the author of it; since, finding in his own mind a principle, which tells him, he owes a veneration, and other suitable sentiments, to the divine author of his being, and his continual and munificent benefactor; since, on these accounts, his conscience will convince him of his obligation to all the essential duties of natural religion; and since, lastly, his reason may assure him, that his soul is immortal, and is therefore capable and desirous of being everlastingly happy, after it has left the body; he must in reason be strongly inclined to wish for a supernatural discovery of what God would have him believe and do. And therefore, if, being thus prepared, he shall be very credibly inform'd, that God hath actually been pleased to discover, by supernatural means, what kind of worship and obedience, which by reason alone he could but guess at, will be most acceptable to him; and to encourage man to both these, by express promises of that felicity, which man, without them, can but faintly hope for; he would be ready, then, thankfully to acknowledge, that this way of proceeding becomes the transcendent goodness of God, without derogating from his majesty and wisdom. And by these and the like reflections, a philosopher, who takes notice of the wonderful providence, that God descends to exercise for the welfare of inferior and irrational creatures, will have an advantage above men not vers'd in the works and course of nature, in believing, upon the historical and other proofs which christianity offers, that God has actually vouchsafed to man, his noblest, and only rational visible creature, an explicate and positive law, enforced by severe penalties denounced against the stubborn transgressors, and promising to the sincere observers of it, rewards suitable to his own greatness and goodness. And thus the consideration of God's providence, in the conduct of things corporeal, may convey a well-disposed mind from natural to reveal'd religion.

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philosophy draws
the mind from
sensual things.*

5. Another thing, that disposes an experimental philosopher to embrace religion, is, that his genius and course of studies accustom him to value and delight in abstract truths; by which term, I here mean such truths, as do not at all, or very little, gratify mens ambition, sensuality, or other inferior passions and appetites. For, as the generality of those who have an aversion to religion, are led to it by a contempt of all truths, that do not gratify their passions or interests; so, he who is addicted to experimental knowledge, is accustom'd both to pursue, esteem, and relish many truths, that do not delight his senses, or gratify his passions, or interests; but only entertain his understanding with that manly and rational satisfaction, which is naturally afforded by the attainment of clear and noble truths,

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its genuine objects and delight. And tho' the discoveries made by the help of physical or mechanical experiments, are not, for the most part, allied to revealed religion; yet, besides that some of them manifestly conduce to establish or illustrate natural religion, we may argue *à fortiori*, that he who is accustomed to prize truths of an inferior kind, because they are truths, will be much more disposed to value divine truths, which are of a vastly higher and nobler order, and of an inestimable and eternal advantage.

6. But farther, both the temper of mind, that qualifies a man for a virtuoso, and the way of philosophizing he chiefly employs, greatly conduce to give him a sufficient, well-grounded, and duly-limited docility; which is a great disposition to the entertainment of reveal'd religion. In the vulgar and superficial philosophy, wherein a man is allow'd to think that he has perform'd his part well, when he has ascribed things to a substantial form, or to nature, or to some real quality, whether manifest or occult, without proving that there are such causes, or intelligibly declaring, how they produce the phenomena, or effects refer'd to them; 'tis easy for a man to have a great opinion of his own knowledge, and be puff'd up by it. But a virtuoso, who cannot satisfy himself, nor dares pretend to satisfy others, till he can, by hypotheses that may be understood and prov'd, declare intelligibly the manner of the operation of the causes he assigns, will often find it so difficult a task to effect all this, that he must easily discern, he needs further information, and therefore ought to seek for it where 'tis the most likely to be had. Besides, the litigious philosophy of the schools seldom furnishes its disciples with better than dialectical or probable arguments, which are not proper fully to satisfy the person who employs them, or to leave his adversary without an answer, as probable as the objection: upon which account, men who have more wit than a sincere love for truth, will be able to dispute speciously enough, as long as they please. And as such slippery arguments are not able to convince even the person who uses them, if he be a man of judgment; so, if he deals with a witty adversary, they will leave him able to elude any arguments of the like nature, with which he shall be press'd. And in effect, we see, that in the *Aristotelian* philosophy there are several establish'd questions; such as, Whether the elements retain their distinct natures in a mix'd body? Whether the celestial orbs are moved by intelligences? &c. which have been disputed from age to age, and are like to continue questions for many more, if that philosophy shall last so long. But a virtuoso, who in his reasonings attends to the principles of mathematics, and sound philosophy, and to the clear testimonies of sense, or well verify'd experiments, acquires a habit of discerning the cogency of an argument, or way of probation; and easily perceives, that dialectical subtilties, and school-tricks, cannot shift off its force; but he finds more satisfaction in embracing a demonstrated truth, than in the vain glory of subtilly disputing against it.

7. Another thing that may, by means of experiments, dispose a studious searcher of truth for reveal'd religion, is, that his inquiries, and course of studies, make him both willing and fit to search out and discover deep

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deep and unobvious truths. I have, with trouble, observ'd, that the greater part of the libertines among us, being men of *Pilate's* humour, (who, when he had scornfully ask'd, what is truth? would not stay for an answer) with great disdain, decline the study of all truths that require a serious and settled application of mind. These men are, for the most part, a sort of superficial and desultory wits, who go no further than the outside of things, without penetrating into the recesses of them; and, being easily tir'd with contemplating one, pass quickly to another; the consideration whereof, they, with the same lightness, forsake. And, upon this account, among others, it is, that such men, though often much applauded by others, because the most are but superficial, as well as they, almost as seldom make good philosophers, as good christians. For tho' all the sound arguments, that may be brought to evince the truth of natural and reveal'd religion, be not abstruse; yet some of the principal, especially those that prove the existence and special providence of God, and the immortality of the soul, are, if not of a metaphysical, yet, at least, of a philosophical nature; and will scarce be clearly understood, and duly relish'd, but by a person capable of, and accusom'd to attentive and prolong'd speculations. Now, a man addicted to prosecute discoveries of truths, not only by serious meditation, but by intricate and laborious experiments, will not easily be deterr'd from effectually pursuing his end, by the toils or difficulties that attend the clearing of those notions, and matters of fact, whereon solid arguments for natural, or reveal'd religion, are founded; how remote soever those truths may be from vulgar apprehensions. In short, a superficial wit, such as is frequently found in libertines, and often helps to make them such, may be compared to an ordinary swimmer, who can reach but such things as float upon the water; whilst an experimental philosopher, like a skilful diver, not only obtains those things that lie upon the surface of the sea, but makes his way to the very bottom of it; and thence fetches up pearls, corals, and other precious things, that, in such depths, lie conceal'd from other men's sight and reach.

*Experimental
philosophy leads
to the christian
religion, in par-
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We have seen, then, that experimental philosophy is, in its own nature, favourable to religion, in general: it also greatly conduces to shew the truth of the christian religion, in particular.

This excellent religion is recommended to minds rightly disposed, by a great number of prerogatives, whereof I shall mention a few.

1. And first, the three grand arguments, that jointly evince the truth of the christian religion in general, are, in my opinion, the excellency of the doctrine, which makes it worthy to have proceeded from God; the testimony of the divine miracles, wrought to recommend it; and the great effects, produced in the world by it. Two of these three arguments are bottom'd upon matters of fact, and, consequently, are likely to be the most prevalent upon those who have a great veneration for experience, and are duly disposed to frame such pious reflections, as it warrants and leads to.

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Now, an experimental philosopher, who is master of others experience, as well as of his own, and duly qualified to reflect upon both, will find strong motives to the belief of christianity, in the two last of these arguments of its truth. We must here observe, that the word experience may admit of several senses, whereof one is far more comprehensive than another; and likewise, of several divisions, and distributions. For, besides its more restrained acceptation, it is sometimes set in contra-distinction to reason, so as to comprehend, not only those phenomena that nature or art exhibits to our outward senses, but the things we perceive to pass within ourselves, and all those ways of information, whereby we attain any knowledge that we do not owe to abstracted reason. So that, without stretching the word to its utmost extent, and to which it has been enlarg'd, it may be look'd upon as a very comprehensive term, and fit to be branch'd into parts. I shall, therefore, divide it into personal, historical, and supernatural, or theological.

I call that personal experience, which a man acquires immediately by himself, and which accrues to him by his own sensations, or the exercise of his faculties, without the intervention of any external testimony. 'Tis by this experience we know, that the sun is bright; fire, hot; snow, cold, and white; that, upon the want of aliment, we feel hunger; that we hope for future good; that we love what we judge good, and hate what we think evil; and discern that there is a great difference between a triangle, and a circle, and can distinguish them by it.

By historical experience, I mean that, which tho' it were personal in some other man, is but by his relation, or testimony, whether immediately, or mediately conveyed to us. 'Tis by this we know, that there were such men as *Julius Caesar*, and *William the conqueror*; and that *Joseph* knew *Pharaoh* had a dream, which the *Egyptian* wise-men could not expound.

Theological experience is that, by which we know what (supposing a divine revelation) God is pleased to relate, or declare concerning himself, his attributes, his actions, his will, or his purposes; whether immediately, as he sometimes did to *Job* and *Moses*, and constantly to our Saviour; or by the intervention of angels, prophets, apostles, or inspired persons; as he did to the *Israelites*, and the primitive christian church; and still does to us, by those written testimonies we call the scriptures.

By personal experience, we know that there are stars in the heavens; by historical experience, we know that there was a new star seen by *Tycho*, and other astronomers, in *Cassiopeia*, in the year 1572; and, by theological experience, we know, that the stars were made on the fourth day of the creation.

I do not, therefore, here take experience in the strictest sense of all, but in a greater latitude, for the knowledge we have of any matter of fact; which, without owing it to our reasoning faculty, we either acquire by the immediate testimony of our own senses, and other faculties, or, it accrues to us by the communicated testimony of others. And, even in common acceptation, the word experience is not always meant of that which

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Since, then, learned men, as well as custom, confine not the application of the word experience, to that which is personal, but employ it in a far greater latitude; I see not, why that, which I call theological experience, may not be admitted; since the revelations that God makes concerning what he has done, or purposes to do, are but testimonies of things, most of them matters of fact, and all of them such, as, (so far as they are merely revelations) cannot be known by reasoning, but by testimony; whose being divine, and relating to theological subjects, does not alter its nature, tho' it give it a peculiar and super-eminent authority.

To apply this distribution of experience, to the matters of fact, that recommend the credibility of the christian religion, I shall offer my thoughts in two distinct propositions.

P R O P. I

We ought to believe several things upon the information of experience, mediate and immediate, which, without that information, we should judge unfit to be credited; or, antecedently to it, actually judged contrary to reason.

This proposition may be understood, either of persons, or of things, and will hold true in both.

And, first, as to persons; if our own observation of what occurs among mankind, does not satisfy us, that we are oblig'd, after sufficient tryal, frequently to alter the opinions, which, upon probable reasons, we had before entertain'd, of the fidelity, prudence, justice, chastity, &c. of this or that person; we need only turn to the records of history, or appeal to the tribunals of judges. For, in both we find but too many instances and proofs from matters of fact, that persons look'd on, even by intelligent men, as honest, virtuous, and holy, have prov'd false, perfidious, disloyal, unjust, sacrilegious, perjur'd, &c. And, in the courts of justice, we find a great deal of time employ'd to detect not only civil transgressions;

as thefts, cheats, forgery, false-witness, adultery, and the like, perpetrated by those, who, before they were thoroughly sifted, pass'd for honest; but, even sins against nature; as, the murders of parents by their children, and children by their parents, &c. Whence 'tis plain, that we ought, upon the testimony of experience, to change the opinions we thought we had rationally taken up of persons. I now proceed to make good the proposition, in the sense I chiefly intended, which is, as it relates to things.

If experience did not inform and certify us, who would believe, that a little heap of light, black grains of matter, should be able to over-turn stone-walls, blow up whole castles and rocks themselves, and do those other stupendous things, that we see actually perform'd by gun-powder, made use of in ordinance, and in mines? Who would think, that two or three grains of opium, should so stupify a large human body, as to force a sleep; and often, even without that, suspend the sharpest torments in the cholic, gout, and other the most painful diseases, in patients of quite different ages, sexes, and constitutions; in whom also the diseases are produc'd by differing, or even by contrary causes? Who would believe, that the poison adhering to the tooth of a mad dog, tho', perhaps, so little, as to be scarce discernible by sense, should be able, after the slight hurt is quite healed, to continue in the warm, perspirable body of the bitten person, not only for some days, or months, but, sometimes, for very many years? And, after having lurk'd all that while, without giving any trouble to the patient, that it should, on a sudden, pervert the whole œconomy of his body, and produce a madness like that of the dog which bit him, discovering itself by that strange and fatal symptom, an *Hydrophobia*?

But, besides a multitude of instances that may be given of truths, which, were it not for experience, we should refuse to believe; because the small strength of such agents, seems altogether disproportionate to the effects ascribed to them; many other instances might be alledg'd, wherein we assent to experience, even when its informations seem contrary to reason, and that which, perhaps, we did actually, and, without scruple, take to be true.

Since gravity is the principle, that determines falling bodies to move towards the center of the earth; it seems very rational to believe, with the generality of philosophers, that, in proportion as one body is more heavy than another, so it shall fall to the ground faster than the other. Whence it has been inferr'd, that of two homogeneous bodies, whereof one, for example, weighs ten pounds, and the other but one pound; the former, being let fall from the same height, and at the same time with the latter, will reach the ground ten times sooner.

But, notwithstanding this plausible reasoning, experience shews us, (and I have purposely try'd it) that bodies of very unequal weight, let fall together, will reach the ground at the same time; or so near it, that 'tis not easy to perceive any difference in the velocity of their descent, from a moderate height.

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Tis generally taken for granted, by naturalists, as well as others, that strong and loud sounds, as they are heard much farther off, than fainter; so, if the sonorous bodies be equally distant from the ear, the very strong sound will arrive much sooner at it, than the other; yet, by the experiments of the moderns, about the velocity of sounds, (in making many of which, I have endeavour'd at accuracy) it appears, that weaker sounds are (at least, as to sense) transmitted through the air as swiftly as the stronger. And, indeed, 'tis often observ'd, that when cannons and muskets are discharg'd together, the reports of both arrive together at the ear.

It seems irrational to conceive, that a smaller and weaker load-stone, should draw away a piece of steel from a larger and stronger; yet, my experience (and that of others) proves, that, in some cases, this paradox is a truth.

It has generally, by philosophers, as well as other men, been look'd upon as manifest, and agreeable to reason, that cold condenses water, more or less, according to its degree; and, consequently, that ice is water reduced into a less bulk. But, 'tis plain, by experiments carefully made, that water is, by glaciation, rather expanded; or, at least, that ice takes up more room, than the water did before it was congeal'd. And of this sort of instances, it were easy to add a vast number.

And, to extend the force of our arguments, to that experience which is not immediate, or personally our own, but communicated by others; provided it be competently attested, and duly convey'd to us; there will need but a little reflection on what is judg'd reasonable, and freely practis'd, by philosophers themselves. For, how many conclusions have the modern naturalists admitted, tho' abstract reasoning never led men to make them, and even while plausible arguments, and the notions and axioms of the most generally receiv'd philosophy, were contrary to them? Thus, that in the heavens there should be generations and corruptions, was not only unobserv'd before the time of *Aristotle*, but is contradicted by his arguments; yet I, with many others, have seen great spots (perhaps bigger than *Europe*) generated and dissipated on or near the surface of the sun; and several of the modern philosophers and astronomers, having never beheld any of these, must take the phenomena upon the credit of those who have observ'd them. And much more must they do so, who, in spite of the vulgar philosophy, which made all comets sublunary, believe they are celestial bodies. For, that they appear above the concave of the moon's orb, we credit upon the affirmation of those who observ'd them; which very few have done themselves. And the height of the famous comet, or disappearing star, in *Cassiopeia*, in the year 1572, whereon so much stress is laid by our philosophers and mathematicians, is admitted and urged, chiefly from the opinion they have, not only of *Tycho's* veracity, but his skill in observing the motions and phenomena of that celestial light; and particularly its having no parallax.

In short, the great artichest of experimental history, Sir Francis Bacon, when he divides it but into three parts, assigns the second of them to what he calls preter-generations; such as monsters, prodigies, and other things; which being, as to us, but casualties, all those that happen'd in other times and places than we have lived in, we must take upon the credit of others. And yet these mediate experiments, by suggesting new instances of nature's power, and uncommon ways of working; and by overthrowing, or limiting, received rules and traditions; afford us a considerable and instructive part of natural history, without which, it would not be either so sound, or so compleat.

PROP. II.

We ought to have a great and particular regard to those things that are recommended to our belief, by what we have reduced to real, tho' supernatural experience.

For, 1. 'Tis manifest, that the most rational men scruple not to believe, upon competent testimony, many things, whose truth did no way appear to them, by considering the nature of the things themselves; nay, tho' what is thus believ'd upon testimony, be so strange, and, setting aside that testimony, would seem so irrational, that antecedently to it, the things, at last, admitted as truths, were actually rejected as errors, or judg'd altogether unfit to be believ'd. And the points wherein experience overrules that, which, before it happen'd, was judg'd to be most agreeable to reason, concern things merely natural, or civil, whereof human reason is held to be a proper judge; whereas, many of the points recommended by supernatural experience, concern things of a superior order; many of which are not to be adequately estimated by the same rules with things merely corporeal, or civil; and some of which, as the essence and manner of existence, and some peculiar attributes, of the infinite God, involve or require such a knowledge of what is infinite, as vastly exceeds the reach of our limited understanding.

But this is not all: for, 2. As 'tis, with justice, generally granted, that the better qualified a witness is, in the capacity of a witness, the stronger assent his testimony deserves; so we ought, of all the things that can be recommended to us by testimony, to receive those with the highest degree of assent, that are taught us by God, thro' the means of those persons who appear to have been commissioned by him to declare his mind to men. For the two grand requisites of a witness, being the knowledge he has of the things he delivers, and his faithfulness in truly delivering what he knows; all human testimony must, on these accounts, be inferior to divine testimony: since the latter is warranted both by the veracity of God, and by his boundless knowledge; which makes it as impossible he should be deceiv'd himself, as the other does, that he should deceive us. And, because that, for the delivery of divine testimony, it has often pleas'd God, who is a most free, as well as a most wise agent, to make use of unpro-

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missing persons as his instruments ; I shall not altogether overlook this circumstance, that an experimental philosopher so often increases his knowledge of natural things, by what he learns from the observations and practices, even of mean, and, perhaps, of illiterate persons, because they are conversant with the works of nature ; that he is not only willing to admit, but often curious to seek for informations from them : and therefore he is not likely to shew much reluctance in receiving the doctrines of revealed religion, such as christianity, if the teachers of it were honest men, and had opportunity to know the truth of the things they deliver ; tho' they were of some mean profession.

And, indeed, such a person as our virtuoso, will, with great willingness, exercise himself in perusing, with attention, and much regard, the writings of the apostles, evangelists, and ancient prophets ; notwithstanding any meanness of their first condition, or of their secular employments. And here, he will not only readily suffer himself to be instructed in the grand and general articles of religion, which, because of their necessity, or very great usefulness, are to be met with in many places, and in variety of expressions, by honest and duly-disposed readers ; but he will, instead of disdain, such tutors, both expect, and carefully strive, to improve his knowledge of divine things in general, even by those hints, and incidental passages, that a careless or ordinary reader would overlook, or not expect any thing from. For, as the fertility of the scriptures is not usually enough discern'd by vulgar readers, when the sacred writers transiently touch upon a great many subjects, that they do not expressly handle ; so, the docility we have ascribed to our virtuoso, will make him repose a great deal of trust in the testimony of inspired persons, such as Christ and his apostles, about things of all sorts, whether usually taken notice of, or not, that relate to objects of a supernatural order ; especially if among these, God himself, and his purposes, be comprized : since several of those things are not knowable without revelation, and others are best known by it. And to be allow'd to ground a belief about such things, on the relations and other testimonies of those that were "eye-witnesses and ministers" of the things they speak of, will, by our virtuoso, be justly reputed such an advantage, in order to the knowledge of things divine ; as the consulting with navigators and travellers to *America*, is, to a person-curious to learn the state of that new world. For an ordinary sea-man or traveller, who had the opportunity, with *Columbus*, to sail along the several coasts of it, and pass up and down thorough the country, was able at his return to inform men of a hundred things, that they would never have learn'd by *Aristotle's* philosophy, or *Ptolemy's* geography ; and might not only acquaint them with many particulars, agreeable to the opinions which their receiv'd physics and cosmography suggested, but also rectify several erroneous presumptions and mistakes, which, till then, they thought very agreeable to the dictates of those sciences, and to reason. And as one, who had a candid and knowing friend, intimate with *Columbus*, might better rely on his informations about many particulars of the natural history of those parts, than on those of a hundred school-

school-philosophers, who knew only what they learned from *Aristotle*, *Pliny*, *Ælian*, and the like ancient naturalists; much more may we rely on the accounts given us of divine things, by the apostles, and constant attendants of him who lay in the "bosom of God his father," and commission'd them to declare to the world the "whole counsel of God," as far as 'twas necessary for man to know.

Fuller trials are allow'd, among ingenious men, to rectify the informations of the more imperfect ones; and therefore I shall add, that, tho' the innate notions and sentiments, which nature gives us of the attributes and mind of God, be highly to be prized; yet the informations that theological experience affords of those abstruse things, is far more excellent and complete.

I have dwelt the longer upon the miracles that may be pleaded to recommend the christian religion, because I thought, that an argument grounded on them is little less than absolutely necessary, to prove that any religion men believe to be supernaturally reveal'd, really proceeds from God.

For, tho' the excellency of the christian doctrine, and other concurrent motives, may justly persuade me, that 'tis worthy and likely to be given by God; yet that, in fact, this doctrine comes from him by way of supernatural revelation, I can scarce be sufficiently ascertain'd, but by the miracles wrought by Christ and his disciples, to evince, that the doctrine they preach'd, as commission'd by God to do so, was indeed his; being, as such, own'd by him. But these miracles having been wrought in the first ages of the church; we can have no knowledge of them by our own senses, or immediate observation, but must take them upon the credit of historical experience, which is afforded us by the duly transmitted testimony of those, who were themselves eye-witnesses of the things they relate. And since we scruple not to believe such strange prodigies, as celestial comets, vanishing and re-appearing stars, islands founded by subterranean fires in the sea, the darkness of the sun for many months together, earthquakes reaching above a thousand miles in length, and the like amazing irregularities of nature, upon the credit of human histories; I see not, why that historical experience should not more be trusted, which has many peculiar and concurrent circumstances to confirm it; and particularly the death that most of the first promulgators cheerfully suffer'd to attest the truth of it; and the success and spreading of the doctrine authorized by those miracles, and receiv'd chiefly upon their account. To which things, some perhaps would add, that 'tis less incredible, that the author of nature should, for most weighty purposes, make stupendous alterations of the course of nature; than that nature herself, for no such end, should, by such prodigies, as were lately mention'd, throw herself, as it were, out of her own course.

Miracles being so necessary to the establishment of reveal'd religion in general, it may be look'd upon as a farther disposition in our virtuoso to receive the christian religion, that the philosophy he cultivates, much conduces to enable him to judge aright of those strange things, that are by many proposed as miracles, and believed to be so.

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For, first, the knowledge he has of the various, and wonderful operations of some natural things; especially when they are skilfully improv'd, and dextrously apply'd by art, particularly, in mathematics, mechanics, and chymistry, will qualify him to distinguish between things that are only strange and surprizing, and those that are truly miraculous: so that he will not mistake the effects of natural magic for those of a divine power. And, by this means, he will be able to discover the subtle cheats, and collusions of impostors; whereby not only the multitude of all religions, especially the heathen, but even learned men of most others, for want of an insight into real philosophy, have formerly been, or are at this day, deluded, and drawn into idolatrous, superstitious, or otherwise erroneous tenets, or practices.

On the other side, the knowledge our virtuoso may have of what cannot be justly expected, or pretended, from the mechanical powers of matter, will enable him to discern, that many things are not producible by them, without the intervention of an intelligent superior power; on which account, he will frankly acknowledge, and heartily believe several effects to be truly miraculous, that may be plausibly enough ascribed to other causes in the vulgar philosophy; where men are taught to attribute stupendous and unaccountable effects to sympathy, antipathy, *Fuga vacui*, substantial forms, and, especially, to a certain Being, presumed to be almost infinitely potent and wise, which they call nature: for this is represented as a kind of goddess, whose power may be little less than boundless: thus, I remember, *Galen* himself compares it to that of God; and saith, that he could not do such a thing, because nature could not; and censures *Moses* for speaking as if he were of another mind.

I know it may be objected, that those who are so well acquainted with the mysteries of nature, and her various, and strange ways of working, as our virtuoso is supposed to be, may, by that knowledge, be strongly tempted to think, that those surprizing things, other men call miracles, are but effects of her power; the extent of which is not easily discern'd by ordinary men, nor safely defined by philosophers themselves. To this I shall only here reply, that to make it reasonable to judge a particular performance supernatural, it is not at all necessary that it surpass the whole power of nature, that is, of physical agents; provided it surpass the power of that cause, or that complex of causes, from which the effect must in reason, if it be purely natural, or physical, be supposed to have proceeded. For instance, that a fisherman or two should speak other languages than their own, does not at all exceed the power of nature, if they employ'd a competent time in learning them: but, that a great number of fishermen, and other illiterate persons, should, all on a sudden, become linguists, and, in an hour's time, be able to speak intelligibly to a great number and variety of nations, in their respective languages, as the new testament relates, that the apostles, and their companions did on the day of pentecost; this gift of tongues, I say, was an ability, which, in those

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circumstances of place, time, and persons, wherein 'twas exercised, may justly be concluded to have been supernatural, or miraculous.

To proceed, 'tis a notorious matter of fact, that, in less than half an age, the christian religion was spread over a great part of the then known world; so that, in a few years after it began to be preach'd, the apostle of the *Gentiles* could tell the *Romans*, with joy, "that their faith was spoken of throughout the whole world." And, in the second century, *Tertullian*, and other famous writers, shew, that the gospel had already numerous proselytes in a great number of different kingdoms, and provinces. This wonderful quick progress of the christian religion, being ascertain'd to our virtuoso, from a thing whereby he is so much sway'd, as experience; it greatly disposes him to believe the truth of so prevalent a religion. For, if he considers the persons who first promulgated it; they were but half a score of illiterate fishermen, a few tent-makers, and other tradesmen. If he considers the means employ'd to propagate this doctrine, he finds, that they had neither arms, nor external power, to compel men to receive it; nor riches, honours, or preferments, to bribe or allure them to it; nor were they men of philosophical subtilty, to intrap, or entangle the minds of their auditors. Nor did they make use of the pompous ornaments of rhetoric, and glosses of oratory, to inveigle, or entice men; but treated of the most sublime and abstruse matters, in a most plain and unaffected style; as became lovers and teachers of truth. If he considers the nature of the doctrine, that, in a little time, obtain'd so many proselytes, he will find, that, instead of being suited to the natural apprehensions, or the receiv'd opinions of men; and, instead of gratifying their corrupt affections, or complying with so much as their most innocent interests; it prescribed such mortifications, and such great strictness of life, and high degrees of virtue, as no legislator had ever dar'd to impose upon his subjects, nor any philosopher on his disciples. And this doctrine was proposed in such a way, and was accompany'd with predictions of such hardships and persecutions, that should, in those times, be the portion of its sincere professors; as if the law-giver had design'd rather to fright men from his doctrine, than allure them to it: since they could not believe what he said, and foretold, to be true, without believing, that they should be made great sufferers by that belief. If our virtuoso considers the opposition made to the progress of the gospel, he will find cause to wonder, that it could ever be surmounted. For the heathens, which made by far the greatest part of the world, were deeply engaged in polytheism, idolatry, magical rites, and superstitions, and almost all kind of crimes; and some of these were shameless debaucheries, which oftentimes made a part of their worship. And the *Jews*, by the corrupt leaven of the pharisees, the impious errors of the sadduces, and the general mistakes of the nation about the person, office, and kingdom of the *Messias*; and by their dotage upon their vain traditions, and numerous superstitions, grounded upon them, were, on these and other accounts, highly indisposed, as well as the *Romans*, the two nations to be converted, to be made proselytes:

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One circumstance there is of the propagation of the gospel, which, tho' it may seem more extrinsic than those hitherto mention'd, is yet too considerable to be here omitted; I mean, that the quick spreading, and success of the christian doctrine in the world, was foretold both by the prophets of the old testament, and the author and promulgators of the new. For, it being notorious, that many errors and superstitions, have, with too much celerity, been spread far and wide in the world; either by reputed mere accidents, that were very favourable to them, or by the industry and artifices of men; it ought to be no small satisfaction to equitable judges, that the sudden progress, and notable effects of the christian religion, were foretold by the ancient prophets, and by the Messias and his apostles. For, by these accomplish'd predictions, it may appear, that the wonderful success of the gospel was not an effect of chance, but was, long before, determin'd by divine providence, to be accomplish'd in a wonderful manner, by his peculiar assistance.

But true prophecies of unlikely events, fulfill'd by unlikely means, are supernatural things; and, as such, may properly be reckon'd among miracles. We might add, that these have a peculiar advantage above most other

other miracles, on the account of their duration : since the manifest proofs of the predictions continue still, and are as visible as the extent of the christian religion ; and some of them are still more and more accomplish'd, by the conversions made of multitudes of infidels, in several vast regions of *America*. So that if we may call some transient miracles, such as the turning water into wine, at a marriage-feast in *Galilee* ; and the darkening of the sun when the moon was in the full, at the crucifixion of Christ ; accomplish'd predictions may be styled permanent ones ; and their difference may appear by the differing states of the *Mosaic* manna : for, tho both that which fell daily (except on the sabbath) in the wilderness, and that which was laid up in a pot before the testimony, were supernatural productions ; yet, tho' a portion of the former out-last'd not two or three days ; that kept in the pot, was preserv'd for many ages, and continu'd to be (as it was foretold it should) a visible miracle.

Besides, the preachers of the christian religion, both pretended and appeal'd to miracles, as proofs of the truth of their doctrine : and if we consider the great disadvantages they lay under, and the powerful opposition of all sorts, that they met with and surmounted ; it cannot reasonably be thought, that such unlikely men should so successfully preach so uninviting a doctrine, unless it were confirm'd by conspicuous miracles. Or, at least, if so uneasy and persecuted a religion, was propagated without miracles, that propagation itself may justly pass for a miracle ; and be no less fit than another, to confirm the religion so admirably propagated.

Thus, I think, I have shewn, that a virtuoso has some helps, which other men, generally speaking, want, to make him judiciously approve the arguments for the truth of the christian religion, grounded on the miracles wrought in its favour, and the wonderful success of it in the world. But, because a reveal'd religion, how true soever, can scarce be proved but by moral demonstrations ; and because it is not, therefore, always sufficient, that the arguments be good in their kind ; but there are some qualifications requir'd in the minds of those who are to be convinc'd by them ; I shall now add, that experimental philosophy also disposes the mind of its cultivator to receive due impressions from such proofs, as miracles, as well as other topics, afford the christian religion.

Another thing, then, that qualifies an experimental philosopher, for the reception of reveal'd religion, is, the habit of endeavouring to give clear explanations of the phenomena of nature, and to discover the weakness of those solutions which superficial wits usually make and acquiesce in, insensibly works in him a great and ingenuous modesty of mind. And, on account of this intellectual, as well as moral virtue, he will not only be very inclinable, both to desire and admit further information, about things which he perceives to be dark, or abstruse ; but he will be very unapt to take, for the adequate standard of truth, a thing so imperfectly inform'd, and narrowly limited, as his mere, or abstracted reason. And tho' a vulgar philosopher, who allows himself to refer the obscurest things in na-

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ture, to substantial forms, real qualities, sympathy, antipathy, &c. which need not, and, perhaps, cannot be clearly understood; and thence presumes that he understands every thing; and that those things must be false, or impossible, which agree not with his philosophy; yet a sober and experienc'd naturalist, who knows what difficulties remain unfurmounted, in the conception and explanations even of things corporeal, will not, by a lazy, or arrogant presumption, that his knowledge about things supernatural, is already sufficient, be induced to reject, or to neglect any information that may increase it.

And this frame of mind is a very happy one for a student in reveal'd religion; where cautiousness is not more necessary for the avoiding of errors, than docility is advantageous, in learning of truth: since the knowledge and goodness of the divine teacher, is such, that a scholar, to improve his intellect, needs but bring a mind fitted to receive the genuine informations, that are most liberally offer'd, and will never deceive him, who employs a due care not to mistake the meaning of them.

A well-cultivated conversation with the works of God, brings a man to discover, from time to time, so many things to be feasible, or true, which, whilst he argued but upon the shallow grounds of uninform'd reason, he judg'd false, or unpracticable; that, by degrees, he acquires a habit of receiving some sorts of opinions, and especially those that seem unfavourable to religion, with a disposition to reform or discard them, upon further information. And this, as he is resolv'd to submit to, in case he meets with it, so he is dispos'd to receive it, by having often found himself obliged, upon subsequent information, to mend, or lay aside his former opinions; tho' very agreeable to the best light he had to judge by, when he entertain'd them. Thus, tho' it seems a visible truth, that the disc of *Venus*, is, in all respects to the sun, totally luminous; yet, when the telescope discovers her to have her full and her wane, like the moon, he will believe this further observation, against the first, made with his naked eyes.

And, indeed, I have sometimes doubted, whether to be vers'd in mathematics, and other demonstrative parts of philosophy, brings a greater advantage to the mind, by accustoming and assisting it, strictly to examine things proposed for truths, and strongly to evince the truths a man knows, to others; than by fitting him to discern the force of a good argument, and submit willingly to truths clearly proved; how little soever he may have expected to find such conclusions true. 'Twill not be difficult to apply these reflections to our present purpose; since there are several passages in the scripture that sufficiently declare, both that multitudes persist in a criminal infidelity, out of a fond conceit of their own knowledge, and a readiness to be sway'd rather by strong prejudices, than by the strongest arguments that would remove them; and, that docility is a very happy disposition to the entertainment of reveal'd religion. And this qualification will be the more easily found in our virtuoso; because, tho' the things, about which he has long been sensible of his ignorance, or desires further instruction, are within

within the sphere of nature, and the jurisdiction of philosophy; many of the things which reveal'd religion declares, are so sublime and abstruse, that they may well be look'd upon as of an higher order than merely physical ones; and, therefore, cannot be satisfactorily reach'd by the mere light of nature. 'Tis true, our philosopher will examine more strictly, than ordinary men, the proofs brought for this, or that proposed revelation; but that is no disadvantage to a supernatural religion, such as the christian, if it be true: and the real truth about religion itself, does not require credulity, but only docility. If a piece of coin, that passes for true gold, be offer'd to an ordinary man, and to a skilful refiner, the latter, will, indeed, examine it more strictly, and not acquiesce in the stamp, the colour, the sound, and other obvious marks, that may satisfy a shop-keeper, or a merchant; yet, when he has try'd it by the severer ways of examining, such as the touch-stone, the cupel, *Aqua fortis*, &c. and finds it to hold good in those proofs, he will readily and frankly acknowledge, that 'tis true gold, and be more thoroughly convinc'd of it, than the other person; whose want of skill will make him still apt to retain a distrust, and render him, indeed, more easy to be persuaded, but more difficult to be fully satisfy'd. Thus, tho' our virtuoso will examine, with more strictness and skill, than ordinary men are able, miracles, prophecies, or other proofs, said to be supernatural, that are alledg'd to evince a reveal'd religion; yet, if the certain, and genuine characters of truth appear in them, he will be more thoroughly convinc'd of it, than a less skilful man, whose want of good criteria, and sound judgment, incline him to be diffident, and to be still afraid of having been imposed on.

I expect, it will be here objected, that I degrade the human understanding, by ascribing so much to experience, natural or supernatural, that it has left nothing for reason to do, but servilely to obey.

This objection, indeed, is plausible; yet the answer to it will not be very difficult, if the matter itself be duly consider'd, and reason be brought to act, not as an interested party, but an unbiass'd judge.

For we have already shewn, that rational philosophers scruple not to alter, or renounce, the opinions which specious reasons had suggested to them, when once they either find those opinions contradicted by experience, or meet with other opinions more conformable to it. And *Aristotle* himself, tho' he be accused of having wrested physics to logical, and metaphysical fancies, confesses, not only that in the science of nature, reason ought to comport with the phenomena, and the phenomena with reason; but that to adhere to plausible reasonings, and neglect sensible observations, is a weakness or disease of the mind. And whether the understanding be, as *Aristotle* taught, like blank paper; and receives no knowledge, but what has been convey'd to it through the senses; or whether the notions are congenite with the understanding, or so easily, and early, acquired by it, as to appear innate; they are but very few, in comparison of those that are requisite to judge aright about any one thing, that occurs either

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in natural philosophy, or theology. For, in the divine nature, power, wisdom, and other attributes, there is an exuberance that has produced numberless contrivances, laws, and other things, which exceedingly surpass both the number and variety that the dim and limited intellect of man could reach to, by framing, and compounding ideas, without the assistance of the patterns afforded by the works and declarations of God.

On account of the same prerogative of the divine knowledge, it must frequently happen, that the notions and opinions men take up of the works and mind of God, upon the mere suggestions of abstract reason; will not only be almost constantly very deficient, but often very erroneous. Of this we see evident proofs in many opinions of the old philosophers; who, tho' men of strong natural parts, were mis-led, by what they mistook for reason, to maintain such things about the works, and the author of nature, as we, who, by the favour of experience and revelation, stand in a much clearer light, know to be false, and often justly think utterly extravagant.

Abstract reason is a narrow thing, and reaches but to a very small share of the knowable truths, whether human or divine; which may be obtain'd by the help of further experience, and supernatural revelation. This reason, furnish'd with no other notices than it can supply itself with, is so shallow and deceitful, that he who seeks for knowledge only within himself, shall be sure to be quite ignorant of far the greatest part of things; and will scarce escape being mistaken about a large part of those he thinks he knows.

I am far from intending to deny reason any of its just prerogatives; experience itself is but an assistant to reason, and supplies informations to the understanding; which still remains the judge, and has the power, or right, to examine, and make use of the testimonies that are presented to it. The outward senses are but the instruments of the soul, which hears by the intervention of the ear, and in respect of which, the eye itself is but a more immediate optical tube; and the sense does but perceive objects, not judge of them. Nor do the more wary among the philosophers trust their eye, to teach them the nature of the visible object; but only employ it to perceive the phenomena it exhibits, and the changes that happen to itself by the action of it. 'Tis confess'd too, that the senses may deceive us, if the requisites of sensation be wanting; as, when a square tower appears round at a distance, and a strait stick, half in the water, appears crooked: 'tis, therefore, the part of reason, not sense, to judge whether none of the requisites of sensation be wanting; which, by the way, often requires, not only reason, but philosophy: and then, also, 'tis the part of reason to judge what conclusions may, and what cannot, be safely ground'd on the informations of the senses, and the testimony of experience. So that, when we say, experience corrects reason, 'tis an improper way of speaking; since 'tis reason itself, that, upon the information of experience, corrects the judgment it had made before.

To illustrate the use of reason, let us suppose an able judge coming to hear, and decide causes, in a strange country: 'tis plain, that the general notions he brings with him, and the dictates of justice and equity, can give him but a very short and imperfect knowledge of many things, that are requisite to frame a right judgment about the cases which will first be brought before him; and, till he has heard the witnesses, he may be very apt to fall into prejudiced opinions of things: but, when an authentic and sufficient testimony has clear'd matters to him, he then pronounces, according to the light of reason he is master of; to which the witnesses did but give information; tho' that subsequent information may have obliged him to lay aside some prejudices he had entertain'd before he receiv'd it. And, what is said of natural experience, with regard to the understanding, may, with due alteration, be apply'd to supernatural revelation: for here, also, the understanding is to examine, whether the testimony be indeed divine; and whether a divine testimony ought to be believ'd, in what it clearly teaches. I do not, therefore, degrade reason from the dignity that belongs to it, of perceiving, and judging; tho' it be obliged, by its own dictates, to take in all the assistance it can from experience, whether natural, or supernatural; and by the fuller accounts of things it receives from those informations, to rectify, if need be, its former less mature judgment.

In short, those who ery up abstract reason, as if it were self-sufficient, exalt it in words; but we who address reason to physical and theological experience, and direct it how to consult both, and take its informations from thence, exalt it in effect: and reason is much less usefully serv'd by the former sort of men, than by the latter; since, whilst those do but flatter it, these take the right way to improve it.



THE High Veneration Man's INTELLECT owes to G O D.

TIS with indignation and wonder, that I hear many men, and some of them divines too, who, little considering what God is, and what themselves are, talk of him and his attributes, as freely, and as unpremeditatedly, as they would of a geometrical figure, or a mechanical engine. And, even the less presumptuous frequently discourse, as if the nature and perfections of that unparallel'd Being, were objects their intellects could grasp; and scruple not to dogmatize about those abstruse subjects, as freely as about other things, that are confessedly within the reach of human reason.

Yet God may have several attributes, and perfections, utterly unknown to us; and of those attributes whereof we have some knowledge, there are effects and properties, whose sublimity, or abstruseness, surpassing our comprehension, makes the divine cause, or author of them, deserve our highest wonder and veneration.

As there are two chief ways to arrive at the knowledge of God's attributes; the contemplation of his works, and the study of his word; it may be doubted whether either, or both of these, will suffice to acquaint us with all his perfections.

For tho' philosophers have rationally deduced the power, wisdom, and goodness of God, from those marks of them which he hath stamp'd upon many of his visible works; yet, since the divine attributes which the creatures point at, are those whereof themselves have some participation, or resemblance; and, since the fertility of the divine nature is such, that its

excellencies may be participated, or represented many ways; how can we be sure, but so perfect, and exuberant a Being, has excellencies not expressed in the visible world, or any of its known parts?

Some of those divine attributes we know, being relative to the creatures, could scarce be discover'd by such imperfect intellects as ours, but from considering some things actually done by God. Thus, supposing, before the foundations of the visible world were laid, the angels had no more knowledge than men; they could scarce imagine in God a power of creating matter, and of producing local motion in it: much less could they have known how the rational soul, and human body act upon one another. Whence it seems probable, that if God hath made other systems than that wherein we live (and who can assure us, he hath not?) he may have displayed in some of the creatures that compose them, several attributes that we have not discover'd by the help of those works of his wherewith we are acquainted.

I readily grant, that the revelations God hath vouchsafed us, have clearly taught us several things concerning their adorable author, which the mere light of nature either would not have shewn, or but very dimly discover'd. Yet the scripture itself informs us, that in this life "we know but in part, and see things but darkly;" and that we are so far from being able "to find out God to perfection," that even the ways of his providence are to us unsearchable. Hence God may have attributes unknown to us.

But it may be alledged, that, besides the two ways mentioned of attaining the knowledge of God's attributes; there may be a third preferable to both the others, by considering the idea of a Being supremely, or infinitely perfect; in which idea, all possible perfections may be said to be contained; so that no new one can be added to it. And, indeed, I readily grant, that this idea is the most genuine I am able to frame of the Deity; yet there may be several attributes, which, tho' in a general way contain'd in this idea, are not, in particular, discovered to us thereby. 'Tis true, that when, by whatever means, any divine perfection comes to our knowledge, we may well conclude, 'tis, in a sense, comprized in the comprehensive notion we have of a Being absolutely perfect; tho', 'tis possible, that perfection would never have come to our knowledge by the bare contemplation of that general idea, but was suggested by particulars; whence such discoveries are not so much derived from, as refer'd to the notion we are speaking of.

These considerations persuade me, that God may have, not only various attributes, but various excellencies and perfections, unknown to us. And, farther, many of the attributes we know he hath, we have but an imperfect knowledge of, especially if compared with his; for he possesses them in a manner peculiar to himself, and far transcending that wherein men possess some faint resemblances of them.

The power and wisdom of God are display'd both in his corporeal, and his incorporeal creatures.

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Effects of the
divine power.

The vast mag-
nitude of the
whole material
world.

Among the manifold effects of the divine power, I shall mention only two; which, tho' often very manifest, are not very attentively reflected on; the immense quantity of corporeal substance that the divine power provided for the framing of the universe; and the great force of the local motion imparted to it, and regulated in it.

The vastness of that huge mass of matter, whereof the corporeal world consists, cannot but appear stupendous to those who skillfully consider it. That part of the universe, which has been already discover'd by human eyes, assisted with dioptrical glasses, is almost inconceivably vast. The fix'd stars of the first magnitude, that to vulgar eyes look but like shining spangles, are by astronomers affirm'd to exceed, each of them, above a hundred times the whole globe of the earth in magnitude: and as little as these stars seem to our naked eyes, they appear much less thro' our telescopes; which taking off the false glittering that makes them look as they are painted, shew them little other than physical points of light. And the sun, which is granted to be some millions of miles nearer to us than the other fix'd stars; tho' it seem at this less distance not to be half a foot broad; is by the generality of mathematicians allow'd to be eight or ten thousand times as big as the terraqueous globe. And it plainly appears by the parallaxes and other proofs, that the globe of earth and water we inhabit, tho' it be divided into so many great empires, and kingdoms, and seas; and tho', according to the received opinion, it be 5400 German leagues in circumference, and consequently its solid content 10,882,080,000 cubic miles; yet this globe is so far from being, for its bulk, a considerable part of the universe, that 'tis in comparison thereof but a physical point. Nay, those far greater globes, the sun and fix'd stars, and all the solid masses of the world, if reduced into one, would, perhaps, bear a less proportion to the fluid part of the universe, than a nut to the ocean. And as an excellent modern astronomer observes, all the stars, crowded into one body, would, at a fit distance, appear no bigger than a star of the first magnitude. After all, I have hitherto spoke but of that part of the corporeal universe already seen by us: I must therefore add, that as vast as this is, yet all that the eye, even when powerfully assisted by telescopes, hath discovered, is far from representing the world of so great an extent, as more perfect glasses will do hereafter. And even then the visible part of the world will be far from reaching to the bounds of the universe*.

From

* "The system of the world is now
says Dr. Halley "understood to occu-
py the whole abyss of space, and con-
sequently to be actually infinite; and
smaller fix'd stars being still discover-
able, as more perfect telescopes are
employ'd, seems to confirm this doc-
trine. Were the whole system finite,
it would, tho' ever so extended, still

"occupy no part of the Infinitum of
space, which necessarily and evident-
ly exists; whence the whole would
be surrounded with an infinite *vacuum*;
and the superficial stars would gravi-
tate towards those near the centre,
and with an accelerated motion run
into them, and in process of time u-
nite into one. But if the whole be
"infi-

The prodigious quantity of motion given thereto, and maintain'd therein.

From the vast extent of the universe, I proceed to consider the stupendous quantity of motion, that the divine power has given the parts of it, and continually maintains in it. Of this we may make an estimate, by considering with what velocity some of the greater bodies are moved, and how large a part of the remaining bodies of the universe, is also endow'd with motion.

The least velocity I shall mention, is that afforded by the *Copernican* hypothesis; according to which, the earth revolves from west to east, about its own axis, in four and twenty hours; yet this terraqueous globe, which we think so great, that we commonly call it the world, moves at such a rate, that, as the learned *Gassendus* computes, a point situated in the equator of it, moves about twelve hundred feet in a second minute: so that a bullet shot out of a canon, scarce flies so swift.

But this velocity is scarce comparable to that of the fix'd stars; if we suppose them to move, in four and twenty hours, about the earth. For supposing the distance assign'd by *Tycho* between us and the firmament to be seven thousand diameters of the earth; a fix'd star in the equator, moves, as *Mullerius* calculates, 3,153,333 miles in an hour, and consequently 52,555 in a minute, and 875 in a second; that is, three thousand times faster than a canon-bullet shot into the air. 'Tis true, that according to the *Ptolemaic* hypothesis, a fix'd star in the equator moves, at most, but three semi-diameters of the earth in a second; but, according to *Ricciolus*, this velocity is fifty times greater than in the *Ptolemaic* hypothesis; and threescore and ten times greater than in the *Tychonian*. For, according to him, such a fix'd star as we speak of, moves 629,128 *English* miles in a second.

But farther, that portion of the universe, which, tho' put into motion, is commonly supposed to be at rest, is so great, that, perhaps, the quantity of motion distributed among seemingly quiescent bodies, may exceed the quantity of motion which the first mover has communicated to the fix'd stars; tho' we suppose them whirl'd about the earth with that stupendous swiftness, the

" infinite, all the parts of it would be
" nearly in *equilibrio*; whence each fix'd
" star, being attracted by contrary powers,
" would keep its place, or move till from
" such an equilibrium it comes to rest.
" 'Tis no more absurd, that the fix'd stars
" should exceed any given number, than
" that duration should be eternal, because
" no number of days or years can complete it.
" 'Tis said, I know, that if the
" number of the fix'd stars were more
" than finite, the whole surface of their
" apparent sphere would be luminous.
" But if we suppose all the fix'd stars to
" be as far from one another, as the nearest
" of them is from the sun; that is, if
" we may suppose the sun to be one of
" them; at a greater distance, their disks

" and light will be diminish'd, and the
" space to contain them increased, in the
" proportion of the squares: so that in each
" spherical surface the number of stars it
" might contain, will be as the biquadrate
" of their distances. If then, as it needs
" must, the distances be immensely great,
" it will follow, that as the light of the
" fix'd stars diminishes, the intervals between
" them decrease in a less proportion; the one being as the distances,
" the other as the squares thereof, reciprocally. And the more remote stars
" are so small, that they must vanish, even
" in the most exquisite telescopes; as a
" small telescopic star is invisible to the
" naked eye." *Philos. Transf.* N° 364. p. 22.

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systems of *Ptolemy* and *Tycho* suppose. For the fix'd stars, planets, or all the mundane globes, whether lucid or opaque, together bear but a small proportion to the interstellar part of the universe. And tho' I should allow all these globes to be solid, yet it must be confess'd, that each of them swims in a fluid of much greater extent than itself. So that the fluid portion of the universe will, in bulk, almost incomparably exceed the solid. And if we consider the nature of a fluid body, we shall find it consist in having its minute parts perpetually and variously moved; so that tho' the whole body of a liquor seems to be at rest, yet its component parts are in a perpetual motion; continually shifting places amongst themselves.

And because the quantity of motion shared by the corpuscles that compose fluids, is not usually reflected on by philosophers; I shall add, that we may guess how great and vehement a motion there may be in the parts of fluid bodies, perhaps, when the aggregates of those particles appear to be at rest, by observing them when their ordinary motions happen to be disturb'd, or to be extraordinarily excited by fit conjunctures of circumstances. Thus we see the strange force and effects of boisterous winds and whirl-winds, which are but streams and whirl-pools of the invisible air, whose singly insensible parts are by accidental causes determin'd to have their motion made either in a strait line, or, as it were, about a common centre. Thus when a mine of gun-powder is sprung, the flame, or some subtile ethereal substance, that is always at hand in the air, tho' both are fluids, and the powder perhaps be kindled but by one spark of fire, instantly exerts a most rapid and furious motion.

The velocity of these discharged flames may appear from that which gun-powder impresses on a bullet, shot out of a well-charg'd gun; and this *Mersennus*, after several trials made to measure it, defines to be about 450 feet in a second. If, then, we admit the probable opinion of the *Cartesians*, that the earth and planets are turn'd about their own axes by the motion of the respective ethereal vortices in which they swim; we shall easily grant, that the motion of the celestial matter, which moves, for instance, upon the remote confines of the earth's vortex, is by a vast excess more rapid than that of the surface of the earth. But if we chuse the *Tychonic* hypothesis, which makes the firmament, with all the vast globes of light that adorn it, to move about their common centre in four and twenty hours, the motions of the celestial matter must be allow'd a far greater, and indeed a scarce imaginable rapidity.

Hence we may have more enlarg'd conceptions of the power and wisdom of the great Creator, who has both put so wonderful a quantity of motion into the universal matter, maintains it therein, and is able, not only to set bounds to the raging sea, but so to curb and moderate those stupendously rapid motions of the mundane globes and intercurrent fluids, that neither the unwieldiness of their bulk, nor the celerity of their motions, have made them fly out, for many ages. And if the firmament itself, whose motion, in the vulgar hypothesis, is by much the most rapid in the world, fail of exactly completing its revolution in four and twenty hours; that retardation is

so

so regulated, that since *Hipparchus's* time, who lived above 2000 years ago, *Physics.* the first star in *Aries*, which was then near the beginning of it, is not yet come to the last degree of that sign.

The wisdom of God is express'd in two different manners. Sometimes it is so manifestly display'd in familiar objects, that even superficial spectators may take notice of it. But there are many other things, wherein the "treasures of wisdom and knowledge" may be said to lie deep. *The wisdom of God differently express'd.*

The contrivance of every animal, and especially of a human body, is so curious and exquisite, that 'tis almost impossible for any one, who has not seen a dissection well made, and anatomically consider'd, to conceive how much excellent workmanship is display'd in that admirable engine. Nay, the meanest living creatures of God's making, are far more wisely contrived, than the most excellent pieces of workmanship that human hands and hands can boast. No watch or clock is any way comparable, for exquisiteness of mechanism, to the body of an ass or a frog. *In the various contrivances of animal bodies.*

But God's wisdom is recommended, as well by the variety, and consequently the number of the kinds of living creatures, as by the fabric of each of them in particular. The skill of human artists is, for the most part, limited to one, or to a few sorts of contrivances; whilst the great author of nature has not only created four principal sorts of living engines, beasts, birds, fishes, and reptiles; which differ exceedingly from one another, as the several regions, or stages, where they were to act their parts, required they should; but under each of these comprehensive genus's are compriz'd many subordinate species of animals, which differ exceedingly from others of the same kind, according to the exigency of their particular natures; as the hog and the hare, the parrot and the batt, the whale and the star-fish, &c.

And what greatly enhances the excellent contrivances to be met with in these automata, is the symmetry of all the various parts whereof each of them consists. For tho' an animal, consider'd in his state of intireness, is justly look'd upon as one engine; yet, really, this whole machine is a complex thing, made up of several parts, which, consider'd separately, may pass, each of them, for a subordinate engine, excellently fitted for some particular use. It ought, therefore, highly to recommend the wisdom of the great "former of all things," that he has so framed each particular part of animals, as not to let the skill bestow'd on that, render the other less perfect: which manifests, that this great artist had the whole fabric under his eye at once; and did, at one view, behold all that was best to be done, in order to compleat the whole animal, and all its parts, at once: whilst many excellent artificers, who can make a single engine very compleat, may not be able to make it a commodious part of an aggregate of engines. Thus, tho' several can make pendulum-clocks go very regularly on shore, they cannot yet fit them to measure time, exactly, in a ship, at sea.

But how much more wonderful than the structure of the grown body, must be the contrivance of the *Semen animatum*; since all the future parts, the functions, and many actions of the animal to be produced, must be durably

The Veneration due to God.

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durably delineated, and couch'd in a little portion of matter, that seems homogeneous, and is, unquestionably, fluid? And what much increases the wonder, is, that one of these latent impressions, or powers, namely, the prolific, is to lie dormant, perhaps, above thirty or forty years, and then to be able to produce many more such engines, as is the animal itself.

In the mutual usefulness of his productions to each other.

Another way whereby God manifests his wisdom in his corporeal creatures, is their mutual usefulness to one another, in a relation either of dependence, or co-ordination. This serviceableness may be consider'd, either as the parts of the animal have relation to one another, and to the whole body they make up; or, as intire, and distinct bodies, which have a regard to, or dependence on each other. To the first sort of utility belong the offices of the parts of the body, which some of them exercise for the good of the whole; as, the stomach for concocting the aliment; the brain for supplying animal spirits; the kidneys to separate the superfluous serum of the blood. There are, also, many other particular parts that have a great subserviency to one another. And several consents of parts, and utilities, that accrue from one to the other, are farther discovered by diseases; which, primarily affecting one part, or member of the body, discover that another has a dependance on it, or a particular relation to it. To the second kind of utility belong those parts that discriminate the sexes of animals; which have such a relation one to another in the male, and the female, that 'tis obvious they were made in order to propagate the species.

It were endless to observe, the wisdom and goodness manifest in the works of creation; the situation of the sun, its motion in the ecliptic, trade-winds, stated rains, &c. with regard to the welfare of men, and other animals. Thus, too, we see, that, according to the usual course of nature, lambs, kids, &c. are brought into the world at the spring of the year, when tender grass, and other nutritive plants, are provided for their food. The like may be observed in the production of silk-worms, whose eggs are hatch'd when mulberry-trees begin to bud; whereon these insects are to feed; the aliment being tender whilst the worms themselves are so; and growing more strong and substantial, as the insects increase in vigour and bulk.

And in forming and governing other systems besides the solar.

I have hitherto taken notice only of the productions of God's power and wisdom observed in the visible world. But, perhaps, as the sun, the nearest fixed star to us, has a whole system of planets that move about him; so some of the other fixed stars may be centres of other systems of celestial globes: since we see, that even some planets, determined by astronomers to be much inferior in bigness to the fixed stars, have other globes, that do, as it were, depend on them, and move about them; the earth has the moon for its attendant; *Saturn* is not unattended; and *Jupiter* has no less than four satellites. But none of these secondary planets, that move about *Saturn* and *Jupiter*, are visible to the naked eye; and were, therefore, unknown to the astronomers who liv'd before the invention of telescopes. Now, in case there are other mundane systems, besides this visible one of ours; I think it may be, probably, supposed, that

God

God has given peculiar, and admirable instances of his inexhausted wisdom in the contrivance and government of systems, that, for ought we know, may be framed and managed in a manner quite different from what is observed in that part of the universe known to us.

However, we need not fly to imaginary ultra-mundane spaces, to be convinced, that the effects of the power and wisdom of God are worthy of their causes, and not adequately understood by us; if, with sufficient attention, we consider that innumerable multitude, and unspeakable variety of bodies, that make up this vast universe. For there being among these, a stupendous number, that may be justly look'd upon as so many distinct engines, and many of them very complex; to know that all these, with the rest of the mundane matter, are every moment sustain'd, guided, and govern'd, according to their respective natures, and with an exact regard to the general laws of the universe; to know that there is a Being, who, every where, manages all things, without either aberration, or intermission; is a thing, that if we attentively reflect on, ought to produce in us great wonder, and adoration.

The *Epicureans* of old did, with some colour of reason, urge against the belief of a divine providence, that 'tis incredible the gods should be sufficient for such differing and distracting employments, as, according to the exigencies of nature, to make the sun to shine in one place; the rain to shower down in another; the winds to blow in a third; the lightning to flash in a fourth; the thunder-bolts to fall in a fifth; and other bodies to act and suffer, according to their respective natures. We, therefore, who, upon good grounds, believe God really does what they thought impossible, are much wanting in our duty, if we do not admire an all-pervading wisdom, that reaches to the utmost extent of the universe; and, with ease, actually performs what these philosophers profess'd they could not so much as conceive.

We have seen God's wisdom and power in his corporeal works; but some of the divine perfections could not be so well express'd, or copied, upon corporeal creatures, as upon the rational and immaterial soul of man, and other intellectual Beings: as the picture of a plain simple thing is not capable of receiving, or containing, so much of an excellent painter's skill, as he could exhibit in a piece wherein the passions of the mind, and the laws of optics, &c. may be fully express'd. And it may well be presumed, that if we were as familiarly acquainted with God's incorporeal creatures, as with his visible ones; we should perceive, that as spirits are incomparably more noble than bodies; so the divine wisdom employ'd in the government and conduct of them, is more glorious than that which we justly admire in the conduct of his corporeal works. And, indeed, let a portion of matter be ever so fine, and ever so well contrived, it will not be more than an engine destitute of understanding and will; and whose excellency, as well as its distinction from other bodies, even the grossest, and most imperfect, can consist but in mechanical properties; which neither excite themselves into motion, nor regulate and stop the motion

Still greater instances of power and wisdom, in the formation and government of immaterial Beings.

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tion they once are in: whereas, true spirits, or immaterial substances, have, by God's appointment, belonging to their nature, understanding, will, and an internal principle, both of acting, and of arbitrarily ceasing from action. And tho' God, as the sole creator of all substances, has, and may exercise, an absolute dominion over all his creatures, as well immaterial as corporeal; yet, since he thinks fit to govern spirits according to the nature he has given them; to create such intelligent, free, and powerful Beings, as good and bad angels; and to govern them on such terms, as effectually to make them instruments of his glory, which multitudes of them subtly and obstinately oppose; requires a wisdom and providence transcending any that can be display'd in the formation and management of merely corporeal Beings. For inanimate engines may be contrived, to act as we please; whilst angels, and human souls, are endow'd with a freedom of acting, in most cases, as they please themselves. 'Tis far easier for a watch-maker to regulate the motions of a watch, than the affections and actions of his son.

Angels, whether good or bad, are very intelligent and active Beings; and each of them is endow'd with an intellect capable of numberless notions, and degrees, or variations of knowledge; with a will capable of no less numerous acts; of having various influences upon the understanding, and of being variously affected by the dictates of it. So that each particular angel, being successively capable of so many differing moral states, may be look'd upon, as, in a manner, a distinct species of the intellectual kind. And, the government of one demon, may be as difficult a work, and, consequently, may as much declare the wisdom and power of God, as the government of a whole species of inanimate bodies, whose nature determines them to a strict conformity to those primordial laws of motion, once settled by the great creator; and, from which, they have no wills of their own to make them swerve.

The scripture tells us, that, in the œconomy of man's salvation, there is so much of the "manifold wisdom of God" express'd, that the angels themselves desire to pry into the mysteries of it. When our saviour, having told his apostles, that the day and hour of his future coming was not then known to any, subjoins, no not to the angels of heaven, he sufficiently intimates them to be endow'd with excellent knowledge, superior to that of men: which, perhaps, may be one of the reasons why the scripture styles them "angels of light." It also teaches us, that the good angels are vastly numerous; and that, as they are of differing orders, God assigns them very differing, and important employments, both in heaven, and on earth; and, sometimes, such as oblige them, in discharge of their respective trusts, to endeavour the carrying on of interfering designs. The same scripture, by speaking of the devil, and his angels, and of the "great dragon, that drew down with his tail the third part of the stars from heaven to earth;" and, by mentioning a whole legion of devils that possessed a single man, &c. gives us ground to conclude, that there is a political government in the kingdom of darkness; that the monarch of it is exceeding powerful, whence

whence he is styled the prince of this world ; and some of his officers have the titles of principalities, powers, rulers of the darkness of this world, &c. that the subjects of it are exceeding numerous ; that they are desperate enemies to God and men ; that they are very false and crafty ; and that their malice is active, restless, and great. These things being taught us in the scripture itself, we may rationally suppose, that if we were quick-sighted enough to discern the methods of the divine wisdom in the government of the angelical, and of the diabolical worlds ; we should be ravish'd into admiration how such intelligent, free, powerful, and immortal agents, should, without violence offer'd to their natures, be made, in various manners, to conspire to fulfil the laws, or, at least, accomplish the ends of that great theocracy, which not only reaches to all kinds of bodies, but comprizes the whole creation, or the great aggregate of all the creatures of God. And, indeed, to make the voluntary, and, perhaps, the most crafty actions of evil men, and of evil spirits, subservient to his wise, and just ends, no less recommends the wisdom of God, than it would the skill of a pilot, to contrive and steer a ship, so as to fail to the designed port, not only with a side-wind, but with one that was quite contrary, and tempestuous.

At that great decretory day, when the whole off-spring of *Adam* shall, by the loud voice and trumpet of the arch-angel, be call'd together, from the remotest ages and most distant climates in the world ; when the fal'n angels, and all the human actors that ever lived, shall appear upon the stage at once ; "when the dead shall be raised, and the books shall be open'd ;" then the wisdom of God will shine in its meridian lustre, and full splendor ; and not only the occurrences which relate to the lives and actions of particular persons, or of private families, and other less societies of men, will be there found not to have been overlook'd by the divine providence ; but the fates of kingdoms and commonwealths, and the revolutions of nations and of empires, will appear to have been order'd and over-ruled by an incomparable wisdom. And those great politicians, who thought to out-wit providence, by their refined subtilties, shall find themselves "taken in their own craftiness ;" shall have their deepest "counsels turn'd into foolishness ;" and not be able to keep the amazed world from discovering, that whilst they thought they most craftily pursu'd their own ends, they really accomplish'd those of God. And the subtle hypocrites, who thought to make pretended religion the instrument of their secular designs, shall find those designs defeated, and made truly subservient to that advancement of religion, which they, in reality, never aim'd at.

To employ, and keep in order, a very complicated engine, tho' all the parts of it be inanimate, and destitute of purposes and ends of their own, is justly counted a piece of skill. And this task is more difficult, and, consequently, recommends the conduct of the artist, in proportion to the intricacy of the structure, and the number of pieces whereof the engine consists. How astonishing, then, will appear that wisdom and providence, which is able to guide, and over-rule many thousand millions of engines, endow'd with wills, so as to make them all be found, in the final issue of

PHYSICS. things, subservient to purposes worthy of divine providence, holiness, justice, goodness?

In short, when all the actors shall appear at once upon the stage; when all disguises shall be stript off, all intrigues discovered, all hearts and designs laid open; then to find, that this whole amazing opera, that has been acting upon the face of the earth, from the beginning to the end of time, has been so contrived, and carried on, by the great author of the world, and of men, that their innumerable various actions, and cross-designs, are brought to conspire to the accomplishment of a plot worthy of God; will appear an effect of so vast, and so all-pervading a wisdom, that human intellects will with admiration confess, nothing but a divine omniscience could compass.

Great instances of wisdom in the redemption of man.

In the redemption of mankind, more of the divine attributes, than are commonly taken notice of, have their distinct agencies; and their co-operation is so admirably directed by the divine wisdom, that an apostle might very justly call it the "great mystery of godliness."

But many divines have largely treated of this subject; tho' I doubt whether most of them have not been more happy in avoiding errors about it, than successful in unveiling the mysteries couch'd in it. There are, in the great work of man's redemption, some characters and footsteps of the divine wisdom so conspicuous and refulgent, that a believer, of mean parts, may easily discern them. But there are also, in this sublime and comprehensive work, some "depths of God," and so much of "the wisdom of God in a mystery," that I cannot think it easy to have a mental eye, so enlighten'd, and so piercing, as to treat largely and worthily of so vast and abstruse a subject. And, indeed, a man must know much of the nature of spirits in general, and even of the father of them, God himself; of the intellect, will, &c. of the soul of man; of the state of *Adam* in paradise, and of the influence of his fall upon his posterity; of the natural, or arbitrary vindictive justice of God; of the grounds, and ends of God's inflicting punishments; of the admirable, and unparallel'd person of Christ; of those qualifications, and offices, that are required to fit him for being a redeemer; of the nature of covenants, and the conditions of those which God vouchsafed to make with man; of the divine decrees, with regard to man's final state; of the secret, and powerful operations of grace upon the mind, and the manner by which the Spirit of God works upon the souls of men, which he converts, and brings, by sanctification to glory; in short, there are so many points, most of them of difficult speculation, that are fit to be discuss'd by him, who would solidly, and fully treat of the world's redemption by Christ; that, when I reflect on them, I am ready to exclaim with *St. Paul*, "who is sufficient for these things?" And, I am so far from wondering, that the generality of divines, and other writers on this subject, have not fully display'd the wisdom which God has express'd in this great work, that to have been able to accomplish it in so admirable a way, as God has actually contrived, and made choice of; is one of the chief reasons of my admiration of the wisdom itself. And, I am persuaded, that

that for God to reconcile his inflexible justice, his exuberant mercy, and all those other attributes that seem'd to clash, inevitably, about the design'd salvation of men, and make them co-operate to it; is a stupendous manifestation of wisdom: there being no proposition in *Diophantus*, or *Apollonius*, in algebra, or in geometry, near so difficult to be solv'd, or that requires a greater number of proportions and congruities, to be at once attended to, and made subservient to the same ends; as that great problem, propounded by God's infinite goodness, to his divine wisdom; the redemption of lost and perverse mankind, upon the terms declared in the gospel: which are admirably fitted to promote, at once, God's glory, and man's felicity.

We have here, and that very imperfectly, only spoke to two of God's attributes, his wisdom, and his power: tho' there are many others where-
with we are acquainted; and, perhaps, still more whereof we are ignorant. Now, the natural and genuine result of all these divine perfections, must be a most glorious majesty, that requires the most lowly and prostrate veneration of all his intelligent works. And, accordingly, the angels, of all his mere creatures the most excellent and knowing, are represented in the scripture, as assiduously employing themselves in obeying and serving, in praising and adoring the divine majesty, with the utmost lowliness and submission.

The immense difference between the creator and his creatures.

This profound respect of the angels, is not to be wonder'd at; since, where esteem springs not from ignorance, but knowledge; the greater the ability and opportunities are of having the knowledge clear and heighten'd, the greater veneration must be produced in an intelligent Being for the object admired; whose perfections are here such as even an angelical intellect cannot fully reach: for, as a line, by being ever so much extended in length, cannot grow a surface; so neither can created perfections be, by any ideas, magnify'd into divine ones. And, indeed, speaking in general, the creatures are but shadowy, and arbitrary pictures of the great creator; of many of whose perfections, tho' they have some marks; yet they are such, as rather give the intellect rise and occasion to take notice of, and contemplate the divine originals, than afford it true images of them. The awful reverence paid to the supreme Being by those excellent spirits, who "are greater in power and might than we," ought to admonish us of the ecstatic respect we owe him; and teach us, that whenever we speak either to God, or of him, we ought to be inwardly affected with the unmeasurable distance there is between a most perfect and omnipotent creator, and a mere impotent creature.

The distance betwixt the infinite creator and the creatures, which are but the limited and arbitrary productions of his power and will, is so vast, that all the divine attributes, or perfections, by unmeasurable intervals transcend those faint resemblances of them, that he has been pleased to impress, either upon other creatures, or upon men. God's nature is so peculiar and excellent, that there are qualities, which, tho' high virtues in men, cannot belong to God, or be ascribed to him without derogation. Nay, there are some virtues that belong to man himself only in his mortal state.

The superiority of the divine knowledge to that of man.

PHYSICS. But, whatever excellencies there be, that are simply and absolutely such, and so may, without disparagement to his matchless nature, be ascribed to God, we may be sure that he possesses them; since he is the original author of all the degrees, or resemblances, we men have of any of them. The psalmist's reasoning is good. "He that planted the ear, shall he not hear? He that formed the eye, shall he not see? He that teacheth man knowledge, shall he not know?" Since all the perfections communicated to, or to be found in the creatures, being emanations of the divine excellencies, belong as much to God, as, in a bright day, all the luminous rays found in the air, belong to the sun. The vast difference, then, between the perfections of the great creator, and those that are analogous to them in the creatures, reaches to all the perfections to be found in both: but the human understanding, as it values itself upon nothing more than wisdom and knowledge; so there is nothing that it esteems, and reverences, more in other Beings, and is less willing to acknowledge itself surpass'd in, than these. Now, 'tis certain, that God knows innumerable things, with which we are altogether unacquainted: he cannot but know all the creatures he has made, whether visible or invisible, corporeal or immaterial; and what he has enabled them to do. Nay, since he cannot but know the extent of his own infinite power, he cannot but know numberless things, as possible, that he has not yet made, or, perhaps, ever will make. He, also, knows those things whereof we men have some knowledge, in a manner, or degree, peculiar to himself. As, what we know but in part, he knows fully; what we know but dimly, he knows clearly; and what we know but by fallible mediums, he knows most certainly.

But the great prerogative of God's knowledge, is, that he perfectly knows himself; that knowledge being not only too wonderful for man, but beyond the reach of an angelical intellect: since, fully to comprehend the infinite nature of God, no less than an infinite understanding is requisite. And for the works of God, even in those that are purely corporeal, our knowledge is incomparably inferior to his. For tho' some modern philosophers have made ingenious attempts to explain the nature of things corporeal; yet their explanations generally suppose the present fabric of the world, and the laws of motion settled in it. But God knows, particularly, both why, and how the universal matter was first contrived into this admirable universe, rather than a world of any other of the numberless constructions he could have given it; and both why those laws of motion, rather than others, were established; and how senseless matter, to whose nature motion does not at all belong, comes to be put into motion, and qualify'd to transfer it, according to determinate rules, which itself cannot understand. But when we come to consider the particular, and more elaborate works of nature; such as the seeds, or eggs, of living creatures, &c. the ingenuous confess, and the confident betray their ignorance. 'Tis likely, that we men know ourselves better than what is without us: yet how ignorant we are at home, if the endless disputes of *Aristotle*, and his commentators, about the human soul, and of physicians and anatomists,

anatomists, about the mechanism of the human body, were not sufficient to manifest, 'twere easy to shew, by the very conditions of the union of the soul and body: which, being settled, at first, by God's arbitrary institution; and having nothing in all nature parallel to them; the manner and terms of that strange union, is a riddle to philosophers; but must needs be clearly known to him who alone instituted it, and preserves it. There are several advantages of the divine knowledge, above that of man. For, we can perceive, and sufficiently attend but to few things at once; but God's knowledge reaches, at once, to all that he can know; his penetrating eyes pierce quite thorough the whole creation at one glance; and "there is no creature that is not manifest in his sight." He always sees incomparably more objects at one view, than the sun himself, endued with sight, could do. For God beholds, at once, all that every one of his creatures, in the vast universe, either does or thinks. The knowledge of God is, also, not a progressive, or discursive thing, like that acquir'd by our reasoning; but an intuitive knowledge. Men, by reason of the limitedness and imperfections of their understandings, are obliged to make the notice they have of one thing, a step and help to acquire that of another, less known: but God, whose knowledge, as well as his other attributes, is infinitely perfect, knows every thing in itself; and, all things being equally known to him, he can, by looking into himself, there see every thing that is knowable, most distinctly, yet all at once. But, further, God knows the most secret thoughts and intentions of men; whence he is call'd the "searcher of all hearts;" nay, he knows mens "thoughts afar off." And, by the way, how imperfectly must mere philosophers know God, since they know him but by his works; and know his works themselves but very imperfectly? Another conspicuous prerogative of the divine knowledge, is the prescience of future contingencies, that depend upon the determinations and actions of free agents. For we men are so far from being able to stretch our knowledge to the discovery of such events, that the greatest scholars in vain have try'd to discover how God himself can fore-know them; and, therefore, too many, even among christians, deny that he can; tho' by several accomplish'd predictions, recorded in scripture, it manifestly appears, that he does.

When I consider the transcendent excellency, and the numerous prerogatives of the Deity, I cannot without wonder and concern observe, that rational men, professing christianity, should wilfully neglect to acquire, or reflect on, those notices that are apt to increase their knowledge of God, and consequently their veneration for him. To aspire to a farther knowledge of God, that we may the better adore him, is a great part both of man's duty and his happiness. God, who has put into men an innate desire of knowledge; and a faculty to distinguish the degrees of excellency in different notices; and to relish those most, that best deserve it; and has made it his duty to search and inquire after God, and to love him above all things; would not have done this, if he had not known that those who make a right use of their faculties, must find him to be the noblest object

The obligations men are under to venerate, and contemplate God.

of

PHYSICS.

of the understanding; and that which most merits their wonder and veneration. And, indeed, what can be more suitable to a rational creature, than to employ reason to contemplate that divine Being, who is both the author of its reason, and the noblest object, about which it can possibly be employ'd? The knowledge of some dead language, or some old rusty medal, or the opinions and customs of some nations or sects, that did not, perhaps, reason or live any better than we do now, are thought worthy of curiosity, and even of the laborious industry of learned men; and the study of things merely corporeal, gains men the honourable title of philosophers. But whatever these objects of inquiry be, in themselves, 'tis certain the greatest discoveries we can make of them, are but trifles, in comparison of the excellency of the knowledge of God; which as much surpasses that of his works, as he himself does them. And 'tis the prerogative of his nature, to be infinitely above all that he has made; whether we contemplate the works of nature, or those of art; the former whereof are under another name, his more immediate works; and the others, the effects of one of his works; and, by consequence, originally his. And tho' it be most true, that God has been pleased to stamp on the corporeal world such impresses of his power, wisdom, and goodness, as have justly exacted the admiration even of philosophers; yet the great author of the world is, himself, incomparably superior to all his workmanship: so that, tho' he could have made, and always will be able to make, creatures more perfect than those he has made, by infinite degrees; yet the prerogative of his nature will keep him, necessarily, superior to the most excellent creatures he can make; since the very condition of a creature hinders it from being self-existent and independent. 'Tis therefore, methinks, a sad thing; that we men should grudge to spend now and then a few hours in the contemplation and internal worship of that most glorious and perfect Being, who continually employs the devotion of angels themselves.

I know 'tis supposed a dangerous thing, to be inquisitive about the nature of God. But the secret things of God, which are to be left to himself, seem to be his unrevealed purposes and decrees, and his most abstruse essence or substance; the scrutiny whereof, I readily acknowledge not to belong to us. And I think there is a great difference between contemplating God out of a bold curiosity, merely to know somewhat that is not common of him; and doing it out of an humble desire, by a farther knowledge, to heighten our reverence and devotion towards him. 'Tis an effect of arrogance to endeavour, or so much as hope, to comprehend the divine perfections, so as to leave nothing in them unknown; but to aspire to know them farther, that they may proportionably appear more admirable and lovely in our eyes, is not only an excusable, but a laudable curiosity. The scripture, in one place, exhorts us "to grow" not only "in grace," but "in the knowledge of Christ;" and in another, "to add to our virtue knowledge." And that we may aspire to great degrees of knowledge, even as to those supernatural objects we cannot adequately know, appears from *St. Paul*, who prays that his *Ephesians*, and all true christians, may be able

to

to comprehend what is "the breadth, and length, and depth, and height, and to know the love of Christ, which, says he, passeth knowledge." Supposing it then lawful to contemplate God, not with design to pry into his decrees and purposes, nor to dogmatize in controverted points about his nature and attributes; but to excite in ourselves the sentiments which his indisputable perfections are, by a more attentive view, qualified to produce; I take the devout contemplation of God, besides other great advantages that it brings the mind, to be one of the most delightful exercises the soul is capable of, on this side heaven. 'Tis generally acknowledg'd, that admiration is one of the most pleasing affections of the mind; which sometimes, when the object deserves it, is so possess'd thereby, as to forget all other things, or leave them unregarded. Now, the pleasure that admiration gives, being usually proportionate to the uncommon nature and endearing circumstances of the thing admired; how can any admiration afford such a contentment, as that which has God himself for its object? The wonder produced in us by an humble and attentive contemplation of God, has two principal advantages, above the admiration we have for any of his works, or of our own. For, first, when we admire corporeal things, how noble and precious soever, the contentment that accompanies our wonder, is allay'd by a kind of secret reproach, grounded on that very wonder; since it argues a great imperfection in our understandings, to be unacquainted with things that are but creatures, as well as we; and, what is worse, of a nature much inferior to ours: whilst 'tis no disparagement, for a human intellect to be possess'd with wonder, tho' heighten'd to amazement, or astonishment, by the contemplation of that most glorious and infinitely perfect Being, who must necessarily exceed the adequate comprehension of any created intellect. But there is a farther and much greater advantage of the admiration of God, above that of other things; for other objects having a bounded nature, and commonly but one thing to deserve our wonder, our admiration of them is seldom lasting; and after a little familiarity, first languishes, and then ceases: but God is an object so very singular, whose perfections are so immense, that no diligence of considering him, can make him cease to be admirable; and the more we know of him, the more reason we find to admire him. So that there may here be a perpetual vicissitude of our happy acquirements of farther degrees of knowledge, and our eager desires of new ones. God is so fertile an object, that we need not fear our admiration of him should expire, for want of variety to keep it up. To the wonderful excellence of God, may be justly applied what *Aristotle* lays down as a definition of *Infinite*; viz. "'tis that, of which how much soever one takes, there still remains more to be taken." If the intellect should for ever make a farther progress in the knowledge of the wonders of the divine nature, attributes, and dispensations; yet it may still make discoveries of fresh things worthy to be admired: as in an infinite series, or row of ascending numbers, tho' you may still advance higher and higher; yet all that you can do by that progress, is to go farther and farther from the first term of the progression, without ever reaching, or so much as approaching to an infinite number.

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PHYSICS.

The manner wherein this is to be done.

Numerous other arguments might be brought, to shew the immense inferiority of man's intellect to God.

I think, then, that it becomes us to use an awful circumspection ; not only when we make philosophical inquiries about God, that is, when we presume to discourse of him ; but when we solemnly design to praise him ; for, 'tis one thing to say true things, and another to say things worthy of God. Our ideas of him, may be the best we are able to frame, and yet may far better express the greatness of our veneration for him, than the immensity of his perfection ; and, even those notions that may be worthy of the most intelligent of men, will fall extremely short of being worthy of the incomprehensible God. The brightest and least unlike idea we can frame of God, is infinitely more inferior, with regard to him, than a parhelion is with regard to the sun. He has not, in my opinion, the truest veneration for God, who can set out his excellencies, and prerogatives, in the most high and pompous expressions ; but he who, willingly, has a deep and real sense of the unmeasurable inferiority of himself, and his best ideas, to the unbounded and unparallel'd perfections of his maker. And, as even our hymns and praises of the supreme Being, deserve our blushes, and need his pardon ; what confusion will, one day, cover the faces of those, who not only speak slightly and carelessly, but often, contemptuously, of that supreme, and infinitely perfect Being, to whom they owe those very faculties which they so ungratefully, and impiously misemploy ? Indeed, such transcendent excellencies, as are the divine, might justly discourage us from offering so much as to celebrate them, if infinite goodness were not one of them. I shall not, therefore, allow myself the presumption of pretending to make a panegyric of God ; but, content my self with an humble adoration of those perfections, whereof my utmost praises would rather express my own weakness, than their excellence ; since, of this ineffable object the highest things that can be expressed in words, must fall short ; for words cannot express him



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THE
P R E F A C E

STATICS is that part of natural philosophy, which considers the gravity of bodies, in all sorts of positions; and is, sometimes, taken for the same as particular mechanics, or the doctrine of motion in bodies, which depends upon their position. But, hydrostatics, in general, regards all statics.

STATICS.

As its denomination implies, it is the weighing of bodies in water; and its branch is contained in this sense; when we are here principally concerned with a dead weight, before Mr. Boyle's doctrine to improve it, consisted chiefly in theory and speculation, having only been treated by mere mathematicians; but, that noble philosopher soon reduced it to practice, and applied it to useful purposes. Some of its propositions, formerly demonstrated in a mathematical manner, he proved by the direct, obvious way of experiment; that is, by the proper way, or hydrostatically. He, also, made many new discoveries in that part of knowledge, which are highly useful in life, and teach us to examine the goodness of drugs, of metals, minerals, and other

Vol. II.

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The first of the following pieces appears under the title of paradoxes; and, truly, the propositions it contains, were paradoxes, even to some mathematicians, at the time wherein they were published. But is this strange, since, as the learned Whiston observes, many persons taking gravity for a permanent power in matter, which must remain unalter'd, so long as it continues its state; and supposing fluids, whilst confus'd, and

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P R E F A C E.

STATICS is that part of universal mechanics, which considers the gravity of bodies, in all sorts of mediums; tho'tis, sometimes, taken for the same as particular mechanics, or the doctrine of motion in bodies, which depends upon their gravity. But, hydrostatics, in general, regards the weight of bodies in all manner of fluids; and, therefore, differs but little from statics, unless when bodies are weighed in vacuo; or, where there is no sensible resistance. There is, also, a more limited signification of the word, hydrostatics; which restrains it, as its derivation implies, to the weighing of bodies in water; and'tis hydrostatics in this sense, wherewith we are here principally concern'd: a doctrine which, before Mr. Boyle undertook to improve it, consisted chiefly in theory and speculation; having only been treated by mere mathematicians: but, that noble philosopher soon reduced it to practice, and applied it to useful purposes. Some of its propositions, formerly demonstrated in a mathematical manner, he proved by the direct, obvious way of experiment; that is, by the proper way, or hydrostatically. He, also, made many new discoveries in this part of knowledge, which are highly useful in life, and teach us to examine the goodness of drugs, of metals, minerals, and other bodies, both solid and fluid.

The first of the following pieces appears under the title of paradoxes; and, truly, the propositions it contains, were paradoxes, even to some mathematicians, at the time wherein they were published. Nor is this strange, since, as the learned Wolfius observes, many persons taking gravity for a permanent power in matter, which must remain unalter'd, so long as it continues its state; and supposing fluids, whilst confin'd, and

at rest, wholly unable to act upon bodies; no reason appears to them, why they should, as it were, take away a part of the gravity of the bodies immersed in them; and, much less, why they should, sometimes, throw them upwards with a great force. This was remarkably the case with that great scholar, Dr. More; who, to solve such an appearance, would fain have introduced a new, and an immaterial principle into physics. And this gave birth to another of the ensuing pieces; which, tho' it also relates to some experiments, that appear under the head of pneumatics, we thought fit to range intire, under this of statics; because, in strictness, 'tis wholly hydrostatical; taking that term in its larger sense; because we would not break in upon the order of the pneumatical pieces; and, lastly, because hydrostatics ought, always, to precede pneumatics; since the latter cannot be understood without a knowledge of the former.



Hydro-

Hydrostatical Paradoxes,

Proved and Illustrated by

EXPERIMENTS.

POSTULATA, & LEMMATA.

1. **S**uppose a tube, open at both ends, held, with one of them, perpendicularly, under water; the lower orifice may be conceiv'd to terminate in a plain parallel to the horizon, or the upper surface of the fluid.

2. All assignable equal portions of this surface, will be equally pressed by the water perpendicularly incumbent thereon.

For the fluid being here supposed homogeneous, as to its gravity, and to stand, at the same height, upon all the parts of the imaginary plain; no equal part can possibly be more pressed than another, in the same surface.

3. If any part of this imaginary plain, be pressed with a greater weight, than another, the former will be either displaced, or depressed.

Thus, whilst an heavy body sinks in water, that part of the imaginary plain, contiguous to the lower part of the body, being pressed by a greater weight than the other portion of the same surface, must needs give way, successively, till the heavy body arrives at the bottom.

4. On the other hand, if any part of the imaginary surface be less pressed upon than the remainder, it will, by the weight on the remainder, be impell'd upwards, till the pressure there be equal to that upon the other parts of the same surface.

If this be doubted, the following experiment will prove it. If a cylindrical glass tube, open at both ends, be steadily held in a perpendicular posture, with one of them immersed, two or three inches below the surface of a proper quantity of water, in a glass vessel; the surface of the water within-side the tube, will be nearly level with the surface that is without-side the same; because the water, in both cases, has a free communication.

If

STATICS.

If, now, a convenient quantity of oil, be gently pour'd upon the water that is external to the tube, that within the tube, which we shall call internal, will gradually rise, and continue to do so proportionably; because the imaginary plain, cutting the immersed orifice of the tube, is every where pressed by an additional weight, except in the orifice itself: which pressure must necessarily be increas'd, as more oil is pour'd upon the external water; whilst a circle of our imaginary plain, equal to the lower annulus of the tube, is, by the sides thereof, guarded from the immediate weight of the oil; whence the external water, being more pressed than the internal, is, consequently, forced up thro' the tube, where there is the least resistance, till the cylinder of water within the tube, gravitates upon the subjacent part of the imaginary plain, equally with the sum of the water and oil, upon every other equal portion of the same surface.

5. The air is a ponderous body.

This has been shewn by several experiments; some whereof, indeed, are excepted to; but the following is unexceptionable, and was often repeated.

Having obtain'd a thin, and large glass bubble, with a slender stem, and gradually rarified the air it contain'd, as much as I conveniently could, by the flame of the lamp, whereat the bubble was blown; the stem, whilst the bubble remain'd exceeding hot, being nimbly put into the flame, thereby became hermetically sealed, in an instant. This glass we permitted to cool leisurely, and afterwards weigh'd it in a very exact balance; then carefully breaking off the sealed end, and preserving the fragments, we weigh'd the glass again, and found it considerably heavier than before; which could be owing to nothing but the natural air, that rushed into the bubble, upon breaking off its stem, whilst it contain'd air greatly expanded. For, the noise hereof, in entering, may be plainly heard by an attentive ear; and, as the rarification is here great, so the bubbles will sometimes break, by the bare force of the external air; and, lastly, if the sealed end of the glass be broke off, under water, that fluid will, by the pressure of the atmosphere, be forced into the bubble, like an artificial fountain, till it be about three quarters full: whence the weight of the air appears to be very considerable; since the fourth part of what the bubble would contain, remains therein not taken notice of by the balance. In one repetition of this experiment, I found the air fresh admitted into the bubble, weighed near $\frac{1}{4}$ of a grain, and its full content of water 906 grains; whence, if the re-admitted air only filled $\frac{1}{4}$ of the bubble, the whole air it contain'd, may be reasonably supposed at a grain. And thus the water weigh'd about 900 times as much as the air of the bubble; which, all things consider'd, comes near enough to some other trials, whereby we determin'd the specific gravity of air, to be to that of water, as 1 to 1000.

P A R A -

In all fluids, the upper parts gravitate on the lower.

Suppose one end of a small, cylindrical, open glass tube, plunged into oil of turpentine, and that liquor to be raised, by suction, to a convenient height therein; when the lips being removed, and the upper orifice of the tube nimbly stopp'd with the finger, to prevent the fluid from falling back, imagine it thus placed perpendicularly in the glass ABCD Fig. 2. almost fill'd with water, so that the surface of the oil may stand somewhat higher than that of the water. This done, and the finger removed from the upper orifice of the tube, the oil will not fall out at the lower, but remain suspended near its former altitude. But oil of turpentine, being an heavy fluid, has a tendency downwards; and as the lower orifice of the tube is open, it must necessarily fall out thereat, did not the pressure of the water underneath sustain it. No contrariety in the nature of these two liquors, can be supposed the cause of this phenomenon; for, if the finger be removed, before the pipe is sufficiently immersed, the oil will subside till it becomes a balance to the water; but the reason of it is manifestly this. Suppose the plain wherein the extremity Q, of the pipe PQ, rests, to be GH; if that part thereof whereon the oil rests at Q, be equally pressed by the incumbent cylinder of oil QX, with the other parts of the same surface GH, by the water incumbent upon them; the part Q, can neither be depressed by the cylinder of oil QX, nor raised by an equal pressure of water upon the other parts of the plain GH. That this equilibrium, betwixt the oil and water, is justly assign'd for the cause of the phenomenon, appears farther, 1. From what is already observed, viz. that if the cylinder of oil reach much higher than the surface of the water, the oil will descend: the reason whereof is, that the plain GH, being more charged at Q, than in any other part, it is there unable to make resistance; whence it must necessarily be thrust out of its place, by the descending oil. 2. Because the oil will continue subsiding only till its surface becomes almost level with that of the water; when the part Q, is pressed by the oil, equally with the other parts of the plain GH, by the water incumbent on them. 3. If, while the oil and water are balanced, the pipe be gently raised from Q to S, the oil will preponderate, and consequently fall out in drops, which by the superior specific gravity of the water, will be buoy'd up, and so float on the surface thereof; and as the pipe is gradually elevated towards the surface LM, more and more of the oil will drop out: but if the tube be stop'd any where in its ascent, at S, for instance, the oil will cease to flow out: and, as at the plain IK, the pressure of the water, by reason of its diminish'd depth LI, upon the other parts of the surface, is not near so great, as upon the plain GH; so the remaining cylinder of oil incumbent on S, is, by reason of its proportionable efflux, unable to press that part more forcibly than the others of the same plain IK, are press'd by the incumbent water. And, if the lower orifice be raised almost to V, or near the upper surface of the water.

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water LM, scarce any oil, for the same reason, will remain in the pipe TV. 4. But if, when the end of the tube rests in the plain GH, where the oil is a balance for the water, it be let down to O, the external water will cause the oil to ascend in the tube, the height of EG; so that the pipe will contain, besides a cylinder of oil α W, a shorter one of water α O; for, now a new plain EF, cuts the lower orifice of the tube, whilst that part of it at O, being, by the incumbent oil, pressed less than the others, by the incumbent water, the oil will be buoy'd up, till the water O α , and the cylinder of oil α W, taken together, gravitate as much upon O, as the rest of the incumbent water does upon the other equal parts of the same plain EF. Since, then, the oil, in this case, may be kept suspended at any point, under water, as at Q, being there in an equilibrium with the external fluid; since, when elevated, as from Q to S, it falls by its own gravity; and, since the deeper it is plunged, the greater weight and pressure are required in the cylinder of oil, to balance the pressure of the water; it follows, that the parts of the water incumbent on the plain GH, press that surface more than the plain IK, is pressed by the parts of the water contiguous to it; and consequently, that all the parts of the water which lie under the upper surface, are pressed by those which stand directly over them; as the upper parts of the oil press'd down the lower, whilst the pipe was drawn up from Q to S; so that the oil flow'd out at the orifice S: for since the lower parts of a liquor press proportionably to the height of the fluid, they must necessarily derive their force from the incumbent parts, which, consequently, gravitate upon them.

A glass bubble, about the bigness of a pullet's egg, was purposely blown at the flame of a lamp, with a long stem turn'd up at the end, that it might be the more conveniently broken off. This bubble being well heated, to rarify the air, and thereby drive out a large part of it, was presently sealed at the end; and, by the help of the figure of the stem, was, by a convenient piece of lead, sunk under water, the weight and glass being tied by a string to one scale of a good balance, in the other whereof was placed a counterpoise to the bubble, as it hung freely in the midst of the water. Then, with a pair of forceps, I carefully broke off the sealed end of the bubble, under water, so that no air appear'd to emerge, or escape thro' the water; but the liquor, by the weight of the atmosphere, sprung into the empty part of the bubble, and about half fill'd the whole cavity; whereupon the bubble immediately subsided, and made the scale, whereto 'twas fasten'd, require 4 drams, and 38 grains, to bring it horizontal. Then taking out the bubble, with the water it contain'd, we, by means of the flame of a candle, warily applied, drove out the water, which, otherwise, is not easily excluded at a very narrow stem, into a glass counterpois'd before-hand, and found it to weigh about 4 drams, and 30 grains, besides a little that remain'd in the bubble, and a small matter that might have been rarify'd into vapour; which, added to the piece of glass, broken off under water, and lost there, might very well amount to 7 or 8 grains. By which it appears, not only, that water has some weight, or gravitates in water,

water, but that it weighs very near, or altogether as much in water, as in the air. We repeated the experiment with another sealed bubble as large as a great hen-egg, and with the like success.

COROLLARY.

From hence it appears, why the surface X, of the oil in the pipe PQ, rises a little above the level LM, of the external water. The slenderness of the pipe may, indeed, contribute hereto; but there would, otherwise, be an inequality. For, since oil of turpentine is specifically, that is, bulk for bulk, lighter than water; and since an equilibrium between them is here constantly observ'd; the height of the oil incumbent upon Q, must, of necessity, be greater than that of the water incumbent on the other parts of the same plain GH, to make the pressure on both sides equal. And were the difference between the specific gravities of these two liquors greater, the difference in the heights of their surfaces X and LM, would be greater also; as may be try'd, by substituting oil of tartar *per deliquium*, a heavy saline liquor, for water.

SCHOLIA.

1. What we have here said, or shall hereafter say, of the properties of water, is applicable to all ponderous fluids, unless where cause of exception appears in particular cases.
2. 'Twas before hinted, that in very small glass pipes, many fluids (I except quick-silver) will appear with their surfaces higher than that of the liquor wherein they are immersed; and that, as far as I have observ'd, in proportion to the smallness of the bore: but this being of small consequence in our present experiments, we shall take no farther notice thereof.
3. Notwithstanding this inconvenience of small tubes, we chuse to make use thereof; because those of a wider bore are apt, upon any small inequalities of pressure, to suffer the oil and water to pass by each other in their cavity, and so disturb the experiment.
4. Common oil and water, or any two liquors that will not mix, may serve in most of our experiments. I made choice of oil of turpentine, because 'tis light, thin, clear, colourless, easily obtainable, not apt to spot apparel, or greatly to adhere to porous bodies; and its offensive scent is easily corrected by oil of rhodium, &c.
5. Oil of turpentine will corrode copper, whence by digesting it upon the crude filings of that metal, a deep green liquor is obtainable; which used instead of the colourless oil, will render the distinction of the fluids more conspicuous.
6. For the same purpose, I frequently employ a decoction of brazil, log-wood, or the common red ink; which must be made very strong and deep, otherwise their colour in a slender pipe will be scarce discernible.
7. As to the shape of the glass-vessel, we need not be solicitous, tho' that of a wide-mouth'd jar be for some uses the most convenient. The depth hereof, and the length of the tubes must be determin'd by the experiments to be made.

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8. It ought to be no discouragement, if the oil of turpentine suck'd up, do not remain in the glass, upon the first or second attempt; 'tis best to draw up more than is necessary, and afterwards to let in the air gradually between the orifice of the tube and the finger; which will suffer the liquor to descend to the height required.

9. Where a considerable difference is required between the two fluids made use of, oil of tartar *per deliquium*, as we said, tinged with cochineal, may supply the place of water, and highly rectified spirit of wine that of oil of turpentine: for these two liquors, tho' they readily unite with water, will not easily mix with each other. Or a very strong filtered solution of sea-salt, may be substituted for water, occasionally. I have sometimes added oil of turpentine to spirit of wine and oil of tartar, and thereby obtain'd three liquors of different gravities, which will not easily mix; by plunging a pipe, with water in the bottom of it, below the surface of the lowest of these liquors, and raising and depressing it in a proper manner, the experiment may be agreeably varied; but still continue explicable by the principles we deliver.

10. This proposition has been thought liable to the following objections. 1. If the upper parts of water gravitate upon the lower, the latter must be more dense than the former. 2. Divers are sensible of no compressure under water. 3. Tender plants grow at the bottom of the sea. 4. An immersed heavy body is supported and drawn out with greater ease, than if no water were incumbent thereon; for it weighs less in water, than out of it. 5. A bucket of water is lighter when immersed than before; and weighs less in water, than, when empty, out of it; therefore, the water in the bucket does not gravitate, either on the bucket, or the water below it: whence the schools have determin'd, that fluids never gravitate in their proper places. 6. Horse-hairs preserve any place assign'd them in water; and do not sink by the incumbent weight thereof. 7. Were the proposition true, all the lower parts of water would be in perpetual motion, and constantly expel the upper.

To answer each of these objections in their order. 1. If the component parts of water be perfectly hard and solid, the upper may press upon the lower without condensing them; as would be the case in a heap of diamond dust. And, possibly, the parts of water will not sensibly yield to so small a weight as that of the incumbent fluid, at least in those heights whereat experiments are usually made; for, in fact, water is not compressible by an ordinary force: nor would any absurdity follow from our doctrine, were the lower parts of the sea condensed by the upper. 2. That divers are insensible of any compressure under water, proceeds from the surprizingly strong resistance of an animal body to an uniform force, at once applied to all the external parts thereof; but that, if a proportionable part of this pressure was sustain'd by one limb, and not by all the rest at the same time, 'twould be greatly pain'd thereby, appears sensibly from covering the mouth of an open cylindrical vessel, with the palm of the hand, whilst applied like a receiver, to be exhausted by the air-pump; for as soon as the

included

included air is withdrawn from under your hand, it will be vehemently press'd from without, against the edge of the vessel; as if some weight were laid upon the back of your hand, and forced it into the hollow of the cylinder. But that so vast a weight of water, as must compress a human body at the depth of near 100 feet below the surface of the sea, (where, a diver told me, he had remain'd without pain) will not sensibly affect it, may appear from the following experiment. To a small, cylindrical, glass syphon, sealed at one end, we exactly fitted a rammer, by means of soft leather, so that neither air nor water could pass between it and the sides of the glass; then pouring in a proper quantity of water, and leaving some inches height of air above it, we let a young tadpole, the body of which animal is exceeding soft and tender, swim about in the glass; when a man forcing the rammer down the tube with his utmost strength, the bulk of the tender creature seem'd somewhat diminish'd by the pressure; however, it continued to move freely about in the water, and often ascended to the top thereof; tho' the instrument stood perpendicular to the horizon: nor could we perceive that the animal suffer'd the least injury hereby. This experiment we frequently repeated, with tadpoles of different ages; whence it may fairly be concluded, that the texture of animals is so strong, that tho' water actually press on water, the body of a diver, at a great depth therein, ought not to be sensible of its weight: for the air being here compress'd into an eighth part of its natural dimensions; the tadpole sustain'd a pressure of water equal to that of a cylinder of the same near 300 feet high.

3. That plants shoot up under water, is owing to the subjacent pressure of the same; the force whereof, we see, is able, when they are detach'd from their roots, to throw them up to the surface, notwithstanding the direct weight of the incumbent fluid. 4. Tho' the water incumbent on a heavy body actually tends to depress it, yet that tendency may not cause it to sink deeper; by reason of an equal or greater pressure upon all the other parts of the same plain, wherein its bottom rests; in which case, the hand that sustains it will not feel the weight of the incumbent fluid, but be able to raise the body more easily here, than in the air: yet if the incumbent water be removed, as in the proof of our eleventh paradox, a great pressure will appear to have been there sustain'd by the body. 5. To pass over that senseless expression of fluids not gravitating in their proper places, I thus solve the phenomenon of the immersed bucket. Suppose ABCD the well, EF the rope whereby the bucket is suspended under water, with its bottom resting on the plain IK; if the bucket wholly consist of wood specifically lighter than water, it must be so far from weighing upon the hand at E, that its upper parts must needs rise above the top of the water: but if this vessel be a composition of wood and iron, it may be specifically heavier than water; in which case, the hand that draws it out, will feel only that weight, whereby it exceeds an equal bulk of the fluid; the water in the cavity G, and so much as rests thereon, being balanced by the pressure of the other water above the plain IK: whence the bottom of the bucket is pressed as forcibly upwards, as the

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weight of the incumbent water, in the bucket and above it, tends to depress it; so that the hand has no more to lift, than the excess of weight of the wood and iron of the bucket, above that of an equal bulk of water. And farther to shew the absurdity of that scholastic doctrine, of water's not weighing in its own element, drawn from this experiment of the bucket, I perform'd the same thing, in water, with a small box fill'd with butter, and some pieces of lead, to add to its weight; and I hope, it will not be pretended, that the butter and lead did not gravitate here, because they were in their own element*. 6. That horse-hairs, supposing them of the same specific gravity with water, stand suspended at any depth in that fluid, is easily accounted for upon our principles. Suppose the body R, to be of the same specific gravity with water, it cannot press that part of the plain S, wherein it rests, either more or less than an equal bulk of water; and consequently, tho' the whole column that stands above it, directly presses thereon, that does not sink it; being balanced by the weight of the other water above the plain IK; whence the body will neither be depressed nor rais'd. And as this must be the case at what depth soever the body be immers'd, it must necessarily keep in the place assign'd it. And I once procur'd glass-bubbles so nicely poised, as to exhibit this phenomenon with a surprizing exactness. 7. If the last objection supposes the lower parts of water to be of any considerable bulk, 'tis already answer'd, when we shew'd that the body R, would retain its place assign'd, any where in the water. And unless we knew the particular magnitude and figure of the component particles of water, 'tis to no purpose to dispute this point. I am, however, so far from thinking it absurd, that the lower parts of water should be in constant motion, that I take this to be the very property that constitutes a fluid; which, tho' not visible, is easily discoverable by its effects. Thus, when salt is thrown into water, the parts near the bottom soon raise saline corpuscles to the top, where they afterwards shew themselves in small, floating, cubical grains.

P A R A -

* This matter cannot be set clearer than in the words of Sir Isaac Newton; who observes, that bodies immersed in fluids, have two kinds of gravity; the one true and absolute, the other apparent, vulgar and relative. "Absolute gravity (says he) is the whole force wherewith a body tends downwards; but the relative and vulgar is that excess of gravity, whereby a body tends downwards, more than the fluid which surrounds it. By the former kind, the parts of fluids, and of all bodies, gravitate in their proper places; and, by their joint weights, compose the weight of the whole. For every whole has weight, as is evident

" in vessels fill'd with liquids; and the weight of the whole being equal to the weights of all the parts, must of necessity be composed of them. But bodies, by the latter kind of gravity, do not gravitate in their own places; that is, do not, when compared with one another, pregravitate; but mutually hindering each other's endeavour to descend, they remain in their places, as if they had no weight. Bodies in the air which do not pregravitate, are thought by the vulgar not to be heavy; but those which pregravitate, they judge to be heavy, so far as the air does not support them: so that the weight of bodies among
" the

PARADOX II.

A lighter fluid will gravitate upon a heavier.

If you suck up fair water in a slender glass pipe, to the height of three or four inches, nimbly stop the upper orifice with your finger, plunge the lower into a glass of oil of turpentine, till the surface of the oil in the vessel rise somewhat higher than that of the water in the tube, and then, lastly, remove your finger; the water will not fall out, but continue suspended by the pressure of the oil; as appears evident from the foregoing proposition, whereof this is only the converse. And if either more oil be poured into the vessel, or the tube be immersed deeper therein, the water will be buoy'd up towards the top of the pipe; that is to say, a heavier fluid will be raised by a lighter. Since then, by the preceding proposition, the liquor rises in the pipe by the gravity of the external fluid, it follows, that a lighter liquor will gravitate upon a heavier.

COROLLARY.

As the surface of the oil in the tube, was always, in the former experiment, higher than that of the external water, so, here, the surface of the water in the pipe, will constantly be lower than that of the oil; because in the plain EF, the cylinder of water IG, contain'd in the pipe Fig. 52. IH, will, upon account of its greater gravity, press as much upon the part I, as the oil KI, IL, which is specifically lighter than it, does upon the other parts of the same plain EF.

SCHOLIA.

1. This second paradox may be farther confirm'd from the common phenomena of glass bubbles, the explication whereof is as follows. The bubble X, consisting of glass, which is specifically heavier than air, which is Fig. 6. lighter than water, and of water it self; whilst the whole composition is lighter than an equal bulk of water, it will float; but if it grows heavier than so much water, it must, according to the laws of hydrostatics, sink. When, therefore, there happens any considerable pressure upon the water wherein the bubble is generally immersed; since glass is a solid body, and

“ the vulgar, is only the excess of their
“ real weight above that of the air. And,
“ therefore, they call those things light,
“ which being less heavy than the air,
“ and yielding to its greater gravity,
“ mount upwards. But these bodies are
“ only comparatively light, not really so;
“ for they will descend *in vacuo*. Thus
“ bodies, which by reason of their greater
“ or less gravity, descend or ascend in
“ water, are but comparatively and ap-
“ parently heavy or light; and their com-

“ parative and apparent levity, is the ex-
“ cess or defect, whereby their real gra-
“ vity either exceeds or falls short of the
“ gravity of water. But whatever bo-
“ dies neither descend by pregravitating,
“ nor ascend by yielding to one that pre-
“ gravitates; tho' they still, by their real
“ weights, increase the weight of the
“ whole; yet comparatively, and in a
“ popular sense, they do not weigh in
“ water.” *Newton. Princip. p. 264.*

water

STATICS. water incompressible, but the air included in the bubble, elastic, and easily compressed; this will shrink, and so possessing less space than before, the contiguous water will succeed in its place; which being a body that is a thousand times specifically heavier than air, the bubble thereby becomes heavier than an equal bulk of water, and consequently sinks: but, upon the removal of this pressure, the pent-up air will, by its own spring, free it self from the intruding water; whence the composition of the bubble growing lighter than an equal bulk of water, it will, from the bottom, immediately emerge to the top.

2. Into a long glass pipe, sealed at one end, and open at the other, pour common water, till about half a yard thereof remains unfill'd. In the next place poise a glass bubble, with a slender neck, so that a small addition of weight will sink it; and place it on the surface of the water in the tube; then continue pouring oil of turpentine gently thereon, till it rises to a sufficient height; when the bubble will descend to the bottom, and there continue as long as the oil remains at that height above the water. The reason whereof is, that the oil, tho' lighter than water, gravitates upon that fluid below it, and thereby forces a part thereof into the open orifice in the neck of the bubble, which thence becomes heavier than an equal bulk of water, and consequently descends to the bottom; and the cause of this submersion, the pressure of the oil, continuing, it must necessarily remain there. In confirmation hereof, it may be added, that if the cylinder of oil be diminish'd, or taken away, the bubble will again ascend to the top of the water; because the water being relieved from the pressure of the oil, the air, by virtue of its own elasticity, recovers its former dimensions, and thereby renders the bubble as light as before.

3. The first paradox may, also, be confirmed by the following experiment: Having provided a tube and bubble, as just mention'd, pour water into the tube, to the height of 6 inches, then cast in the bubble, and keep it constantly plung'd below the surface, whilst more water is slowly pour'd into the tube, to a considerable height; when, the bubble, which before attempted to emerge, will, by the additional weight of the incumbent water, be depressed to the bottom of the tube, and there continue, whilst the same weight of water remains.

4. Tho' the weight of the incumbent water alone, here causes the bubble to sink, yet, if it were not kept immersed, a still greater weight of water would not depress it; because, being left to itself, it would rise with the fluid, and never permit the same to come sufficiently above it for that purpose. And that the weight of the incumbent water really sinks the bubble, and keeps it at the bottom, appears from taking away a sufficient part thereof; for, when that is done, the bubble will presently, and of itself, begin to float up towards the surface, from whence it may easily be made to subside, by pouring back the same water that was taken out.

5. The reason why oil of tartar *per deliquium*, was not here made use of, instead of fair water, is, that in such small pipes, as those employ'd about the first experiment, I found that liquor so ponderous, as to flow
down

down into the oil of turpentine, whilst the oil passed upwards by it, along the other side of the tube : whence I am surprized, that *M. Paschal* should say, that if a tube above 14 feet long, with one orifice placed 14 feet under water, be full of quick-silver, it will not all run out thereat, tho' the top be exposed to the air, but stop at the height of one foot; because the moment it acquires in falling, must, probably cause the whole to come out. My own trials with quick-silver, and small tubes, with this more favourable circumstance of oil of turpentine, and oil of tartar, tempt me to suspect, that *M. Paschal* never made the experiment, at least, not in a tube so large as his scheme supposes. Experiments, that are only true in speculation, should be proposed as such, for they may often fail in practice.

6. This second paradox, I am sensible, contradicts the current opinion of the schools; and even of many modern mathematicians, and writers in hydrostatics; but whether the cause of gravity be the trusion of any superior substance; or the magnetic attraction of the earth, &c. there is, in every heavy body, a constant tendency towards the centre of the earth, that cannot be destroy'd by the interposition of another heavier body, tho' its nearer approach towards the centre, may be hindred thereby; the latter, from its greater gravity, obtaining the lower place. But then, the lighter body tending downwards, must needs press upon the heavier that lies in its way; and jointly therewith, press upon whatsoever body supports them both, with a weight made up of their united gravities. Some men of learning are kept from acknowledging this truth, by finding that a lighter fluid, surrounded by a heavier, does not descend, but emerge to the top; whence they consider the former, not as a heavy body, but a light one. 'Tis true, in respect of the heavier liquor, the less heavy may be called light; yet it still retains its own absolute gravity, or tendency downwards, as strongly as before; tho' by a contrary and more powerful tendency of the contiguous liquor upwards, such its endeavour does not appear. Thus whilst a piece of light wood emerges from under water, it retains its gravity in its ascent, being only buoy'd up by the water, whose specific gravity is the greater; so that the sum of the water, and the ascending wood, weighs more than the water alone would do; and therefore, when this wood floats, with some part above the surface of the water, a lighter body actually gravitates upon a heavier. Thus, also, were a man weigh'd in a balance, with a stone in his hand, which he lifts above his head; tho' that may seem light, in respect of his body, yet it loses nothing of its natural weight, neither during its ascent, nor afterwards: for the hand that raises it up, will constantly perceive its tendency downwards, and the stone; and the man together, will weigh no less in the scale, than if he did not support it, but both were weigh'd apart, and their respective weights afterwards added together. And thus, lastly, if a glass of water, with a large quantity of common salt, in powder, be balanced in the scale, whilst the salt gradually dissolves and becomes one body with the water; yet the weight here will not be diminish'd as the salt comes to be supported by the fluid; but both together will gravitate, as at first, on the scale that supports them. PA-

Hydrostatical Paradoxes.

PARADOX III.

If a body be wholly, or in part, immersed below the surface of water, its lower part will be pressed upwards by the water contiguous to it, from beneath.

This appears from the first experiment, ; for, where-ever the lowest part of the body be, in either case, supposed, all the parts of the plain, wherein it rests, are pressed upwards. And if this force under the body be greater than that wherewith it tends downwards, 'twill buoy it up ; but if the body be specifically heavier than water, the tendency of the contiguous fluid from beneath upwards, proves only ineffectual to raise or support it, but is by no means destroy'd ; the heavy body being constantly resisted and retarded by the water, as it would be in a pair of scales, were it hung at one end, and an equal bulk of water at the other end of the beam.

Fig. 3.

The same truth is farther manifest from the third experiment ; for, if the tube, wherein the water is suspended, be plunged deeper into the oil ; or, if more oil be poured into the vessel ; the water in the tube, will rise the higher ; which could not happen, unless the oil, tho' specifically lighter, pressed against the lower surface of the water more forcibly than the water tends, by its gravity, downwards. And even when the two fluids balance each other, the oil continually presses upwards against the lower surface of the water ; for herein consists its constant resistance to the perpetual endeavour of the water to descend. And since the same phenomenon happens, whether the water be suspended in oil, or *vice versa*, the proposition holds equally true, let the body be specifically heavier, or lighter than the fluid wherein it is immersed.

SCHOLIA.

Fig. 7.

1. That water makes a resistance to bodies which sink therein, is thus demonstrated. Suppose the pipe E F, contains oil of cloves, or any other oil, that is specifically heavier than water ; that the oil in the tube, and the external water are in equilibrium ; and lastly, the pipe to be slowly raised towards the top of the vessel : 'tis evident, that drops of oil will, during the tube's ascent, fall from the bottom thereof to that of the glass ; but much more slowly than if they fell in the same manner, thro' air.

2. To compute the quantity of pressure against the lower parts of a drop, suppose it to be G, touching the plain H I ; we must observe, that if the drop of oil were not there, its place would be supplied by an equal bulk of water ; which being of the same specific gravity with the rest of the fluid in the vessel, the surface H I, would be every where equally pressed ; and, consequently no part thereof be displaced. But the drop of oil being heavier than an equal bulk of water, that part of the surface whereon it rests, is more pressed than any other equal part of the same ; and therefore, will give way to the descending drop. And as this must be the case in all the other surfaces as well as in H I, the drop will continue



nue falling, till it arrives at the bottom of the glafs. And if the drop G, were not specifically heavier than water, but barely equal thereto, the contiguous part of the surface H I, would be no more pressed than the other equal parts thereof, and, consequently, it would be neither depressed nor raised; but the drop G, continue in the same place: whence we conclude, with *Archimedes*, and others, that a body immersed in a fluid, of the same specific gravity with itself, will rest at any assignable depth therein. Since, then, if the drop G, be of the same specific gravity with water, it will neither sink, nor emerge; if it be heavier, 'twill only fall by the weight whereby it exceeds an equal bulk of water; for this, alone, is the cause of its descent, the other being wholly resisted by the water; and consequently, it loses in the water the weight of an equal bulk of water, weighed in the air.

3. This grand theorem is thus easily confirm'd by experiment. Hang a piece of lead, tied to an horse-hair, from one scale of an exact balance, counterpoise it, and suffer the lead to sink down, and hang freely, in a vessel of water; and you will find the former counterpoise much too heavy for the lead; part of it, therefore, being taken away, till the balance be reduced to an equilibrium, 'tis easily seen what proportion of weight is lost in the water. If, then, another piece of lead be weigh'd in air, where it equals, suppose, 12 ounces, and afterwards be hung to the scale by an horse-hair, as the former, and weigh'd in water, you may conclude that 11 ounces, (supposing lead to be to water, as 12 to 1) put in the opposite scale, will now reduce the beam, how uneven soever it might hang in the air, to an horizontal position.

COROLLARIES.

1. From this pressure of water, against the lower surface of bodies immersed therein, may be deduced the cause why light substances emerge to the top thereof; which has, hitherto, remain'd an inexplicable phenomenon. Supposing, then, a pipe almost filled with oil of turpentine; if, as in our first experiment, the lower orifice be plunged under water, tho' not near so deep as the oil is high, and the upper end be gradually unstopp'd, the oil will fall, in drops, from the bottom of the tube, and presently emerge to the surface of the water; notwithstanding they are pressed as well downwards, as upwards: the reason whereof is, that the upward tendency of the water, contiguous to the lower surface of the oil, is stronger than the tendency of the oil, and the incumbent water downwards. Thus, when the drop G, touches the two parallel plains H I, K L, 'tis evident, that Fig. 7. the lower part of the drop G, is more pressed by the water, than the upper part M, because upon the whole surface K L, there is only an uniform pressure of the water, A K B L, whilst upon all the parts of the surface H I, there lies a greater quantity of Water, A H B I, except upon the part N, where the oil G, being lighter than an equal bulk of water, and exposed to a greater pressure from beneath, than its own gravity, with that of the incumbent water, can resist; it must necessarily give way, and

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be impelled upwards. And, as the same reasoning holds, for all the points in its ascent, it must, consequently, be driven, gradually, to the surface, and there float. Or, in other words; when any body, specifically lighter than water, is immersed in that fluid, it sustains the pressure of two columns of water, one against its upper, and the other against its lower part: and, as the height of these columns must be computed from the top of the water; the lower part of the immersed body is pressed upon by a column longer than that which presses upon the upper, by the thickness of the body, which, therefore, will be pressed more upwards than downwards. And the greater difference there is in the specific gravities of the water, and the body made use of, the swifter, *ceteris paribus*, will be the ascent; because of the greater pressure upon all the other parts of our imaginary plain, than upon that which is contiguous to the bottom of the ascending body.

Fig. 7.

2. Hence, also, we may solve that hydrostatical problem; why if two cylindrical pieces of wood, one of them twice the length of the other, be plunged to an equal depth under water, and thence suffer'd to emerge, at the same time, the longer gains the top before the shorter: for, suppose OP , to be one of these bodies, and two feet long, QR , the other, and one foot, and that they both rest upon the same plain, parallel to the upper surface of the water, and three feet distant therefrom; there will be a lateral pressure of the incumbent water upon the lower ends of each of these bodies, as great as that upon the equal parts of the plain whereon they rest; but upon the upper end of the shorter body QR , there will press a column of water two feet high, whilst the taller PO , sustains only a pillar of one foot; so that both being specifically lighter than water, they will be impell'd upwards; but the column consisting of one foot of wood, and two feet of water, will, by its gravity, resist this impulse more than that which is made up of two feet of wood, and but one of water; whence the reason of their unequal velocities is evident.

3. From hence, also, we may, probably, deduce the reason why, in distilling aromatic oils, with water, the whitish liquor, which frequently comes over, long continues turbid; for this chiefly proceeds from the exceeding minuteness of the drops, whereof it consists, allowing the height of the water, pressing upon the upper part thereof, to be almost as great as that which presses against the lower, so that the drops are raised but very slowly, tho' they will, at length, emerge; especially when, by their frequent occurrences, they have form'd drops of a larger size.

Fig. 8.

4. From this proposition, it, likewise, follows, that all bodies floating upon a fluid, sink till the immersed part be exactly equal, in bulk, to such a quantity of the fluid, as equals the whole weight of each body. For instance, if V , be a cubic yard of wood, 6 pounds in weight; since 'tis heavier than air, it will sink in water, suppose to the plain XW , where it must necessarily rest; for all the other equal parts of that plain XW , being pressed upon by columns of water, equal in height to XA , if the whole weight of the cube be greater than that of as much water as equals the immersed

immersed part thereof, it must sink lower, because the subjacent part at V, would then be more charged than the rest. On the contrary, if the cube were lighter than the water, whose place the immersed part possesses, it must by the greater pressure of the water upon the other parts of the plain XW, than upon that contiguous to the wood at V, be impell'd upwards, till the pressure of the whole cube upon the part that sustains it, be equal to that of the water upon the rest of the plain; and, consequently, be the same with the water, whose place the immersed part possesses: the lightness of that part, in respect of so much water, being compensated by the weight of what remains unimmersed above the level of the water. Thus, when a piece of wood is thrown into water, tho' the moment it acquires in falling, carries it thro' many imaginary plains below its proper station; yet the superior pressure to which each of those plains is exposed in all other parts, but in that contiguous to the bottom of the wood, presently impels it up again, till at length it rests in the place we have assign'd it.

S C H O L I U M.

This ingenious and useful proposition, relating to floating bodies, is mathematically demonstrated by *Archimedes* and his commentators; and may be farther manifested to the eye, by the following experiment. Having poured a convenient quantity of water into a large deep glass, and placed another deeper, that was properly shaped for swimming, within it; we furnish'd the latter with ballast, a deck, and other parts of a ship, till it sunk down to some particular marks we had placed on the outside. Then, observing how high the water reach'd in the larger vessel, we carefully placed two or three marks in a level with the horizontal surface of the water; and taking out the floating glass, we wiped the outside dry, and weigh'd it, with all its rigging, in an exact pair of scales; and afterwards pour'd an equal weight of water into the large glass, which we found reach'd up to the marks whereto our counterfeit ship had rais'd it. This experiment we several times repeated in vessels of different sizes, shapes, and lading. From whence it appears, that the floating vessel, with all it contained and supported, was equal in weight to the water, whose bulk equall'd the immersed part of the vessel, had it been cut off from the other, by a plain continu'd from the horizontal surface of the water. This theorem, with what we have brought to confirm, or illustrate it, may be apply'd, *mutatis mutandis*, to a ship with all her rigging and lading; for 'tis universal, that the weight of a floating body equals that of the water, whose space the immersed part possesses. And hence the learned *Stevinus* somewhere says, that a whole ship, with whatever belongs to it, presses no more upon the bottom it floats over, than a bulk of water equal to the immersed part of its hull.

Hydrostatical Paradoxes.

PARADOX IV.

A competent pressure of an external fluid, is, alone, sufficient to raise the water in pumps.

The truth of this proposition may appear from the preceding experiments; however, we shall give it a farther illustration and proof, as follows.

Suck deeply tinged water into a slender glass-pipe, to the height of an inch; and nimbly stopping the upper orifice, immerse the lower end into a glass half fill'd with the same tinged liquor, till the surface of that in the pipe be an inch below the top of the external fluid; next, pour oil of turpentine thereon, till it swim three or four inches above the water; when easing your finger, gently, from the upper orifice of the pipe, to admit a communication between the internal and external air, the liquor in the tube will be impell'd up almost as high as the surface of the external oil. Now, it cannot here be pretended, that the ascent of the liquor, in the pipe, is owing to nature's abhorrence of a *Vacuum*; since, as the pipe is full of air, and its orifice open, there's no danger of a *Vacuum*, tho' the water did not rise; the air and water remaining contiguous as before. The true reason, then, of this ascent, is, that, upon all the other parts of the imaginary plain cutting the immersed end of the tube, there is a pressure of water and oil swimming thereon, equal to four or five inches height of water; whilst, upon that part where the liquor contain'd in the pipe rests, there is only the pressure of one inch of water; whence the parts near the immersed orifice, must be necessarily thrust out of their places by those of the water that are more pressed, till so much liquor be forced up into the pipe, as makes the pressure on that part of the imaginary plain equal to that of every other such part; when, the water will rise no farther, but, by reason of the equilibrium, rest a little below the surface of the oil; the specific gravity of this being less than that of water. In like manner, it may happen in pumps; for as the oil of turpentine, tho' lighter than water, by resting upon the surface of the external fluid, forces up the liquor, within the pipe, far above the external water; so the air, tho' lighter than oil of turpentine, reaching to the height of many miles, and resting upon the water in a well, will press that water up the cylindrical cavity of a pump, much higher, if all impediments were removed, than the surrounding water. Now the obstacle here is either the sucker, or the pressure of the external air; the whole use of the sucker is, therefore, to free the water in the pipe from any impediment to its ascent; as, in our experiment, the sides of the tube sufficiently defend the water therein from any pressure of the external oil that might obstruct its rising. Lastly, as the liquor in our pipe was forced up, till the cylinder raised, became a balance to the pressure of the water and oil on the outside; so in pumps, the water rises but to a certain height, *viz.* thirty-three, or thirty-four feet, beyond which it cannot be impell'd; because at that standard the pressure of the water in the pump upon that part of the imaginary plain wherein its

lower

lower orifice lies, is the same with that the other equal parts thereof sustain, from so much of the external water and atmosphere as rests thereon. And this may suffice to shew, that nothing more than a competent weight of an external fluid, such as the air, is requisite to raise the water in pumps.

S C H O L I U M.

For a farther confirmation of this proposition, and, at the same time, to shew that a small quantity of quick-silver may be suspended in an open tube; I shall here offer another experiment. Having suck'd into the lower end of an open glass-tube, the diameter of whose bore was above the sixth of an inch, near half an inch height of quick-silver, and nimbly stopped the upper end with my finger; I plunged the mercury into a deep glass of oil of turpentine, being careful not to open the upper orifice, till the quick-silver was sunk eighteen or twenty times its own height below the surface of the oil; after which, it remain'd suspended, while the tube continued unstopped: and as if I raised it towards the upper surface of the oil, it would drop out; so if I depressed it deeper into that fluid, it would be impelled higher up into the pipe: and, by this means, so ponderous a body as mercury, was made sometimes to rise, and sometimes to fall, but still to continue supported by a liquor specifically lighter than common spirit of wine. This experiment, however, may easily miscarry, unless great care be used; and, after all, the oil will soon insinuate itself between the sides of the pipe, and so short a cylinder of mercury; for which reason, I here prefer water to oil of turpentine.

C O R O L L A R Y.

Since a cylinder of mercury, about thirty inches high, is equal in weight to one of water, about the height of thirty-three, or thirty-four feet; 'tis evident, that the pressure of the external air, which can raise and suspend that height of water, will do the same to the proportionable one of mercury, in the *Torricellian* experiment.

P A R A D O X V.

The pressure of an external fluid will keep an heterogeneous liquor suspended at the same height, in tubes of very different bores.

Pour a sufficient quantity of deeply tinged water into a clear, deep, wide-mouth'd glass, and close it with a broad, thin cork, wherein various round holes, of different magnitudes, have been burnt; into each of which you may thrust an open glass-tube of a corresponding size, so that they may stand parallel to one another, and perpendicular to the surface of the fluid, wherein they are immersed. Besides these holes, another aperture must be made in the cork, to receive the small end of a glass-funnel, by means whereof, oil may be conveyed into the vessel, when stopped; and in the slender part of this funnel, 'tis proper to place some cotton, to break the force of the descending oil, which might otherwise frustrate the experiment.

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Fig. 9. riment. Things being thus prepared, pour oil of turpentine thro' the funnel, till the tinged water be pressed up into all the pipes, and rise to a considerable height therein, above the lower surface of the oil; when, you'll plainly perceive the liquor to stand at the same level, both in the greater and smaller tubes, while in each it is plainly sustain'd at that height above the other water, by the pressure of the external oil; which being specifically lighter than water, its surface will remain somewhat higher on the outside of the tubes, than the tinged water they contain. And if, by means of a syphon, apply'd in the place of the funnel, the oil be gradually drawn off from the water; as the depth and pressure of the same decreases, the liquor in all the pipes, both great and small, will gradually subside. For, suppose EF, the surface of the water within-side of the tubes, as well as without them, before any oil swims upon it; the lightness of that fluid, compared to water, will prevent it, when pour'd thereon, from getting into the cavity of the pipes L, M, N: and, therefore, rising on the outside thereof, it must, by its gravity, press the water, and impel it up into them. And if the upper surface of the oil reach to GH, and that of the water descend to IK; by means of the quantity of oil made use of, the liquor in the pipes ought to have its several upper surfaces in the same level with each other, notwithstanding the great inequality of their bores; since that part of the plain IK, whereon rests the circular orifice of the largest tube L, is no more pressed by the incumbent water, than any other equal part of the same, is by the oil it sustains; the oil having only an additional height on the plain IK, to balance the pressure of the liquor in the pipe. And tho' the tube L were twice its present bigness, 'twould but press the subjacent plain IK, equally with the oil on the other equal parts. Nor ought this pressure of the external oil to raise the water in the slender pipe N, above the surface Q, in the same level with O; for were the liquor higher in the small pipe, it must press that part of the plain IK, whereon it rests with a force superior to that of the oil upon the other parts of IK, which is greater than the oil could resist; whence, consequently, the liquor in the slender pipe, must subside, till its surface fall below that of the external oil; for, till then, the different fluids cannot rest in equilibrium. In a word, it matters not how wide the cylinder of water be, which the oil is to support, provided it rise no higher than the difference in the specific gravities of the two fluids will admit of; for in this case, the pressure of water on the sustaining part of the plain, will be equal to the pressure of the external oil on all the other parts thereof; and, consequently, the two liquors must remain perfectly balanced.

S C H O L I A.

1. It may here be observ'd, that there's no necessity the glass-tubes L, M, N, should be of the same length; since, as their lower orifices are open, the liquor will rise to the same height within them, tho' they lie not in the same plain below the water.

2. We

2. We all along suppose, and particularly in the last experiment, that the smallest glass tubes employ'd are of a moderate size, and not exceeding narrow; or, in case they are, that allowance be made for the property of water's rising in them to a greater height, than can be attributed to the balance either of water or oil, that keeps it there suspended.

P A R A D O X VI.

The direct pressure sustain'd by a body, placed any where under water, with its upper surface parallel to the horizon, is that of a column of water, whose base is the horizontal superficies of the body, and height the perpendicular depth of the water; and if the water pressing upon a body, be contained in open tubes, its pressure is to be estimated by a column of the same, the base whereof equals the lower orifice of the pipe, and height a perpendicular from thence to the top of the water: and this, tho' the pipes stand obliquely, be irregularly shaped, or wider in some parts than the said orifice.

The former part of our proposition we might, with *Stevinus*, make more general, and thus demonstrate. Suppose *ABCD*, a solid rectangular figure of water, whose base *DC*, is parallel to the horizon, and height *GE*, a perpendicular let fall from the upper to the lower surface of the water; if that part of the base *EF*, be charged with more water than *GEFH*, the overplus must come from the adjacent water; suppose it then, if possible, to come from *AGED*, and *HBCF*: then, for the same reason, the base *DE*, will have more water incumbent thereon than *AGED*; and the like may be said of *FC*, whence the whole base *DC* will sustain a greater weight than that of the whole water *ABCD*, which is absurd. By the like reasoning, we prove that the base *DC*, sustains no less a weight than that of the water *GHFE*; since, then, it sustains neither a greater weight nor a less, than the column of water *GHFE*, it must, exactly sustain the same. Fig. 10.

To this demonstration, we shall annex sensible experiments, to confirm and illustrate each part of our proposition. First then, suck oil of turpentine into a slender, glass syphon, till it fill the shorter leg, and rise two or three inches high in the longer; then nimbly stop the upper orifice with your finger, and plunge the lower part of the syphon into a deep glass of water, till the surface of the oil in the longer leg rises but little higher than that of the external water, and upon removing your finger, the surface of the oil will either intirely, or almost, preserve its former station; when if the syphon be immersed deeper, the oil in the shorter leg will be depress'd; but upon gently raising the syphon towards the top of the water, it will exceed its former station, gradually flow out in drops, and emerge to the surface. Now, since the water, at first, kept the oil in the longer leg, suspended no higher, than it would have been by a cylinder of water, equal in basis to the orifice of the shorter leg, and reaching, in length, from thence to the top of the water, as may be easily seen in a syphon with legs sufficiently long; and since, when by raising the syphon, the Fig. 11.

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COROLLARY.

It follows from hence, that water may prefs as regularly upon an immersed body, when not restrained by pipes, as when included in them; we may, therefore, properly conceive a particular column of water, tho' it has no actual bounds, in an unlimited quantity of the same fluid.

The first part of our paradox holds equally true, whether the incumbent water be free or confined in vessels of any irregular shape. Thus, tho' the shorter leg of the syphon be fashion'd into a funnel, and filled with water, the oil in the other leg will resist the pressure thereof, so that the surface of the oil in the longer leg will rise but little above that of the water in the funnel. For farther confirmation hereof, we try'd the experiment in a syphon; in one leg whereof, a glass sphere was made to communicate with the upper parts of the same. Into the uniform leg of this syphon, we poured a proper quantity of oil of turpentine, and fill'd the lower and globular part of the other, with water; which proved insufficient to sustain the oil at a greater height, than if this leg also had been uniform: all the water in the spherical cavity, which fell not directly over the lower orifice thereof, being supported by the sides of the same. And when the irregular leg was fill'd with oil, and the other with water, the former fluid would not sustain the latter to an equal height, notwithstanding its excess of quantity contain'd in the concave sphere. To clear up this matter still farther, we poured mercury into the syphon *ABCD*, till it almost reach'd the bottom of the globular part, in the shorter leg, and to an equal height in the longer; then letting a proper quantity of water run into the longer leg, it impell'd the mercury in the shorter leg, till it more than half filled the cavity of the spherical part *E*, the tube *AB*, not proving sufficiently long to allow of its being quite fill'd; when, we observ'd the surface of the quick-silver *HG*, to lie as high as the different specific gravities of the two fluids required: so that notwithstanding the great weight of the mercury contain'd in the concave sphere *E*, no more pressed upon the subjacent slender part of the leg *EC*, than stood directly over its lower orifice: whence the water in the leg *AB*, appear'd press'd only as much as if the leg *CD*, had been uniform, and without the spherical cavity *E*. And thus, if the sphere had been made larger, a small quantity of water in the leg *AB*, would have somewhat raised a much greater weight of mercury than its own.

The second part of our present proposition is proved thus. We took three open, glass pipes, of the irregular figures in the scheme, and plunged them in a glass vessel of water, the two crooked ones standing very oblique to the upper surface thereof, and the higher ends of them all coming thro'



thro' particular strait holes made in a cork fitted to the vessel's mouth, whilst the water ascended to a certain height, thro' the lower orifice of every pipe; then pouring a considerable quantity of oil of turpentine thro' a funnel, into the vessel, the water was, thereby, impelled to the same height, of two or three inches, into the several tubes: whence 'tis apparent, that notwithstanding the irregularity of shape in the pipes, and the different wideness in each of their bores, the pressure of the contain'd water upon their lower orifices did not exceed that of five cylinders of the same, with each of those orifices for bases, and the perpendicular depth of the water and oil therefrom for their height. For had each of the pipes contain'd only such a cylinder, its upper surface would have stood at the same height; and if it did not depress the subjacent fluid as much as the external force tended to raise it, that excess of pressure must have impelled it higher. Since then the water rose to the same height in the several pipes, tho' two of them, being greatly inclined, contain'd much more water than if they had stood erect; by a like way of reasoning, we may conclude, that the imaginary plain cutting their immersed ends, sustain'd no greater pressure than that of a shorter, erect cylinder of water. For in all cases where the pipe is either inclined, or wider in one part than in another, the weight of the liquor it contains, is not wholly supported by the body contiguous to the lower orifice, but in some measure by the sides of the pipe. Thus, when oil of turpentine, in a slender tube, balances the pressure of the external water, if the tube be barely inclined to the sides of the containing glass, a considerable quantity of water will get up into the pipe; because the oil no longer resting wholly upon the water, but partly upon the tube, its pressure on the water is diminished, while that, continuing its force, impels up the oil, and intrudes it into the pipe; which being restored to a perpendicular position, the oil will again depress, and drive the water out of its cavity. We farther caused three pipes to be blown differently from the former, and filled their winding and irregular parts, as also their uniform stems, to a proper height, with oil of turpentine; we then plung'd them to a due depth under water, and there unstopping their upper ends, the surface of the oil they contain'd appear'd at the same height above the water, it would have done had the tubes been strait; as we found by a cylindrical one fill'd and immersed as the rest; tho' the quantity of oil in one of these pipes were, perhaps, thrice as great as that in the strait one. Hence we may fairly conclude, that the pressure of fluids contain'd in pipes, must be computed by their perpendicular height, how wide, long, or irregular soever they are.

Fig. 14.

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The learned *Stevinus* has a corollary from the preceding proposition, to this purpose. If thro' the upper surface of a concave cylinder, an open, cylindrical tube be erected perpendicularly, and the whole united cavity of both fill'd with water, the bottom circle will sustain a pressure equal to that of a cylinder of water, whose basis is that of the vessel, and height that of

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Fig. 15.

the tube therefrom. The truth of this paradox we attempted to try in the following manner. Having provided a latton vessel *ABEHGF*, furnish'd with a false wooden bottom *CD*, which was cover'd with a fine bladder, and oiled about the lower edges, to facilitate its rising from the rim of wood *HG*, that lay every where contiguous to the inside of the metal, and kept the water from passing between; to the middle of this loose bottom we also fasten'd a strong string, which came out at the orifice *A*. Thro' this orifice we then pour'd in water, which pressing the false bottom *CD*, help'd to tighten the vessel, and keep its parts close. When the whole cavity was fill'd, we fasten'd the upper end of the string *A*, to the beam of a balance, and gradually placed weights in the opposite scale, till they elevated the false bottom *CD*, from the rim *HG*, and consequently lifted the incumbent water, which soon ran down between them. Now, we had before-hand found what weight sufficed to raise the bottom *CD*, alone; deducting that, therefore, from the weight in the scale, and comparing the remainder with the weight of as much water, as the shallow cylinder *BECHGDF*, would, alone, without taking notice of that in the pipe *AB*, contain; we found the pressure upon *CD*, so vastly greater than could be attributed to the whole quantity of water made use of, had it been contain'd in an uniform cylinder of the same basis with our instrument, that we thought it some small confirmation of *Stevinus's* doctrine; tho' the paradox itself be greatly question'd by some men of learning*.

* 'Tis now an establish'd proposition, that fluids press not according to their quantity, but perpendicular altitude; and may be demonstrated thus.

Fig. 16.

Let *ABCD*FE, be a vessel full of water. Now the column *BF*, being heavier than the column *HG*, 'tis plain that if the vessel were open at *H*, the column *GH*, would ascend till it balanced the column *BF*: but since it cannot ascend at *H*, the water there must be press'd back by the obstacle, with a force equal to the weight of *BL*; for action and re-action are equal; and all pressure here being reciprocal, the water at *G*, will press against the bottom of the vessel with the same force. Now, as the weight of the column *GH*, is added to the former, the force of the water's pressure at *G*, will be the same, as if the column *GH*, were of equal height with the column *FB*, that is, as if it reach'd to *M*. And the same reasoning is applicable to all the other columns in the vessel; consequently, the bottom of it *ED*, will sustain the same pressure, as if the vessel were fill'd up to *NO*.

This surprizing property in fluids seems

applicable to considerable purposes; as it shews us how an exceeding small quantity of matter may be made to supply the place of one infinitely larger.

Thus, for instance, if the tube *Fig. 17.* *A'B*, of the inverted syphon *ABCD*, were close shut, and the small pipe *CD*, fill'd with water to *D*, that little quantity *CD*, would exert such a force upon the larger, *ABC*, that if the cover of the great orifice were perforated, the water would be made to fly out thereat, as violently, as if part of the little tube, *CD*, contain'd as much water, as the great one *AB*.

Again, if two cylindrical emboli were fitted to the tubes *AB*, and *CD*; the weights laid upon them would be balanced, provided they were proportionable to the capacity of the tubes; that is, suppose the capacity of the tube *AB*, was four times greater than that of *CD*, one pound laid upon the little embolus would balance the force of four pounds upon the greater. And these experiments might be varied infinite ways. See *Clark. Annot. in Robault. p. 41.*

PARADOX VII.

A body, immersed in a fluid, sustains a lateral pressure therefrom ; which increases with the depth whereto 'tis plunged.

Bend a small glass tube at right angles, into a longer and a shorter part ; Fig. 18. then, by suction, draw up oil of turpentine enough to fill the shorter, and to rise two or three inches high in the longer ; nimbly stop the orifice, and immerse the lower end of the tube under water ; so that the longer leg, E F, may stand perpendicularly to the surface A B, and the surface of the oil I K, but a little above the same A B ; when, if the tube be unstopp'd at E, the oil will continue as it was, and, consequently, there's a lateral pressure of the water against the oil contiguous to G ; for, nothing else could hinder the perpendicular pressure of the oil in the longer leg, from forcing it out ; as will farther appear, by gently raising the tube, in the same posture, towards the top of the water ; for, as the lower leg ascends, the oil will be driven out thereat, by the pressure in the other. And that the lateral pressure of the water against the lower orifice is, before the tube be raised, nearly the same with the perpendicular pressure of a cylinder of water, reaching from the orifice G, to the top of the water ; appears from the surface of the oil in the longer leg, always continuing a little above that of the water ; as would happen, were a pipe of an equal bore, continued from the orifice G, to the top of the water at H. But, farther, if the syphon be plunged deeper into the water ; the oil, by the lateral pressure thereof, will be gradually driven quite out of the shorter leg into the longer ; and if you immerse it still deeper, a cylinder of water will rise in the same longer leg, and sustain that of the oil, which is now no longer able to balance the lateral pressure of the water at so great a depth ; whence we find, that water has also a lateral pressure against water, which increases proportionably to the depth ; for the external fluid, could not, otherwise, impel that of the shorter leg into the longer, which it does by greatly exceeding the resistance of the whole cylinder of oil therein. But if the tube be now gently raised again, and the lateral pressure of the water against the immersed end thereby diminished, the oil will force the water first out of the longer leg, and then out of the shorter, till, at length, it leaves the orifice G, and emerges in drops to the surface. Again, when the oil in the tube is a balance for the external water ; if its shorter leg be kept parallel to the surface thereof, and moving backwards and forwards any way at the same depth therein ; the oil in the longer leg will appear at the same height ; from whence 'tis plain, a pillar of water, with a basis equal to the side of the body immersed, may be any where supported in the containing vessel ; and that tho' this imaginary column, as G H, be not included in any firm surface, its lower parts have, by means of the incumbent fluid, a lateral pressure, tending outwards against its imaginary sides ; and lastly, that this pressure increases in proportion to the height of the column of water above them. To conclude, if instead of

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holding the longer leg of our tube in a perpendicular posture, it be variously inclined to different angles with the surface of the water *AB*, so that the orifice of the shorter leg *FG*, may sometimes tend towards the top, and, at others, towards the bottom of the glass; the oblique pressure of the water depends so intirely upon its height above the orifice *G*, that the oil in the longer leg will still continue with its surface *IK*, but little higher than that of *AB*.

SCHOLIA.

Fig. 19.

1. We once attempted to discover the quantity of this lateral pressure by the following experiment. Having obtain'd a glass bubble *X*, with a slender neck, whose gravity fell very little short of that of water; and provided a large wide-mouth'd glass, exactly fitted with a cork, perforated in the middle, to receive, perpendicularly, a long, slender, glass pipe, that descended considerably below the cork, wherein, also, was another hole, near its edge, for a second small pipe to pass thro', whose upper end reaching only two or three inches into the air, was firmly closed; then the vessel being filled with water, and the bubble suffer'd to float thereon, the cork was applied, and well cemented to the mouth of the glass, so that nothing could pass in or out of the vessel, except thro' the long slender pipe; all the other vents being carefully stopp'd with cement. When things were thus prepared, and the bubble was made to float at a considerable distance from the deeply immersed end of the long tube, we poured water thro' its upper orifice *E*, which, for want of other vent, ascended back again into the same cavity; and if the water therein was only permitted to rise as high as *I*, for instance, the bubble *X*, would still continue a-float: but if the water rose to *K*, the bubble presently fell to the bottom of the vessel, and remain'd thereat while the water stood to the same height in the pipe *EF*; but if the height were diminish'd, by pouring some of the water off, the bubble would re-ascend. Hence we see, that the upper parts of water gravitate on the lower; and that, in a full vessel, the same thing happens, tho' the former be not directly incumbent on the latter, but at a considerable distance from the line of their direction; for the additional cylinder of water *KI*, acting like any other heavy body upon the water, causes the bubble *X*, to subside. And, since the air included in this bubble, was remarkably condensed, it follows, that the cylinder of water *KI*, pressed upon the subjacent fluid in the vessel; for otherwise the air in the bubble could not have been compress'd: and as this bubble floated on one side, at a tolerable distance from the pipe *EF*, and above its lower orifice *F*; 'tis evident, that the cylinder of water *IK*, presses as well laterally, as directly, upon all the bodies that lie beneath its surface. And thus every assignable part of the sides of the vessel, sustains a pressure proportionable to its depth under water, and its own magnitude: if, therefore, any part were so weak, as to yield to the weight of the cylinder *IK* (due allowance being made for the obliquity of the pressure) 'twere unfit to enter the composition of our glass. Even the cork itself, resists a pressure proportionable to the breadth

breadth of its under surface, and the height of the cylinder K I; and might, therefore, if not too strongly fasten'd, be rais'd thereby; so that if the cement should fail, the water would forcibly issue out of its vents. Thus, also, the stopple G, of the short pipe G H, may, by this means, be violently thrust out of its place, as will more satisfactorily appear, by stopping the orifice at G, with your finger, which will, there, be strongly press'd upwards.

2. To shew that the very oblique pressure sustain'd by the bubble, from the water in the long tube, differ'd not considerably from that of water placed directly over it, I set two such bubbles separately a-float in long cylindrical glasses of water, when, if they were plunged but to a small depth, they would rise again to the top; so that I was obliged to immerse them to a considerable depth, before the pressure of the incumbent water would sink them.

3. We may here, also, observe, how the pressures of such different fluids, as air and water, will communicate. For, having sometimes left a small part of our vessel A B C D, unfill'd with water, so that air remain'd between the surface L M, and the cork; the pressure of the water in the pipe from I, to K, would, if the cork were tight, cause the bubble to sink, as if the vessel had been wholly fill'd with water; the compress'd and imprison'd air above L M, communicating the pressure it receiv'd to the contiguous water.

P A R A D O X V I I I .

Water will as well depress, as support a body specifically lighter than itself.

We have proved, that the upper parts of fluids gravitate upon the lower, or bodies immersed therein. Now, when a body is unequally press'd, it must necessarily move from that place where the pressure is greatest, to where it is least; whence, if a quantity of oil, hath its upper surface alone, exposed to the pressure of water, and nothing be in the way to hinder its descent, the oil must necessarily be depress'd, and not buoy'd up thereby. To illustrate this by an experiment; take a small glass syphon E F G H, Fig. 20. with its shorter leg G H, 3 or 4 inches long, and parallel to the other E F; plunge the shorter into oil of turpentine, till it be quite filled, and the fluid reaches to an equal height in the longer, as from F, to I; then stopping, with your finger, the orifice E, of the longer leg, immerse them both a little under water: remove your finger, and as you gradually plunge the syphon deeper, the oil, in its shorter leg, will sink, and, at length, wholly rise into the longer; which sufficiently proves and illustrates our proposition.

S C H O L I U M

At once to confirm the present paradox, the seventh, and the second, I took a slender glass pipe O P N M L, and suck'd oil of turpentine into Fig. 21.
its

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its lower orifice L, till it reach'd to P, in the longer leg, in a level with L; and, stopping the upper end O, with my finger, I plunged the tube to a convenient depth under water; when, removing my finger, the external fluid first drove away the oil in LM, then depressed it from M to N; and lastly, wholly impelled it into the longer leg NPO; so that the oil was, here, evidently pressed not only laterally, but also downwards, and upwards.

PARADOX IX.

Notwithstanding the doctrine of positive levity, an oil, lighter than water, may be kept immersed in that fluid.

Since the surface of stagnant water is, physically speaking, horizontal; that part which presses against the lower superficies of an immersed body, must be deeper than that which presses upon the upper: if, therefore, the water incumbent on the upper part of a body, can be made to stand at a greater height than the rest of the fluid wherein 'tis immersed, so as to balance what presses against the lower; oil may be suspended betwixt two parcels of water. To reduce this theory to practice, I suck'd water into a strait, slender glass pipe TP, to the height of an inch, and there held it suspended, by stopping the upper orifice with my finger; then plunging the lower end of the tube, two inches below the surface of some oil of turpentine, deeply tinged with copper, whereby the oil, driving up the water, ascended to the height of an inch in the lower end of the pipe; when, I again closed its upper end, to keep both liquors suspended. In the next place, I plunged the tube into a glass of water, three or four inches below its surface; so that the water, upon the removal of my finger, was pressed into the lower end of the pipe P, to Q, impelling up the oil QR, to R, and the incumbent water to TS; at which stations, the three several parts of oil and water together, press upon P, as much as the external water upon the other parts of the same plain GH; yet the oil QR, ascends not, because the water RS, is kept from descending below it, by the sides of the tube; whilst its upper surface TS, stands higher than AD, that of the rest of the water: by which means, the incumbent fluid may be made to press upon the upper surface R, of the cylinder of oil, as forcibly as the water presses against Q, the lower surface of the same; whence the oil will remain suspended.

PARADOX X.

The ascent and flux of water in syphons, are explicable, without supposing a Fuga vacui.

Provide a cylindrical glass ABCD, both wide and deep, as also, a syphon FKG, with unequal legs, communicating with a long cylindrical pipe KE; to each leg of this syphon, fix a cylindrical glass I, and H, filled with water, as in the scheme, that the water may run from the higher glass

Fig. 22.

Fig. 23.

Hydrostatical Paradoxes.

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glass I, into the lower H; then, while the pipe E K, is held still, pour oil of turpentine, gently, into the cylinder A B C D, till it rise above the top of the syphon F K G; when, the oil pressing on the water, will raise it into the legs of the syphon, and force it out of the vessel I, into H: in which case, 'tis manifest, the water runs thro' the syphon, whilst the air has free admission thereinto, at the orifice E; so that nature's abhorrence of a vacuum, cannot be the cause of this flux. A just solution of the phenomenon may be had from considering the pressure of the oil, and the situation of the vessels; for, that fluid ascending higher than K, presses upon the surface of the external water, in the glasses I, and H, wherein the legs of the syphon are plunged, and forces it up into them; but as the excess of water in the glass I, proves an over-balance for the greater depth of oil in the glass H, the water is obliged to run from the shorter leg into the longer. Thus it happens in syphons; when once the water is brought to run thro' them, the air gravitates upon the surface of the external fluid, wherein their legs are plunged, and not upon that which they contain; and, therefore, tho' the incumbent air, stands a little higher over the fluid in the lower vessel, than in the upper, yet the water in the longer leg, greatly over-balancing that in the shorter, it must needs flow out at the latter. And the pressure of the external air being able to raise water to a much greater height than that of the shorter leg, the efflux will continue till some other cause put a stop thereto. But if the legs of a syphon exceed 35 feet in perpendicular altitude, the water will not flow thro' them; because the pressure of the external air is unable to raise water to that height. So, likewise, were an hole made in the top of a syphon, whilst water was flowing thro' it, the flux would presently cease; for the air, then pressing, as well within as without the syphon, would return the water in each leg to its proper vessel. To confirm this particular, I procured two syphons, the one of tin, the other of glass, each of which had in the upper part of its bend, a small perforation, which I could stop with my finger, or open at pleasure; wherein I frequently perform'd the experiment. And having once carefully stopp'd one of the above-mention'd holes, with oiled paper and cement, and found the water to run freely thro' the syphon; yet a puncture made in the paper, by the point of a needle, admitted air sufficient to make the water in the legs fall back, before I could possibly stop the needle quick enough into the hole to prevent it.

P A R A D O X X I.

The most ponderous solid body we know, immersed in water to a depth exceeding that of twenty times its own thickness, will float, if it be there fenced from the direct pressure of the incumbent fluid.

Let a deep glass vessel A B C D, be almost fill'd with water; wherein if a Fig. 24
small cylinder of brass E F, be immersed, till its upper surface is just cover'd, and then let go; it must, by reason of its greater specific gravity, necessa-

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necessarily fall to the bottom. But, were it placed on the plain IK, which lies at above the depth of nine times the body's thickness below the upper surface of the fluid; I assert that it would not descend, unless it were pressed downwards by the weight of the incumbent fluid. For brass being nearly nine times specifically heavier than water; the body EF, alone, would press upon the part F, as much as a cylinder of water, of an equal basis, but about nine times the height of the brass; and, consequently, the part F, will be no more depressed, than any other part of the plain IK. But since water gravitates on water, and also upon bodies heavier *in specie* than itself, if immersed therein; the brass-cylinder EF, wherever it rests in the fluid, must be forced downwards, both by its own weight, and that of the incumbent water. But were the incumbent water prevented from gravitating thereon, it follows, that the body would, at that depth, be kept suspended. And, in case the body rested, in like manner, upon the plain LM; the much greater pressure of the water on the other parts of the same surface, would impel it upwards, with a force proportionable to the difference of their respective weights.

Fig. 25.

And having found, by exact tryals, that the purest gold, the most ponderous body we know, is not quite twenty-times so heavy as an equal bulk of water; if this metal, also, were immersed into a greater depth than that of twenty times its own thickness, and there fenced from the pressure of the incumbent water; it must necessarily float. To confirm this by an experiment. Suppose the brass-body EF, to be the cover of a brass-valve, and the valve itself strongly cemented to an open glass-pipe OP; when, the body EF, being the lower part of the instrument, and every way detached therefrom, would fall by its own weight, if unsupported. Then to the button Q, fasten a thread, which, by being pulled upwards, may cause EF, to close the orifice of the valve: the valve being now plunged under water, to the depth of a foot, the cement, and the sides of the pipe OP, will prevent the fluid from pressing upon the upper part of the body EF; and, consequently, that part of the plain VW, contiguous to its surface, will be press'd upon only by the weight of the body EF, but in its other parts by that of the incumbent water; so that, letting go the string which held the body EF, close to the instrument, that body will remain suspended by the bare pressure of the water beneath it. And that this alone is the cause of the phenomenon, appears by gently raising the instrument towards the top of the water; for when it has ascended near the surface, suppose between the plains IK, and XY, the body EF, will fall down, and the water rise in the pipe OP, to a level with that in the containing vessel. But if the valve be at first plunged much deeper under water, before the string be relaxed, for instance, to the plain SR, the valve EF, will require a considerable weight, as L, besides its own, to disjoin it: but if the weight prove insufficient for the purpose, it may soon be render'd effectual, by raising the instrument, far less than before, towards the top of the water.

COROLLARY.

This experiment renders it unnecessary to fly to a *Fuga vacui* for a reason why two flat polish'd marbles, when they adhere, require a considerable force to disjoin them; they being pressed together by air of an immense height, after a like manner with our valve, and the instrument, here, by water; which requires the great weight *L*, to over-balance it. To as to force them asunder.



MATERIA MEDICA

HY-
Vol. II.

HYDROSTATICS

Applied to

ORES,

And to the

MATERIA MEDICA.

SECT. I.

Fossils, their medicinal virtues, whence?

AS the virtues of gems proceed, chiefly, from the metalline and mineral substances, that, whilst their original matter remain'd either fluid or soft, are incorporated therewith, and afterwards hardned into stones; so it seems very probable, that several boles, clays, earths, and especially minerals, &c. may be endow'd with considerable medicinal virtues, and, perhaps, greater than those of the finer gems; because herein are often found, more of the metallic and mineral parts, which, whilst *in solutis principiis*, might very plentifully insinuate themselves into the more open bodies, and there sit looser than in diamonds, rubies, sapphires, &c. which *Aqua fortis*, itself, is unable to penetrate and dissolve.

A new way of examining them, hydrostatically.

I, therefore, thought it might be of use to physicians, mineralists, and miners, to advance a new way of examining fossils; for tho' the method itself pretends not, directly, to discover more than one quality of the body it examines; yet that quality, being its specific gravity, is so considerable, that it may lead one farther than might, at first, be imagined.

Its foundation.

Reflecting that rock-crystal is of the most pure, and homogeneous kind of stones; I pitched upon this for the standard, whereby to adjust and ascertain the purity and simplicity of other mineral substances.

By

By carefully weighing several pieces of native crystal, clear and colourless, first in air, and then in water, we found its proportion to clear water, of the same bulk, as $2\frac{1}{2}$ to 1, nearly. Thus, if a hollow, metalline cube be exactly fill'd by one ounce of water; and afterwards, again, as exactly fill'd by a cubical piece of rock-crystal, that bulk of the stone will weigh about two ounces and a half. Some of my trials, indeed, made with tender balances, represented the proportion of these two bodies, with a very inconsiderable variation; but 'tis probable, that different pieces of crystal, tho' of equal bulk, may not have precisely the same weight: however, the difference is so small, that it may here be safely neglected.

And to prevent any scruple about the origin and nature of crystal, I shall add, that I procured some strong isicles, that had been fasten'd to vaults, &c. as bodies that would be acknowledg'd true stones, and yet to have once been in a liquid form: and having hydrostatically examin'd these concretions, their specific gravity appear'd to be little differing from that of crystal.

To apply this fundamental observation to the present occasion; when I would, with probability, discover whether the mere stony matter of a mineral body were mix'd with some adventitious substance of a metalline nature, or of some other mineral more ponderous than crystal; I carefully weigh'd it, as we said, in air, and then in water: when, if its proportion to water of the same bulk, exceeded that of $2\frac{1}{2}$ to 1, or 5 to 2, I conclude it probable, that the concrete had in it a portion of foreign matter specifically heavier than crystal, proportionably to the excess of the weight of the solid body above that of an equal bulk of water. To illustrate this procedure by example.

The magnet, upon account of its great hardness, is usually reckon'd among stones; but having observ'd a particular kind thereof, to be apparently more ponderous than common stone, of equal bulk, we weigh'd them in air and water; and found the specific gravity of some of them so far to exceed that of crystal, or marble, that we concluded they contain'd a considerable quantity of metalline matter, which, by collateral experiments appear'd to be of a ferruginous nature.

Emeri is commonly reputed a mere stone; but finding its weight in water considerably to exceed that of an equal bulk of crystal; for it was to that liquor nearly as 4 to 1; I conjectured, that it contain'd a metalline substance; as afterwards, by proper trials, I found it did.

By the weight, also, of *Lapis Hamatites* on my hand, I conjectured, it largely partook of a metalline ingredient; and presently I discover'd that iron or steel was contain'd therein. And, in general, I have frequently suspected different bodies to be of a metalline or mineral nature, and was seldom mistaken in my conjecture; tho' they have commonly been thought to contain no such substance. The same thing I, likewise, found true, even in granats and *American talc*, from which I have extracted metalline parts.

But we must here set down some preliminary observations. And first, if a proposed fossil be much lighter than an equal bulk of crystal, it is

Sf 2

almost

Examples to illustrate the doctrine.

Preliminary observations, with regard to fossils.

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almost certain it cannot be a metalline ore; and this negative proof is here usually stronger than the affirmative kind. Thus, for instance, when I find that jet, tho' a fossil, has a far less specific gravity than crystal, I conclude it to be no metalline body. The like determination I make as to fossil-amber, *Sulphur vivum*, common sulphur, *English* and *Venetian* talc, and some other firm concretions, which are dug out of the earth: among which, I also rank black-lead. For having found its weight to that of water as 1.86 to 1, I afterwards discover'd it to be not lead-ore, but a particular kind of mineral, and near allied to a sort of talc that I have met with.

2. We must distinguish between the several uses to be made of fossils by men of different professions. So that if a fossil be found very little heavier *in specie* than crystal or marble, it may, possibly, have a metalline or mineral portion, which, however small in quantity, will perhaps entitle it to the esteem of a jeweller, physician, or chymist. But if this excess of its specific gravity be inconsiderable, the fossil itself will prove of little price to a mineralist; for if it be poor in metalline substance, it may chance to be hardly worth the working.

3. But tho' the great ponderosity of a fossil generally proceeds from a proper metalline substance embody'd with the other parts thereof, which shews it not to be a mere stone; yet this, alone, is not a sure sign that the mineral part is properly metalline: and, therefore, where any doubt arises, we should have recourse to collateral signs. For, besides metalline ores, properly so call'd, there are other fossils, which, tho' of affinity to metals, may be distinguish'd from true metalline ores; such, for instance, are antimony, bismuth, *Lapis Calaminaris*, and marcasites. But there will not, perhaps, occur many cases, wherein we need to have recourse to collateral signs, to shew whether the mineral part of a fossil be, in a strict sense, of a metalline nature, or not. For such semi-metals are most commonly found either in veins, masses, or great lumps of their respective kinds; and easily discover to what species they belong. I have, indeed, receiv'd a lump of matter from *Devonshire*, wherein I found some antimony mix'd with lead, which was the predominant body. But such mixtures occur so seldom in *England*, that our way of estimating ponderous fossils, will, nevertheless, be useful on most occasions.

4. Two different estimates may be made of the specific gravity of ores; one, when the body proposed is weigh'd in its natural state, that is, as it comes out of the earth, accompanied with the spar, or other heterogeneous matter, that firmly adheres to it; and the other, after it has been beaten small, and separated from heterogeneous substances by the help of water, after being skilfully agitated, wherein there is a remarkable difference in weight between these and the genuine parts of the ore; which being thus sever'd from the rest, are call'd washed. 'Tis sometimes, also, very proper to prepare the ore by roasting it once or more, or by keeping it for several hours in a competently strong fire; as is usually practis'd in preparing copper-ore; especially if it be stubborn. I purposely mention these

two states, wherein the weight of an ore may be taken, because I have observ'd, that, in several cases, 'tis of great importance to distinguish them carefully. For several ores, which, in their natural state, have too little specific gravity, to make them judg'd worth the charge of being wrought, may yet, after they are prepar'd by water and fire, afford a metalline portion, so ponderous, as to allow one to suspect they contain either silver or gold. I remember, that a piece of lead-ore, brought from *Ireland*, seem'd to me so light in the lump, that I thought it deserv'd not to be wrought for lead; yet, afterward, upon trial, it appear'd so well stor'd with particles of silver, that I encourag'd the owner of the mine to work it.

5. There is one kind of minerals that I have observ'd to impose upon men so frequently, that I shall take a particular notice of it. For, not to mention examples from travels, and voyages, I have myself met, with numbers who had great expectations from marcasites. And I have receiv'd not only from places near home, but even from the *Indies*, fossils, whereof my opinion was desir'd; which I found to be only marcasites. Many of these fossils have two qualities, that make them fit to delude the vulgar; for, they shew a multitude of shining streaks, resembling gold; or have, at the same time, a weight equal to that of true metalline ores. Marcasites, then, being thus adapted to impose upon the unwary, I have had much ado to undeceive some persons, as to the pleasing confidence they had entertain'd, that these promising fossils were lumps of rich gold, or silver-ore. Wherefore, since their weight, the criterion of minerals, is one of the two things that delude so many, I shall give a few instances of the specific gravity of marcasites, to make it appear, that some of them are, bulk for bulk, far more ponderous than true metalline ores. And, indeed, the great ponderosity of fossils, has several times occasion'd me to determine, before trial, that they were marcasites. But, to prevent being impos'd on, by this, or the like specious substance, it is but placing it in a strong fire, and blowing, now and then, with a pair of bellows; for, by this means, the sulphur wherewith marcasites abound, will take fire, and burn with a flame for the most part blue, like that of common sulphur: so that once, by distillation, in a close vessel, I obtain'd four ounces of good brimstone, from three pounds of these stones. And, if when this fossil ceases to flame, and smoke, it be taken out of the fire, and suffer'd to cool, it will be depriv'd of all its gaudy appearance, and turn'd to a brittle, blackish substance, very different from that of a proper metalline ore. Marcasites, however, may be look'd upon as a kind of metalline bodies; for, I have found several of them to contain, not only particles of copper, but also of iron, or steel; for, after calcination, applying the pulveriz'd remains, to a vigorous load-stone, great multitudes of chalybeate corpuscles quickly adhered thereto. And, I remember, in a catalogue of the fossils of *Mishia*, published by *Kentmannus*, under the head of *Pyrites*, he places several marcasites; some whereof contain'd copper, others silver, others gold, and others, again, both silver and gold. Having presented a very fine marca-

To distinguish
marcasites from
metalline ores.

Put it into
a strong fire
and blow it
with bellows.

When the
fossil ceases
to flame, and
smoke, it will
be depriv'd of
all its gaudy
appearance, and
turn'd to a
brittle, blackish
substance.

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sire to the overseer of one of the emperor's best mines, he presently examin'd it, by a peculiar way, with hopes to find in it some gold or silver; but, instead thereof, obtain'd a parcel of running mercury, which he presented me. Notwithstanding the caution here given, I do not deny, but that 'tis possible, for a skilful artist, to make a profitable use of marcasites, either by fixing the volatile gold, or silver, to be found in some of them; by graduating silver by their means, &c. But that for which I much more value them, is, what I desire should be well remark'd, that I am persuaded, if they were simply, and, of themselves, but dextrously handled, they may afford very noble and uncommon medicines, particularly excellent in continual fevers; tho' their usual operation be almost insensible, unless by their good effects.

Directions to find the best flux-powders, for assays.

6. I must not here omit, that tho' many, who make trials of ores, value their own flux-powders, or such as are cry'd up by others; yet they commonly seem to expect nothing from those they prefer, more than that they should best facilitate the fusion of the ore; as that which being once done, the metalline part will separate by its own weight, or, as it were, spontaneously. But yet, having purposely examin'd the matter more nicely, and compared the quantities of metal obtain'd from two portions, of equal weight, of the same ore, we found those proportions considerably differ'd; tho' that which yielded least metal, was flux'd down with a costly, and well-adapted powder. And, I doubt not, but from other metalline ores, a greater quantity of pure metal is obtainable, by some flux-powders, that are but little employ'd, or known; than by others, that are much more famous and common. Thus two equal portions of the same lead-ore, clear of spar; being, the one reduced with a due weight of nitre and tartar fulminated together, afforded me a much less proportion of malleable lead, than the other, by means of half, or a quarter the quantity of filings of iron. And, to instance in a much more precious mineral than lead-ore, I try'd the like with some ounces of good native cinnabar, finely pulveriz'd; by adding to one half a fix'd alkali of tartar, and to the other, a different flux-powder, we obtain'd from the former, twice as much mercury, as we did from the latter, tho' distill'd with a fix'd alkali even of a mineral nature.

Directions relating to the management of the hydrostatical balance.

7. Before we proceed farther, 'tis necessary the reader be acquainted with the method of weighing heavy bodies in water; which being liable to several contingencies, 'tis proper in this place to take notice of the more considerable ones.

But, first, to give a general notion of the thing, I desire the figure of the hydrostatical balance itself, with all its apparatus, may be view'd; wherein A A, is the beam of the balance, B B, the dishes, C C, the frame whereon the beam is suspended; with D, its sliding socket; E, the arm; with F, a pulley; over which, and G, another pulley passes H, a line fasten'd to I, a moveable weight; by which the beam is elevated and depress'd. K, is a hair wherewith to suspend L, the body to be weigh'd in the water of M, the glass cistern. N, is the bucket for liquors, O, the box of grains, P, the

Fig. 26.

the forceps, wherewith to manage them, Q, the pile of weights, R, the handle of the balance, and SSSS, the table.

The solid body to be examin'd, should be ty'd about with an horse-hair, of a competent length, and fasten'd at its other end, to one of the scales, of an exact and tender balance; that the body, having been carefully weigh'd in the air, may be immersed in a proper vessel, almost full of fair water, so as to hang freely therein, and be on every side encompass'd with the fluid. This done, put weights into the opposite scale, to balance the body hanging in the water; when the beams of the scale will lie horizontal, and the weights employ'd give that of the body in water. Deducting, therefore, this weight from the weight of the same body in the air; by the remainder, that is, the difference of the two, divide the whole weight of the given body in air; and the quotient will shew the proportion in specific gravity, between the solid, and an equal bulk of water. To illustrate this by an example, we put a fine piece of white marble, which seems the most pure of any common opaque stone, into an exact balance, and took its weight in the air 1169 grains; then a horse-hair being ty'd about it, and the other end of the same hair, fasten'd to one of the scales; under which, at a convenient distance, was placed a deep glass, almost full of fair water; we suffer'd the stone to hang freely beneath the surface of the fluid; and, in the opposite scale, placed weights enow to bring it to an equilibrium with the other. These weights amounted to 738 grains, which gave us the weight of the marble in water; which being subtracted from the weight of the same stone in air, there remain'd 431 grains for the weight of the equal bulk of water: by this remainder, we divided the weight of the marble in air, viz. 1169, and found the quotient to be 2.7 nearly, for the specific gravity of white marble to water.

'Tis here manifest, from the nature of the thing, that the body propos'd to be weigh'd, must be heavy enough to sink in water; otherwise its weight in the fluid cannot be significantly deducted from its weight in air; but if there be occasion to weigh in water, a body lighter *in specie* than water, it may be done, tho' with difficulty, by joining to it another body, that is able to sink it.

8. A horse-hair is made choice of for hydrostatical operations, because 'tis thought to be of the same specific gravity with water; and tho' that be not strictly true, yet a horse-hair is fitter to be employ'd in these trials, than any other string I know of; and its weight usually differs so little from that of water, that the difference may be safely neglected: but if the solid proposed, be too heavy to be sustain'd by a single hair, several of them may be twisted together.

9. I sometimes had occasion to examine bodies hydrostatically, which, by reason of their roundness, or other inconvenient figure, could not well be fasten'd to a hair; in which case I used hairs interwoven into a kind of a small hoop-net, the meshes whereof would not suffer the body to slip thro' them.

10. And

statical trial is made, by means of a horse-hair, there must be put into the opposite scale, as much of the same hair as seems equal to what sustains the body; and appears above the surface of the water; for that fluid only takes off the weight of as much of the hair as is immers'd in it; so that its unimmers'd part adds to the weight of the pendulous solid; and, therefore, ought to be compensated by an equal weight in the other scale.

11. When I reserv'd a balance for hydrostatical trials, I found it convenient, on several occasions, to take off one of the scales, with the strings belonging to it, and substitute, in its stead, a piece of metal, of a conical, or other convenient figure, and equal in weight to the opposite scale; as also, at the same end of the string, to fasten one end of the horse-hair that tied the body to be weigh'd in water. And sometimes, likewise, when I did not take off one of the scales, I caused it to be perforated in the middle, without lessening its weight; that so the body to be immers'd, might hang perpendicularly from the centre of the scale.

12. Care must be taken, that the body to be examin'd, hang freely in the water, so that no part of it, any where, touch the bottom, or sides of the vessel, or reach above the upper surface of the water it contains; for, when any of these circumstances are neglected, the true weight of the solid is somewhat alter'd; and if any part of the body, or horse-hair, 'tis tied with, appears above the water, it adds to the weight thereof. And, as nothing but the water is to touch the hanging body, so no part of the fluid must touch the scale from whence it hangs. I have several times observed, that immers'd bodies have been concluded to weigh more in water than they really do, from a neglect in observing, that if the string be too short, or the vessel too full, the vibrating motion of the balance will, at one time or other, carry down the scale to which the suspended body is tied, so as to make some part of it touch the surface of the water; a few drops whereof will readily stick to it; which, because they adhere to its under part, may lie conceal'd from a heedless eye, sensibly add to the weight of the scale, and make the body be thought heavier than it naturally is: an error very prejudicial, when exact experiments are required.

13. But the most usual cause of mistakes, in hydrostatical trials, proceeds from hence, that the given solid, and the string whereto 'tis fastned, carry down with them particles of air; which, perhaps too, may extricate others that lay conceal'd in the pores of the liquor, in the form of bubbles. These aerial particles fasten themselves to the little asperities they meet with, on the surface of the immers'd body, and like so many little bladders of air, endeavour to buoy up the body they adhere to; and, therefore in proportion to their number and bulk, lessen the due weight of the immers'd body, in water. Great care, therefore, must be had, in nice experiments, to shake the string, and warily knock the body against the sides of the glass, that the adhering bubbles may thereby be displac'd, and emerge to the top of the water. And, on some occasions, this caution should be used more than once in the same experiment; because I have often ob-

serv'd,



serv'd, that all the immerg'd body has been freed from the first bubbles that appear'd about it; others succeeded in their stead, before the experiment was finish'd.

I have been the more circumstantial in explaining the way of weighing bodies in water, because experience hath taught me, that the practice of it is not so easy as might, at first sight, be expected.

Having obtain'd the weight of the given body, first in the air, and then in water, according to this method; it will not be difficult to discover, practically, the proportion in weight between the solid and the fluid. The foundation of our practice, is this theorem of *Archimedes*. "A body heavier than water, weighs less in that, than it does in air, by the weight of an equal bulk of water." Whence we deduce a rule for our present purpose. For if the weight of the proposed body, whilst it is every way surrounded with water, be subtracted from that of the same body in air, the remainder gives the aerial weight of a quantity of water equal in magnitude to the solid; so that having the weight both of the solid and fluid, divide the greater by the less, and the quotient compared to an unit, will be the antecedent of the proportion sought between the solid and the water. And this rule holds equally, *mutatis mutandis*, for other liquors, as well as water.

To apply this doctrine first to ores.

Many learned men have been of opinion, that, properly speaking, there were no such things as gold-mines; and I confess myself to have been long kept in suspense as to this particular: for, notwithstanding all the inquiry I could make after mines, wherein gold was the predominant metal, I could not find a man who would say he had seen any. At length, I met with some ore, presented to King *Charles II.* which I judged to be genuine; and also received from an unknown hand in the *East-Indies*, another piece, in the clefts whereof, and a little beyond them, there appear'd some lumps, wherein, by their colour and other signs, it seem'd manifest that gold was the predominant metal. But the largest piece, and that which was best furnish'd with metalline parts, weighing about an ounce and a quarter, contain'd so great a proportion of spar, that its specific gravity to water, was but as 2.21 to 1. Its metalline portion, however, seem'd to be all gold; for there appear'd no sign of any other metal therein, nor, in some lesser pieces that I receiv'd along with it. Its spar, or stony matter, wherein the true ore is immediately lodg'd, did not resemble the spar of lead-ore, or that of any other of our *English* metals; but seem'd, at first view, to be a kind of white marble, with a dash of yellow. And, upon trial, I found it differ'd still more from the spar of lead-ore, which, with us, is usually white, and almost semi-diaphanous. For our spar of lead-ore is often so soft, that it may easily be cut with a knife; but the sparry portion of this gold-ore was a solid stone; and so hard, that when struck against a piece of steel, it would yield sparks of fire. And, farther, the spar of lead-ore will easily, and suddenly, calcine to a kind of lime; but our golden-spar, tho' kept for some hours red-hot in a crucible, did not appear to be at all calcined

The hydrostatical balance applied to ores, and first to gold-ore.

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cined. The spar of lead-ore will, also, dissolve in some acid menstrua, and even in distill'd vinegar: yet I could not find that this golden spar, when kept for several hours in spirit of salt, *Aqua fortis*, or *Aqua regia*, was manifestly wrought upon by any of them.

A piece of spar, that seem'd scarce to contain any gold at all, being hydrostatically examin'd, we found its specific gravity to water, as 2.65 to 1.

These observations, imperfect as they are, may, perhaps, be serviceable to such as have never seen true gold-ore; and, in particular, to those who search after gold-mines in *Jamaica*: where, as general *Venables*, the person who conquer'd it for the *English*, inform'd me, at his return from thence, the *Spanish* governor of the island, whilst his prisoner, confess'd there was mineral-gold; tho' the *Spaniards*, for want of workmen, could not dig deep enough in quest of it.

By what I have said of the true gold-ore, I would not have any one discouraged from seeking for gold in other metals; because, I know, it may sometimes be found blended with predominant minerals, as appears from the copper-mines of *Cremnitz*, whence a considerable quantity of gold is yearly obtain'd. And I have seen an *English* tin-ore, wherein there lay, in little cells, a number of small leaves or chips of gold; and tho' the tinmen, being unable to separate them to profit, usually melted both the metals together, and sold the mass for mere tin; yet the owner of the mine assured me, that one of his workmen advantageously employ'd his own children to pick the gold out of the ore, when skilfully broken. There is, also, a place in *Scotland*, where, over a lead-mine, near the surface of the ground, they often find large grains of native gold free from spar; some of which I thought worthy to be presented to that curious examiner of ores, *Prince Rupert*; and I still have a piece of native metal by me, that came from the same place, in weight above forty grains, and wherein gold is the predominant metal.

I have found a grain of natural *Scotch* gold, without any adhering spar, to weigh three drams, twenty-one grains; another grain of the same, that had a little spar sticking to it, weigh'd three drams, three grains; so that, allowing for the heterogeneous substance, it weigh'd about three drams. And, lastly, a grain of *Scotch* gold weigh'd, in air, 43 grains; in water, $39\frac{1}{2}$ grains; whence its proportion to water is as $12\frac{1}{2}$ to 1.

It often happens, that, among the lesser grains of gold, properly call'd sand-gold, there are pieces, singly big enough to be ty'd about with a horse-hair, and weigh'd in water; to which, therefore, our hydrostatical way of examining ores may be usefully apply'd. For, since pure gold is to water of the same bulk, as 18 or 19 to 1; it will readily appear, whether the fragment propos'd be perfectly pure, or not.

We frequently receive from the maritime parts of *Africa*, small fragments of gold, in the form of sand or gravel, which seem to have been wash'd away from hidden veins by the violence of the waters; to estimate the genuineness and degrees of purity whereof, our hydrostatical method

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of examination may be of singular service. For the specific gravity of perfectly refined gold to water, being known; 'tis, thence, easy to examine its degrees of fineness. And, when once the true specific gravity of a parcel of sand-gold is known, together with its degree of fineness, gain'd by collateral trials; this specific gravity may be used as a standard, whereby to try the fineness of other parcels of the like native gold: whence the fraud of the *Negroes* may, in this case, be prevented; who often clandestinely mix with the right sand-gold, filings of copper, or brass; whose specific gravity is not half so great as that of fine gold.

Aqua fortis, also, would immediately discover this fraud, which will not work upon gold; but corrodes brass, and thereby gains a colour betwixt blue and green; tho' if much silver be naturally mix'd with the gold, this proof, by *Aqua fortis*, will require skill: and, therefore, good spirit of urine may be substituted in its stead, occasionally; for this will readily work upon filings of copper or brass in the gold, and gain from them a fine blue colour. And, to hasten the operation of this liquor on filings of brass, or copper; 'tis but spreading them thin, upon a piece of white paper, and moistening them thoroughly therewith, that the air may promote its action; for, by this means, I have often produced, in a few minutes time, a pleasant blue colour upon the paper. Spirit of hart's-horn, or other volatile alkalies, or even stale rank urine, will serve in time of need, and yield the same phenomenon.

But if a solid substance be desired for this purpose, common sal-armoniac may be used, by making a strong solution thereof in water, and applying it as any of the former; for this also, will soon gain a greenish colour, or a blue, from brass or copper, and without affecting the gold.

I have observ'd such a variety of appearances and disguises in metalline bodies, and other minerals, that I would advise the searchers after mines to have their eyes always open, and ready to take notice of any unknown, or uncommon fossil; and not neglect to weigh it in their hands; and if it seem to exceed the weight of crystal, or marble, to examine it hydrostatically. For there are in *England*, and several other countries, useful fossils, usually overlook'd by the unskilful. I have found, in this kingdom, eagle-stones even upon the high-ways; and some other minerals that were not suspected to be of *English* growth. And, I remember, that passing by a potter's work-house, and viewing the ground attentively, I made a discovery of manganese, a mineral very proper for glazing and colouring of earthen vessels, which the potter, afterwards, gladly used for that purpose. The same person shew'd me a place that contain'd great store of a fossil substance, unknown in *England*, that ran very far under ground, like a vein of metalline ore; by some easy trials I found it abounded with vitriolate salt, much more than any marcasite I had examin'd in the form of stones: whence I concluded, it might more profitably be employ'd to make vitriol, than the marcasites used for that purpose in the works at *Deptford*, or elsewhere, in *England*.

All minerals should be carefully examined, and, if ponderous, hydrostatically.

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How to examine earthen, or soft substances, hydrostatically.

I remember, also, that a mineral of an odd appearance, being sent me, as unknown to the miners who dug it up; I found it to be the ore of bismuth, tho' the vein that afforded it was very small. But the chief reason for which I give this particular caution, is, that an application of our general remark upon the specific gravity of fossils, may be extended to a new and considerable use; for we need not confine ourselves to examine only those fossils, whereof we can obtain pieces sufficiently large to be singly weigh'd in water; because, not to mention the minerals that may be found useful to the physician, the druggster, or the mineralist; the ores of metals may be often found disguiz'd in the form of earth, or mud, that is easily dry'd; which fossils, tho' unfit to be immediately suspended by a horse-hair, may conveniently be examin'd by means of a glass jar-bucket, of a known specific gravity: for, this vessel being almost fill'd therewith, and that matter, carefully counterpois'd in air, and made thoroughly wet with water, and the whole warily let down in the same fluid, and there kept suspended by a horse-hair, tied to a tender balance, the difference between the weight of the mineral, and vessel in air and water, will be obtain'd; the weight, therefore, of the vessel, in water, being subtracted from that difference, will give the weight of the fossil in water, and, consequently, its specific gravity in water.

And to shew that this method wants not proper subjects, whereto it may be apply'd; we are told, that one of the best sorts of *Swedish* iron is often found in the form of a red mud, at the bottom of lakes, or other stagnant water: and I have observ'd *English* okers to be richer in iron, than some ores of that metal. And, an experienc'd writer, upon the gold and silver mines of *America*, takes notice, that gold, itself, is often found disguised in a reddish earth. *Vannochio*, also, a famous *Italian* mineralist, tells us, that a reddish sort of earth, sometimes, contains the richest metals. I, myself, have observ'd some finely figur'd crystals, to grow in a red earth. And, lastly, a traveller presented me with a certain earth, which he affirm'd to come from the diamond-mines; and this, also, I found to be red.

Colour'd sands, and gravel.

But a mineralist may make a more advantageous use of our hydrostatical bucket, by employing it in weighing colour'd sands and gravel. And, to shew how apt we are to over-look sands, for want of trying them by weight; I have, sometimes, seen a sort, slighted as worthless, which being wash'd, and view'd thro' a microscope, appear'd like a heap of small granats, and, perhaps, were really so. But what is more extraordinary, having observ'd, that the black sand, usually employ'd to dry fresh writing, seem'd, manifestly, heavier than the common; I examin'd it by the hydrostatical bucket, which gave us its specific gravity to water, as about 4.6 to 1; and, by melting it down, with two or three parts of antimony, and casting it into an iron cone, I found it of an iron nature; and, by applying it to a load-stone, to be far richer in metal, than any of our *English* iron-ores; for, at least, seven parts in eight, would easily be taken up by the magnet. But such observations as these, are not my chief motives to recommend the examination of sands and gravel to the mineralist.

'Tis

'Tis well known, that *European* traders yearly bring large quantities of gold from the coast of *Guinea*, which is wash'd, or pick'd out of the sand. And, even in *Europe*, there are rivers whose sand is enrich'd by grains of gold. For this, the *Tagus*, that runs by *Lisbon*, and *Pactolus*, were famous among the ancients. An industrious chymist assur'd me, he got gold, with profit, from the sand found on the banks of the *Rhine*: and there is a little river in *Savoy*, on the banks whereof, after a land-flood, I saw poor people employ'd in seeking for grains of gold. Some trial, also, that I caus'd purposely to be made, confirm'd me in a persuasion, that the sands, of many places, if skilfully treated, by chymistry, may afford much more gold than is got from them in form of grains. For, besides that there may be many particles of gold, so very minute, and closely fix'd to grains of sand, as not to be taken notice of by the eye, or prove separable by washing or picking; there may, as I conceive, be many small portions of that metal incorporated with the body of the sand; which a skilful artist, by the help of proper additions, might separate to good advantage; especially, if with litharge, or minium, the sand be first reduced to a glass; and then care be taken to get the volatile gold, by giving it a pure body, fit to retain, and fix it; such, for example, is fine silver; out of which, I remember, we separated, by quartation, from a crucible full of vitrified sand, and two or three fluxing materials, of small price, sixteen grains of pure gold.

Volatile gold is, I know, by many learned men, look'd upon as a fictitious thing; yet I have, by the help of an addition, inconsiderable as to bulk, and more so, as to weight, without a naked fire, and in a glass retort, sublimerd crude gold; sometimes in the form of a yellow salt; and sometimes, when the operation succeeded better, in that of thin crystals, prettily shaped, glossy, and as red as rubies: this by the bye. It may, perhaps, be more useful to searchers after rich fossils, to observe, that when they meet with sands, earths, mineral fragments, &c. whose specific gravity but little exceeds that of crystal; and yet, by the place wherein they are found, or by other tokens, give hopes of their containing particles of gold, they should not hastily reject them. For, having sometimes discover'd corpuscles of iron, and steel, in a vast variety of fossils, and disguizes, I suspect that some golden particles may lie conceal'd in several bodies, which are thought to contain no metal; and still more in metalline ores.

But those who would apply hydrostatics to ores in general, should endeavour to procure specimens of different mines, especially if they be found in the same country; and either by trial, or strict inquiry, inform themselves what proportion of the respective metal they contain. For these portions of ores, and minerals, being carefully weigh'd in air and water, and their specific gravities thereby known, may serve for a kind of standard, wherewith to compare, hydrostatically, the metalline portions contain'd in other parcels of ore, of the same species. Thus, for instance, all our *English* lead-ores, worth taking notice of, may be divided into

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*Ores in general,
and that of lead
in particular.*

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As to the ores, presented me from different countries, I have inserted their specific gravities, in the table annexed to this treatise; supposing it may be of use, in making a conjecture, with other concurrent circumstances, whether a mine may be advantageously wrought; but, concurrent circumstances are, by no means, to be neglected.

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The hydrostatical balance applied to the *Materia medica*; and first, to the *Lapis Hamatites*.

WE come now to the second part of our design; that is, to apply the hydrostatical balance to the *Materia medica*. And first, there is a deeply red, and opaque hard mineral, call'd, in the shops, by the name of *Lapis Hamatites*, which, tho' little used by our *English* physicians, is, in several places abroad, highly esteem'd, and, in my opinion, not without cause; particularly for the somniferous quality to be observ'd in some of its preparations. Of the *English* sort of this fossil, we weigh'd, in a tender balance, a piece that chanc'd to amount to three ounces, two drams $\frac{1}{4}$, first in the air, and then in water; and found its proportion to that liquor, as 4.15 to 1; that is, almost twice as heavy as a mere stone of equal bulk. So great a weight, confirm'd me in the conjecture I had made, that this stone contain'd a large proportion of metalline substance. I therefore sublimed it, when finely powder'd, and diligently mix'd it with a double weight of sal-armoniac, and found it, as I expected, very astringent upon the tongue, like several preparations of iron; and, for a farther proof, having put less than a grain of it into a spoonful, or two, of a strong infusion of galls, it immediately produced a black and inky mixture.

To the *Lapis Lazuli*.

Lapis Lazuli is sometimes used by *European*, but more frequently by the *Arabian*, and other eastern physicians; and, particularly, as an emetic; which faculty I thought it possess'd, upon account of some metalline ingredient; and, accordingly, having hydrostatically examin'd a piece, that was judg'd moderately rich, we found the proportion thereof to an equal bulk

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bulk of water, as 3 to 1; which argues, that notwithstanding its briskness in operation, it contain'd a much smaller proportion of metalline substance, than *Lapis Hamatites*, or several less violent minerals.

The load-stone is applicable to medicinal uses, and may have sensible operations upon the human body; for which reason we examin'd it hydrostatically, and found the weight of a lump thereof, that I judg'd to be either *English*, or *Norwegian*, was, in proportion to water of the same magnitude, as 4.93 to 1.

Lapis Calaminaris, is frequently used in physick, especially by chymists, to absorb acidities; for which purpose I prefer it to several more famous drugs; but though 'tis usually employ'd only as an external remedy, yet some uncommon chymical preparations of it, make me think it deserves to be further examin'd. A famous empiric candidly assured me, that the medicine he with great success made use of, against fluxes, was nothing but pure, and well-ground *Lapis Calaminaris*; hence I readily conjectur'd, that it participates of a metalline nature: which may also be argued from its operation upon copper, which it turns into brass: wherefore, weighing a piece of this fossil, we found it to water, as 4.69 to 1.

Let it be here observ'd, that tho' when a hard fossil is found to be much heavier than crystal, of the same bulk, 'tis very probable, the solid concrete contains a considerable portion of some metalline, or ponderous mineral body; whence its good or bad qualities, with regard to the animal œconomy, may, probably, be deduced; yet 'tis very possible, for a fossil to be endow'd with medicinal virtues, or noxious qualities, on account of its extraneous matter, tho' its specific gravity but little exceeds that of crystal, or the difference be inconsiderable; for a very small proportion of adventitious metalline, or mineral substance, if it be of a violent nature, may, in some cases, diffuse it self thro' the rest of the mass, and impregnate it with active qualities; as we shall see hereafter.

But further, this method of examination, may assist us to guess, with probability, whether a mineral body be of a stony nature, or not. Thus coral, for instance, is, by some, thought to be a plant; by others, a *Lythodendron*; but, by the greater number, a precious stone; in which difference of opinions, its specific gravity may prove of considerable service. We, therefore, weigh'd a piece of choice, and well-colour'd red coral, first in air, and then in water, and found its proportion, to an equal bulk of the latter, to be as 2.68 to 1: whence their opinion, who take it for a stone, seems most probable; since its specific gravity exceeds that of crystal.

Pearls, because of their hardness, are often thought of a stony nature. A monstrous one being presented me, that was taken out of an oyster, its shape irregular, its magnitude extraordinary, and its aerial weight amounting to 206 grains; I weigh'd it in water, and found it to be thereto as 2.51 to 1: so that its specific gravity nearly equal'd that of crystal.

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Calculi humani.

Calculi humani are frequently supposed to be true and genuine stones; and, indeed, the great hardness of several of 'em, may not improperly entitle them to that appellation: however, I should rather call them animal stones, than simply stones; which, by our method, may be easily distinguish'd from those, and other the like hard concretions, found in the bodies of animals. For, having a considerable number of these animal stones; I found, not only by chymical analysis, that they belong'd not to the mineral kingdom; but, by an hydrostatical examination of several of them, I found they differ'd greatly in specific gravity from true fossil stones; one of them, weighing above six drams and a half, was found to be in proportion to an equal bulk of water, as 1.76 to 1; and another, that weigh'd four drams and above a half in the air, being also weigh'd in water, appear'd to be thereto as 1.69 to 1.

I mention these stones as belonging to the *Materia medica*, tho' they are look'd upon rather as morbid substances; because a famous physician, who practis'd long in the *East-Indies*, and had better opportunity than any *European* had before him, to try the virtue of bezoar, either equals or prefers them to oriental bezoar.

And bezoar.

We made the like experiment upon bezoar-stones; the first of which weighing in the air three drams, and odd grains, was found to be in proportion to water of the same bulk, as 1.47 to 1. Another, weighing somewhat less than three drams, prov'd to an equal bulk of water, as 1.53 to 1. A third, being the nucleus of a larger stone, weigh'd in the air two drams and fifty-one grains; and its proportion to water of the same magnitude, was found as 1.55 to 1. In which several instances, we may observe, that these animal stones, not amounting to twice the weight of an equal bulk of water, have a less specific gravity, by above a fifth part, than a true fossil stone.

The hydrostatical balance will distinguish between stones of the same denomination.

Again, we may by hydrostatics be assist'd to discover the resemblance or difference between bodies of the same denomination. Thus, for instance, we have found a notable difference between the specific weights of several load-stones dug up in different countries or mines: and if a greater number and variety of experiments of this kind were made, we might possibly find, that, *ceteris paribus*, the magnets of one country or mine, are considerably heavier than those of another: for I usually observe, that the *English* and *Norwegian* load-stones are heavier *in specie* than those which are said to come from *Italy*. And this difference of weight between fossils of the same kind, when considerable, may help us to distinguish between the stones of the same lowest species, that are peculiar to different countries or mines. But in case the unequal weight proceed, as it often does, from an adventitious matter insinuated into the more genuine part of the fossil, whilst that remain'd fluid or soft; it may enable us to conjecture at its greater or less purity; which, on many occasions, may be of no small use to a physician, a jeweller, or a naturalist.

Between genuine stones and counterfeit.

Another considerable advantage of our hydrostatical method, is, that it helps us to discern genuine stones from counterfeit, which too often pass for

for true, to the great prejudice of physicians and their patients, and the great loss of lapidaries and their customers: for as there are few qualities of bodies more essential than their ponderosity; so there is scarce any wherein impostors find more difficulty to make a notable alteration, than in this, without being discover'd. In several cases, indeed, 'tis not very difficult to alter the specific weight of a particular body, yet it may, perhaps, be impracticable to make a considerable change in that quality, unless by such additions or operations, as will cause a sensible alteration in some other qualities too, and thereby subject the fallacy to a discovery. And this will prove more particularly difficult to vulgar cheats, or adulterators of gems, and other valuable minerals; because the small knowledge they have of the number and variety of natural and artificial productions, confines them within narrow bounds, to accomplish their fraudulent designs. And whilst they are intent upon counterfeiting only the more obvious qualities of things, and, perhaps, upon eluding the known and popular trials; they will, probably, neglect the specific gravity. By this means, a pearl, for instance, is discoverable to be counterfeit, without the least prejudice to it. And I remember, some factitious corals, which I made, to shew what might be done in that kind, were, notwithstanding their fine colour, shape, and gloss, easily discoverable by their specific gravity manifestly exceeding that of natural corals.

And, sometimes, I have made pastes, or factitious gems, with red lead; which, tho' transparent, and finely colour'd; yet by reason of their weight, were very liable to be detected by hydrostatical trials. I have seen a fair adulterate bezoar-stone so resembling the genuine, that a great price was set upon it; but being brought to me for my opinion, I made no doubt of its being counterfeit, from its appearing as heavy, as a mineral stone of the same bulk.

Another use to be made of our hydrostatical method of examining solids, is, on many occasions, to assist us in making estimates of the genuineness, or the degree of purity, of several bodies, that may be usefully employ'd in physic; provided they are heavy enough to sink in water: for when we have once found the specific gravity of a concrete, which we know to be good and genuine; the degree of its ponderosity may serve for a kind of standard, whereby to judge of other portions of the same denomination.

A great difference should, doubtless, be made between the estimate of some stones, vulgarly called gems, according as they serve the purposes of jewellers and goldsmiths, of physicians or chymists: for the tradesman, who usually aims wholly at the beauty and lustre of a stone, may justly esteem those, *ceteris paribus*, the best, which are in *specie* lightest; because such are generally more uniform as to sense, and more transparent; and also receive their colour from pigments of finer parts. But, on the contrary, those who seek for medicinal virtues in gems, may justly value such the highest, as are most ponderous; because they are more plentifully stock'd with metalline or mineral substances: whence the greatest part of their virtue is, in all probability, derived. And this difference in the

specific weight of stones of the same name; I have sometimes found far greater than one, who has not try'd them, would imagine; as will appear hereafter: yet I would not from hence infer, that even such stones as appear fine, and are but light in their kind, have not metalline particles that may give them considerable medicinal virtues. For there are mineral pigments of so subtile a nature, that such an insensibly small quantity, as will scarce render them heavier than colourless gems, may be diffused thro' the whole, and impregnate every sensible part thereof. Thus I have known five grains of powder'd zaphora, mix'd with an ounce and half a dram of finely pulveriz'd *Venice* glafs, and kept an hour in fusion by a very violent fire, afford a transparent mass, which was throughout of a fine deep blue colour; so that one part of the pigment tinged above a hundred parts of the glafs: and when we added eight grains of zaphora to an ounce of glafs, that is, one part to sixty; the mixture having been kept for the like time in strong fusion; the mass was too deeply colour'd for a handsome gem.

And further to manifest, that a little metalline matter may suffice to give a tincture, and thereby impart a virtue to a vitreous body, and even to gems; I shall add another experiment. I had long conjectured, that there was in granats, especially in some of a deep colour, abundance of chalybeate corpuscles, more than sufficient for the granat itself: upon this supposition, I took a *German* granat, which I had kept by me for a rarity, on account of its bigness and deep colour. Of this, carefully reduced to very fine powder, we exactly mix'd eight grains with an ounce of finely pulveriz'd crystalline glafs, and afterwards kept the mixture for two hours in a very hot furnace; by which means, we obtain'd a pretty uniform mass, ting'd of a green colour, like what prepared iron or steel gives to pure glafs.

Thus much may serve in some measure to shew the use of the hydrostatical method of examining drugs; upon a supposition that they are solid, of a sufficient bulk, and gravity able to sink them in water. But many simples, and other ponderable substances, that may, upon good grounds, be refer'd to the *Materia medica*, want one or more of these conditions: I shall, therefore, offer the expedients I make use of in such cases, to bring them to this hydrostatical trial. Now a body may either be liquid, and consequently not immediately applicable to a slender string; in the form of a powder, or small fragments, and impossible, or difficult at least, to fasten each of them to a hair, and suspend it as a body of a greater bulk; or lastly, dissoluble in water, and consequently unfit to be weigh'd therein.

Suppose, then, the liquor we would examine, be specifically heavier than water, and unapt to mix therewith; I take such a bucket, or wide-mouth'd glafs, as we formerly mention'd, capable of containing an ounce or two of common water, and whose weight in air is about three or four drams. This glafs we weigh very carefully, once for all, first in air, and then in water; and thereby find a weight equivalent to it in water: which being put into the opposite scale of the balance, whilst the vessel hangs under the surface of

This method of examination applied to fluids heavier than water, and unapt to mix therewith; for instance, to mercury.

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of the water, may be consider'd as having no weight at all; and, consequently, the additional weight of this bucket, may be alone taken for that of the contain'd body itself in water: so that, for instance, the bucket thus makes a mass of quick-silver, tho' fluid, as fit for weighing as if it were coagulated into a solid. And in this manner we proceed to weigh it hydrostatically, as if it were a solid.

But, for the more ready performance hereof, 'tis convenient to have in readiness a couple of weights, the one equal to the weight of the glass-bucket in the air, and the other to the weight of the same in water; for these immediately afford a counterpoise to the vessel in either medium. And, thus, we put a small glass-jar, capable of containing half an ounce of water, into one scale of a tender balance, and furnish'd the opposite with a counterpoise thereto; into this little vessel we pour'd 480 grains of *Spanish* mercury, and suffer'd the bucket, with this mercury in it, to hang by an horse-hair, from one of the scales, in a deep glass-vessel of water: there was now in the opposite scale a counterpoise to the weight of the glass in the water; so that the weights that were added, gave us the weight of the quick-silver only; the weight of the glass being already allow'd for. By this means we found the weight of the quick-silver, in water, to be 446 grains; whereby the greater number being divided, the quotient is 14, and about $\frac{7}{11}$: so that the mercury appear'd to be in gravity, to water of the same bulk as 14.11 to 1: tho', in former trials, I scarce found common quick-silver to weigh full fourteen times, and sometimes scarce thirteen and a half so much as an equal bulk of water. Whether the weight of the present mercury proceeded from its participating of gold more than others; which, from another experiment, seem'd not improbable; I will not here determine.

However, upon abundance of tryals of this kind, I did not find that all running mercuries, tho' they appear'd unadulterated, were precisely of the same weight. Nay, I observ'd, that even distill'd mercuries, if once combined with metalline bodies, and particularly if drawn from fine gold, differ'd more from the common sort, than these from each other: and, also, between common mercuries distill'd, we found a notable difference. And, by this method, tho' not always with the same ease, we may examine the specific gravity of other heavy fluids that are indisposed to mix with water; such as are, for instance, the chymical oils of cinnamon, cloves, guaiacum, &c. But what made me the more circumstantial in delivering the foregoing experiment, was, that this practical direction for weighing one liquor in another, will, hereafter, appear applicable to very useful purposes.

The way of hydrostatically examining the powders and fragments of sinking bodies, hath been already touch'd upon; and differs little from the former: only it deserves to be again repeated, that great caution must be here us'd to moisten the powder with the same water wherein 'tis to be weigh'd, before the experiment is began; that the liquor may have time to insinuate itself between the dry corpuscles, and thence expel the air that

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If this way of examining bodies be carefully practis'd by a skilful hand, and with a tender balance, it may prove of considerable use to physicians, druggists, apothecaries, lapidaries and goldsmiths. Thus, for example, the fragments of the five precious stones, that are made ingredients of *Confectio Hyacinthi*, may each sort of them, a-part, be properly examin'd by their weight in water, when the true specific gravity of a parcel of the finest is once known. And, by the way, tho' granats are reckoned among the five precious medicinal stones, and, in some dispensatories, are prefer'd as the best; I have found a great difference, in point of ponderosity, between *European* and *American* granats, compar'd with others, which I myself took out of an odd *American* mineral; whence, 'tis natural to conclude, that their virtues might be very different, if not as to kind, yet at least as to degree. And not only factitious pearls, that often pass for genuine, may often, by this expedient be discover'd, especially if mercury enter their composition; but we may, probably, by the same method, distinguish the natural pearls of several kinds, and different countries; whereof I have seen, and possess a far greater variety than would easily be credited. But, because it more concerns physicians, and their patients, to make an estimate of seed-pearls, than of the larger sort, that are seldom used but for ornament; I shall here observe, that, when furnish'd with very fine oriental seed-pearls, which seem'd proper for a standard wherewith to compare others, we found them to water, of the same bulk, as 2 $\frac{1}{2}$ to 1.

To bodies that
will dissolve or
mix with water.

But what course must we take, when the body to be hydrostatically examin'd, will dissolve, or easily mix with water? Why, tho' the proposed body cannot be immediately weigh'd in water; yet we may substitute another liquor, that will not dissolve it, and thereby find its specific gravity to that medium; and then, by comparing the difference of those two liquors, in point of gravity, we shall discover the body's weight in water; supposing it could have been kept undissolv'd therein. Considering, then, that, except quick-silver, the visible fluids we can command, are either of an aqueous, or oily nature; and that most bodies, whereof we can make solutions in liquors of the former kind, will not dissolve in those of the latter; we presume, that the most saline bodies, such as alum, vitriol, sal-gem, borax, sublimate, &c. may be commodiously weigh'd in oily fluids, as oil of turpentine, for instance, or rather the spirit of turpentine, which I, in this case, prefer to any other fluid.

And here, to avoid the trouble of calculation, we may, often, easily find the specific gravity of bodies to others of the same, or a different species; and so make a probable guess as to their genuineness, by being provided with one piece of the body, of known purity, to serve as a standard.

dard. Thus, for example, if the goodness of sublimate be suspected, take an ounce of some that is known to be fine, carefully balance it in oil of turpentine, and note its weight therein; then, an ounce of the suspected sublimate being weigh'd as the former, if it prove of the same gravity with the standard, it may be supposed good; but if it weighs less, 'tis a sign it wants a due proportion of mercury. The same method is applicable to *Mercurius dulcis*, and many other bodies, wholly, or partially dissoluble in water; as alum, which is often sophisticated with some baser salt; to *Roman vitriol*, which is, sometimes, either counterfeited, or adulterated, by roch-alum and a tincture of copper: and, according as the weight of the body in oil recedes more or less from that of the standard; so the adulteration may be probably concluded less, or greater.

I might, also, observe another useful way of estimating the gravity of solids and fluids, by taking a heavy solid, and carefully observing, once for all, its weight in the air; then weighing the same, first in one of the liquors to be examin'd, and afterwards in others: whence, the difference between the solid, and each of the fluids being observ'd, 'tis easy to find the specific gravity of each, and to assign the proportions betwixt them. Another way of finding the specific gravity of fluids.

But if only one solid be employ'd, 'tis necessary such an one be chosen; as hath a much greater specific gravity than is barely necessary to sink it in water; because some liquors are much heavier than that: nay, it ought to be heavy enough to sink in all fluids, except quick-silver, if 'tis design'd for experiments in general. But 'tis not very easy to pitch upon such a single solid as hath all the qualities requisite to our purposes: for, it ought not to lose any of its weight by the insensible avolation of effluvia, yet be of a considerable specific gravity, and not too large, or intensely heavy, lest it overload a tender balance, or require too much liquor to surround it; its texture should be proof against sharp and piercing menstrua; it should, also, be of a make not liable to be broken or spoil'd; and, lastly, of a natural, uniform, and procurable substance.

Brimstone, hard wax, ivory, and white marble, have been, when properly shaped, severally made use of for this purpose; yet they all wanted one or other of the desirable qualifications mention'd. I, therefore, frequently employ'd three other bodies, as more proper. The first was a piece of amber, between three and four drams in weight, of a high yellow colour, very transparent, and of an uniform texture, and convenient figure. This we apply'd to examine the lighter sort of liquors, such as water, wine, brandy, &c. but 'tis not proper for the more ponderous liquors, since 'twill float at the top thereof.

The second is a globular glass, which I caus'd to be blown at a lamp, and to be hermetically seal'd at the neck, that was purposely made very short, after there had been lodg'd therein just as much quick-silver as we guess'd would serve to sink it in any fluid, except mercury. This, by reason of its great bulk, in proportion to its weight, was fit to discover minute differences in the gravity of the liquors 'twas weigh'd in, and could

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could not be corroded by sharp menstua; and, therefore, on several occasions, I prefer'd this instrument to either of the other.

But this being too liable to accidents, for experiments that are to be imparted, and recorded for posterity; I made use of rock-crystal, which, on account of its numerous good qualities, affords a fit measure, whereto other bodies may be compared. Of this pure concrete, we employ'd an almost compleat globe, weighing in the air two ounces, thirty-three grains; wherein were two small holes near one another, that might easily be stopped up with hard wax, after an horse-hair had been run thro' them; by means whereof it was easily fasten'd to the scale, and made to hang in the water. The bulk of this sphere made it the more fit to discover small differences between liquors, in point of gravity. But, because we may have often occasion to know the weight of fluids, which, by reason of their scarcity, we can command only in small quantities; as chymical oils, tinctures, essences, &c. we provide for such liquors an hexagonal prism of natural crystal, with a kind of pyramid at one end; which, by reason of its oblong figure, might be commodiously weigh'd in a slender cylindrical glass, that required but a small quantity of liquor to surround a body so conveniently shaped, and that weigh'd in the air but half an ounce, and sixteen grains. And, to render the observations made with these two bodies upon medicinal, and other liquors, the more useful; let it be taken notice of, once for all, that the ball of crystal was to an equal bulk of water, as 2.57 to 1, or thereabouts; and the prismatical oblong piece, as 2.66 to 1, to the same.

I have been the more particular upon the way of trying the gravity of several liquors with one solid, because a double application may be made of it on several occasions, serviceable to chymists, physicians, apothecaries, and others.

The several uses
thereof.

And, first, a piece of clear amber, or some such convenient body, that is not too little, nor, *in specie*, too heavy, may serve the chymist, apothecary, &c. to make probable guesses at the degree of spirituousity, or tenuity, to be found in many liquors belonging to the vegetable, or animal kingdom; and this with far less error, than by those signs whereon the common conjectures are grounded. For, a standard-liquor being provided, 'twill be easy, by observing the different weights of amber in several fluids, to judge of the fineness of any of them in its kind; for, *ceteris paribus*, that is the thinnest, or abounds most in spirituous parts, where the solid weighs more than in another; as, for instance, the amber we employ'd, that in water weigh'd $6\frac{1}{2}$ grains, weigh'd in common red French wine $8\frac{1}{2}$ grains; in common brandy, of a pretty good sort, $17\frac{1}{2}$ grains; and in spirit of wine, highly rectify'd, $34\frac{1}{2}$ grains. The same way may be employ'd to judge of the strength of spirit of vinegar, &c. But here it must pass for a general rule, that, probably, as in liquors the solid weighs more or less, according as the liquor 'tis weigh'd in, is more or less spirituous; so, on the contrary, in acid spirits and liquors, the less the solid weighs, the stronger the fluid must be reputed; that greater decrease
of

of weight proceeding usually from the greater proportion it contains of fixed salts.

'Tis another advantage of this method, that as the common way of trying the goodness of spirit of wine, brandy, &c. by setting fire to a spoonful of them, to see how much is inflammable, and what portion of phlegm will be left behind ; whereby, in tract of time, a considerable loss is sustain'd ; the proper proof may be made in our way, without any such expence.

By these observations, also, estimates may be made of undistill'd liquors of the same kind ; as, several sorts of beer, ale, cyder, &c. and the same solid may be employ'd to compare liquors of different kinds with each other.

But if the liquors to be examin'd are very ponderous ; amber will not be a proper solid in this case : for I have found, by trial, not only that it swims or floats in several fluids made by solution of salts, whether in the moist air, or with water ; such as oil of tartar *per deliquium*, a solution of the salt of pot-ashes, &c. but that some distill'd liquors would not suffer it to sink to the bottom of them ; as I found in oil of vitriol, spirit of nitre, and even good spirit of salt.

There is still another use to be made of our hydrostatical solid, which may frequently be as considerable, in general, by assisting in proportioning the strength of menstrua ; as the former is to discover the strength of particular liquors already prepared. For there are many experiments that do not succeed so well, unless the liquor be of a determinate degree of strength. Thus, I have found, that if *Aqua fortis*, whose strength is reckoned the best quality it can have, were rectify'd too high, it would not dissolve silver ; but require to be weakned, by an addition of water ; and that it would not near so well dissolve the raspings of crude lead, when only moderately strong, and fit to dissolve silver, as when considerably diluted with water. And in making extracts from many vegetable substances, chymists themselves may fall into a mistake, by affecting to employ their most rectify'd spirit of wine, as the best menstruum for their purpose ; for the medicinal virtue of many such bodies does not wholly reside in what they call their sulphur, or rosin, which indeed is best dissolved by highly rectify'd spirit of wine ; but in a more gummy, and, perhaps, mucilaginous substance, for whose extraction a moderately low spirit is more proper. Thus we see that gum-arabic, gum-tragacanth, &c. are indisposed to dissolve with the best rectify'd spirit of wine, but readily mix with more aqueous liquors. And some bodies, tho' dissoluble in both kinds of menstrua ; yet open less easily to strong spirit of wine, than waterish fluids ; as may be observ'd, particularly, in myrrh.

And thus much may serve to shew how useful it will be, on several occasions, to regard the degree of strength of the menstruum, or liquor, employ'd in any curious experiment ; so that when it should be repeated to the same purpose only, we may be able to bring the fluid we make use of, to the same degree of strength with that before employ'd, which produced the:

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the designed effect. But, in very accurate experiments, 'tis proper to observe; that if the liquor be very ponderous *in specie*, as oil of vitriol, or oil of tartar *per deliquium*; something ought to be placed in the scale, from which the solid hangs, to compensate for the increas'd part of the hair, which must be consider'd as a somewhat lighter body than the fluid, and, therefore, capable of buoying up the solid.

Still other methods for the same purpose.

Besides the method just mention'd, there is another, which we have often found of service, in comparing the weight of different liquors of the same magnitude. This is done by successively filling a vial, furnish'd with a long, slender cylindrical stem, to a certain standing mark, made near the top, with the several liquors, to be, by weight, compared together. But this not being hydrostatical, I shall no longer insist upon it here. To proceed, therefore, there is, likewise, another way to discover whether two or more liquors propos'd, differ in specific gravity; and to make some estimate of their difference: *viz.* by a hollow cylinder of brass, or other metal, made somewhat heavy at the bottom, that it may swim upright; which will sink more or less, in several liquors, as they are lighter or heavier than one another. But *Mersennus*, who proposes this way, confesses it to be very difficult to make sure observations thereby; to which I add, that the metal may be corroded, or otherwise affected by urinous menstrua.

What *Mersennus* said of this instrument, is applicable to a different one, made of two glass bubbles, and a very slender stem, which is hermetically sealed; with a ballast of quick-silver, in the lowest, to keep it steady, when partially immers'd in liquors; where like the metalline cylinder, it sinks deeper in the lighter than in the heavier, according to their differences in gravity. But tho' I have often employ'd this instrument, and found it serviceable, when I made use of several of different sizes, according to the various liquors I was to examine; yet it is, in many cases, inferior to the balance.

Mersennus, also, proposes another hydrostatical manner of weighing liquors in water, thus. Take a glass vial, to which, being first weigh'd in air, and then in water, you are exactly to fit a stopple of wax, or cork; this done, fill the vial with the liquor you would examine, so that no air be left between that and the stopple: the vessel, thus fill'd, is to be weigh'd in water; and subtracting from its weight there, that of the glass in water, and also that of the stopple, the remainder will give the weight of the liquor in water. This may, perhaps, be serviceable, on some occasions, yet, I fear, it is troublesome, in practice; for, ordinary vials, capable of containing a competent quantity of liquor, are usually too heavy to be applied to tender balances; and common stopples will be subject to various inconveniencies: for which reason, I prefer to it a method I formerly thought of, and which I have, sometimes, put in practice, by chusing a small round vial; and, instead of ordinary stopples, fitting it with one of glass, carefully ground to the neck of it: for, by this means, the inconveniencies of a stopple, lighter than water, were avoided; nor would that alter its specific gravity, either by absorbing, or evaporating; nor would it be penetrated

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by the most subtle, or corroded by the most fretting spirits: to which may be added, (because, in some cases, it is considerable) that a glass stopple will not communicate any tincture, or extraneous quality to the liquor; which cannot be said of cork, or wax, in regard of some subtle, and very corrosive liquors. This vial, then, together with its stopple, being carefully weigh'd, first in air, and then in water, to settle the gravity of the whole instrument; we fill'd it exactly with the liquor to be examin'd, and proceeded as if we were to weigh quick-silver, in the manner already described: the weight of the given liquor in water being thus obtain'd, its proportion, to an equal bulk thereof, may be easily discover'd. This way of examining liquors has its use; and I the rather, sometimes, made choice thereof, because 'tis applicable to all kinds of fluids, whether heavier *in specie* than water, or lighter.

Laying aside the stopple, the round ball may serve, on several occasions, instead of the hydrostatical bucket, formerly mentioned; for the weighing of quick-silver, and heavy powders; especially coarse ones. But, if the instrument be fitly shaped, and not too heavy, a greater conveniency than this attends it: for when you have liquor enough to surround the bottle, it may be commodiously substituted for the hydrostatical bubble, with quick-silver; its exact stopple supplying the place of an hermetical seal: and, besides, 'tis far less subject to break than a bubble. That I made most use of, which weigh'd about 709 grains, being well stopp'd, with only air in it, would sink, by its own weight, in brandy, wine, water, &c. And, if it were to be employ'd in liquors much more ponderous than water, as *Aqua fortis*, oil of tartar *per deliquium*, &c. 'twas easy to fit it for them, also, by putting into it a quantity of quick-silver, of a determinate weight, before it be stopp'd; which balast, when the operation is over, may be easily taken out, and kept a-part for the like uses.

But, notwithstanding all this, since glasses of a proper size, shape, and weight, fit for tender balances, and furnish'd with exact glass stopples, are very difficult to procure; and since the way itself is subject to some inconveniencies, it seems, in general, that this way of finding the weight of liquors in water, is inferior to the more simple ones, before recommended.

Having now laid down the method of weighing one liquor in another, 'tis fit we subjoin some application of it.

The use and advantages of weighing one fluid in another.

Among other advantages of hydrostatics, to a sagacious physician, 'tis none of the least, that it gives him the specific gravities of various liquors; which may not only help to distinguish the genuine and good, from the adulterate and decay'd, but serve to other purposes also. Instances hereof are afforded by the juices of herbs, and fruits; a determinate quantity of which, being first weigh'd in the hydrostatical bucket; and some oil of turpentine poured on it, we sink it warily in the same liquor, whose specific gravity to refined silver, or clear rock crystal, is before-hand carefully registred: for thus substituting this oil for common water, we may discover the specific gravity of liquors that would mix with water. And,

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by this means, we may find, not only the difference in weight, between the juices of plants of differing kinds, but, on some occasions, observe how far the keeping of a juice, for some time, or the fermentation, or putrefaction thereof, will alter its specific gravity. Other liquids, also, that are used in physic, but not ponderable in water, may, by this way, be examin'd; as honey, vinegar, verjuice, &c. And, by the same method, likewise, the specific gravity of the juices of fruits, may be discover'd, and compared together. And hence a person of curiosity will, probably, be enabled to take notice of the differences produced in their specific gravities, thro' their several successive states, at different times. Thus the juice of ripe grapes alters when newly press'd; again, when it begins to ferment; when it becomes new wine; when it has attain'd its full maturity, and perfection; and, lastly, when it degenerates into prick'd wine, and absolutely changes into vinegar, or *Vappa*.

All matters near-
ly of the same
weight.

It may be here proper to obviate an objection, that will, probably, be made against the method hitherto deliver'd, of finding a proportion in weight, betwixt a sinking body, and water of the same bulk. Naturalists might alledge, that, by this method, we cannot discover the proportion between a solid body, and water in general; but only betwixt the proposed body, and the particular water 'tis weigh'd in; because there may be a great disparity between liquors that are call'd common water. Thus some travellers tell us, that the water of the river *Ganges* is, by a fifth part, lighter than ours.

To this plausible objection, I answer, first, that having had opportunity to examine the weight of various waters, some of them taken up in places very distant from one another, I found the difference between their specific gravities to be exceeding small; perhaps, not above the thousandth part of the weight of either: nor did I find any considerable difference, between the weight of several waters of differing kinds; as between spring-water, river-water, rain-water, and snow-water: tho' this last was somewhat lighter than any of the rest. And having received water from the river *Ganges* itself, I found it very little, if at all, lighter than some of our common waters.

And, secondly, 'tis not necessary, that the proportion obtainable by our method, should be mathematically exact: for, in experiments, where we are to deal with gross matter, and employ material instruments, a physical accuracy will suffice.

To discover the
magnitudes of
bodies, hydrosta-
tically.

There is an use of hydrostatics, which tho' it do not directly tend to the examination of the *Materia medica*, may yet be serviceable, both to the physician and the naturalist, in delivering descriptions thereof; and thereby indirectly conduce to the knowledge of drugs; and help to distinguish between genuine and adulterate simples. 'Tis known, that the writers upon the *Materia medica*, usually set down the magnitude of the bodies they describe, by very uncertain guesses; and those who assign them determinate measures, either do it by saying, such a fruit, for example, is an inch, or two inches long, and half an inch, or a whole one, broad: but, according

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ding to this way of describing bodies, there may, by reason of the great variety of figures they are capable of, be a very great difference in the bulk of bodies, to each of which, the same length and breadth are applicable.

I caused several cubes to be carefully made, by skilful artificers, of different sizes, and materials, whose sides were each of them an exact inch, or a precise number of inches. These cubes, being carefully weigh'd in exact balances, first in air, and then in common water, we concluded, from the result of our several trials, that, without considerable error, a cubical inch of water might be supposed to weigh 256 grains.

Suppose, then, for example, that a solid heavier *in specie* than water, having been weigh'd first in the air, be found in water to lose 256 grains of its weight; the dimensions of this solid, if it were of a cubical shape, will equal a cubic inch; so that if the given body be suppos'd a metal easily fusible, as tin, or lead, melted and pour'd into a hollow, metalline, cubic inch, and suffer'd to cool, it would exactly fill it. For a sinking solid weighs less in water than in air, by the weight of an equal bulk of water. And, as a cubical inch of water weighs 256 grains, it follows, that when the decrease of a body's weight in water, is 256 grains, the solid content of that body is a cubical inch; since a body of water, weighing 256 grains, is equal in magnitude, as well to the solid, as to a cubic inch of water.

And here it may prevent a scruple, to observe, that to make bodies equal in magnitude, it is not necessary that they should be of the same weight, or matter; as is evident in bullets of copper, tin, and gold, cast in the same mould. For, tho' they be all equal in bulk, yet the bullet of copper will be much heavier than that of tin; and the bullet of pure gold, more than twice as heavy as that of copper. Whensoever, therefore, a solid, ponderous enough to sink in water, loses therein 256 grains of the weight it had in air; the magnitude, or bulk of that body, is equal to a cubical inch; of whatever matter it consists, or, of what shape soever it be. And, in case the solid propos'd, loses of its weight in the water, less than 256 grains; its bulk will be proportionably less than a cubical inch. Thus every 32 grains that the solid loses of its weight in the water, answers to an eighth of an inch in the bulk of the body: so that if the decrease be 128 grains, the solid will be half a cubic inch; and if but 64 grains, a quarter of a cubic inch: on the other side, if the decrease of the given body, exceed the standard, 256 grains, twice, thrice, &c. the body will be equal to two, three, &c. cubical inches.

'Tis easy, from the doctrine deliver'd, to discover, hydrostatically, the solid contents of a body, heavier *in specie* than water: but to measure, by the help of water, the solidity of a body lighter *in specie* than that fluid, is a work of more difficulty. In order to it, we may consider there are two sorts of bodies, which will not, naturally, sink in water; some, being of a closer texture, resisting it; and others abounding with pores, which dispose them to imbibe the water.

To gain the solidity of a body, hydrostatically, tho' lighter than water.

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Now the weight of a body of the first kind, may be gather'd from that of the water equal in magnitude to the immerfed part of the body, when it floats freely thereon. Thus, if a parallelopiped, or a cylinder of wood twelve inches long, placed upon water, should rest there when a twelfth part of it lies beneath the surface of the fluid; the weight of the water, equal in bulk to that immerfed twelfth part, would be equal in weight to the whole body of the wood. But because the bodies, whose bulk physicians and chymists may have occasion to examine, will very seldom happen to have regular figures; 'tis proper to add another method, more fuitable to the present design. To measure then the solid contents of bodies specifically lighter than water, but irregularly shaped*; the body must, first, be weigh'd in air. 2. A plate of lead, capable of sinking this body in water, and of some round sum in weight, should be provided. 3. This plate being weigh'd in water, and its weight therein subtracted from its weight in air, will give the weight of as much water, as is equal in bulk to the immerfed lead; or the specific weight of the lead in water. 4. The plate of lead, and the lighter body, must be tied together with horse-hair, and the weight of the aggregate noted. 5. This aggregate must be weigh'd in water, and its weight therein subtracted from that it had in air; and the difference will give the specific weight of the said aggregate in water. 6. From this difference subtract the specific weight of the plate in water, and the remainder will give the weight of the lighter body in the same fluid. Then, lastly, that weight of the light body in water being divided by 256 grains, will give its solid content.

To clear and confirm this method by an example; we took a conveniently shaped piece of oak, that weigh'd in air $193 \frac{1}{2}$ grains; to this we tied, with an horse-hair, a plate of lead, weighing just 240 grains. But, before we tied them together, the lead was weigh'd in water, where it lost of its former weight 20 grains; which, being deducted out of the 240 grains, left a difference of 20 grains, for the specific weight of the lead in the water. Then the aggregate of the wood and lead was weigh'd, first in the air, and found to be $433 \frac{1}{2}$ grains, and next in water, where it amounted but to 162 grains; which being subtracted from the aggregate of the same bodies in air, the difference was $271 \frac{1}{2}$ grains; from which, the other difference of 20 grains of the leaden-plate in water being deducted, there remain'd $251 \frac{1}{2}$ grains, for the weight of a bulk of water equal to that of the given piece of wood. Now, if this number of grains had amounted to 256, we might have concluded the solidity of the body

* There is a very easy method of obtaining the content, or cubic measure, of any solid, tho' ever so irregularly figured. For, since such a body, immersed in a fluid wherein it will not dissolve, causes that fluid to rise in direct proportion to the bulk immersed; if the fluid be contain'd in a cylindrical or prismatic vessel,

and the additional height given it by the body be marked on the vessel; this will enable us to measure a part of the whole cylinder or prism, equal in bulk to the irregular body. And thus may the solid content of statues, carv'd or emboss'd work, &c. be known.

to be a cubic inch; since 256 grains of water, which we formerly found equal to a cubic inch of water, was also now found equal to the bulk of the given piece of wood. And having caused the wood I employ'd to be formed into a cubic inch; the difference of its weight in water from 256 grains, may, probably, be imputed to some little imperfection in the figure, or other like circumstance. First, then, the cube of oak in air, weigh'd $193 \frac{1}{2}$ grains. 2. The lead in air, 240 grains. 3. The lead in water, 220 grains; which, substracted from its weight in air, leaves for its specific weight in water, 20 grains. 4. The aggregate of the two in air, $433 \frac{1}{2}$ grains. 5. The weight of both together in water, 162 grains; which, substracted from the sum of their weight in air, gives the difference of the two several aggregates, $271 \frac{1}{2}$ grains. 6. The difference between the weight of the lead in air, and in water, 20 grains; which, substracted from the difference of the weights of the aggregates in air, and in water, gives for the weight of the proposed cube $251 \frac{1}{2}$ grains.

This way of measuring bodies is appropriated to such of them as will not readily dissolve in water. But, because there are many other solids, as salt, alum, vitriol, sugar, &c. whose magnitudes it may often be proper to know, and compare; I shall add, that the same method is applicable, also, to solids dissoluble in water, if, instead of that fluid, be substituted oil of turpentine, whose proportion of specific gravity to water, is otherwise known. To discover this, I employ'd the hollow cubic inch of brass, made use of to find the weight of a cubic inch of water; and found, that, when carefully filled, it contained $221 \frac{1}{2}$ grains of this oil: by which number, the difference of the weight of a solid in the air, and in that oil, being divided, the quotient will give the solid contents of the body to be examined.

Let us now consider how such bodies as, by their porosity, are subject to imbibe too much of the liquor, while the experiment is in hand.

Mersennus's expedient, in this case, is to cover the body to be weigh'd in water, with wax, pitch, or some other glue, of a known specific weight in water. But I prefer bees-wax for this purpose; and proceed with it in the following manner. The solid which is lighter than water, having been first weigh'd in air, over-lay it carefully with thin bees-wax; then take, also, in the air, the weight of the wax employ'd; and fasten to the body thus coated, a plate of lead, or tin, heavy enough to sink it; and observe the weight of the aggregate in water. This done, substract the weight of as much water as is equal in bulk to the wax, and proceed as is before taught.

But this method helps us only to the weight of the proposed body in water; to discover its solid content, we must divide the weight of the solid in water, by 256 grains.

And, by the way, I have, sometimes, made use of another expedient to hinder small solids, whether lighter or heavier *in specie* than water, from imbibing the liquor wherein they were weigh'd: for having first found the weight of a cubic inch of quick-silver; we placed the body to be measured,

The Hydrostatical Balance.

STATICS. fured, in a vessel, whose solid contents were known before-hand; when, the space that remain'd unpossessed by the firm body being fill'd with quick-silver, 'twas easy to know, by the difference in weight of that quick-silver, from the weight of the quick-silver requisite to fill the whole vessel, how much thereof was equal to the surrounded body. And, by this means, and the knowledge before gain'd of the weight of a cubical inch of mercury, the solid content of the body propos'd, was easily obtain'd.

What accuracy
is to be expected
in hydrostatical
experiments.

Before I put an end to this discourse, 'tis proper to shew what credit may be given to the estimates of the weights and proportions of bodies obtain'd by hydrostatical trials; because mathematicians, either not knowing, or not applying our observation about the specific gravity of rock-crystal, and the nature of oil of turpentine, have given us very different accounts of the proportions of metals, and a very few other familiar bodies, which are all they seem'd to have examin'd by this method.

And, indeed, I should not be surpriz'd to find, that the experiments of the same person, made at distant times, and under different circumstances, disagreed; for some difference there may be betwixt the waters employ'd in this case; especially if the air be at one time intensely hot, and at another exceeding cold. The difference, also, of degrees in goodness of the balances used in nice experiments, is not altogether inconsiderable. But a greater hindrance to the accuracy of hydrostatical experiments, is the difficulty of finding an exact uniformity in weights of the same denomination, which are vulgarly supposed to be exactly equal. I have myself found it so difficult in practice to procure, and keep weights as exact as I desired, that I have left off the hopes of it; for the very air may, in time, cause an alteration in them. And tho' the accurate *Ghetaldus's* tables of the weight of metals, and some other bodies, in respect to one another, are look'd upon as the most authentic that have been publish'd; and are, accordingly, the most made use of; yet 'tis certain, the weights he employ'd are not divided as ours are. For tho', according to him, as well as with us, the ounce consists of four and twenty scruples; yet the scruple, which with us is divided but into twenty grains, he divides into twenty-four. But tho' hydrostatical experiments are not, always, either singly accurate, or exactly agreeable to one another; they prove accurate enough to be very useful in practice, and more exact than any other method, hitherto employ'd, of determining the proportions of bodies, in point of weight and bulk, and of measuring their solid contents; but, especially, such little ones as are necessary to be examined in the *Materia medica*. And this is a corollary from the whole of what we have deliver'd.

And, indeed, as little as my skill is in hydrostatics, I would not be debarr'd from the use of it, for a very valuable consideration; for it has already done me acceptable service, and on a vast variety of occasions; especially in the examination of metals and mineral bodies, and of several chymical productions. I have often been able, by its means, to undeceive artists in their persuasion of possessing *Luna fixa*, and other valuable commodities; and to make a judgment, as to the genuineness

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or falsity, and as to the degrees of worth or strength of many rich and poor metalline mixtures, and other bodies, both solid and fluid; whose fair appearances might, otherwise, have greatly deceived me.

And, to shew some curious persons how far hydrostatics might be serviceable to as accurate mensurations as need be expected in physical experiments; I desired a virtuoso to mix tin and lead in a certain proportion, unknown to me; and melt them into one mass; which I carefully weigh'd in water, and also examined it algebraically; and from hence assign'd the respective quantities of each; which agreed, within little more than a grain, with those he had committed to paper, before he mixed them. And this small difference, probably, proceeded from some scarce avoidable inaccuracies in melting and managing the given bodies.

To conclude; 'tis not to be expected, that the specific gravities of the bodies mention'd in the following table, shall, all of them, be found, in future tryals, precisely the same as we there exhibit them. For, besides that experiments are made by persons of different qualifications, with different degrees of care, and with different instruments; * the varieties may proceed from a difference in the texture and compactness that may be found in several bodies of the same kind. For, neither nature, nor art, give to all the productions of the same name, a mathematical preciseness, either in gravity, or other qualities.

* Dr. *Jarin* recommends it, as a necessary caution, to all those who shall attempt to weigh dry, porous solids in water, for philosophical purposes, that, by some proper means, they first extricate the air out of all the small pores and cavities of them; whereby the water may have free liberty to enter thereat: otherwise, the air contain'd therein, by keeping the water out, will render the solid lighter in water than it really is. The best way of avoiding this inconvenience, the Doctor tells us, is to set the vessel of water, wherein the solid is immersed, under the receiver of an air-pump, and ex-

tract the air out of the body; which will succeed the better if the water be first heated over the fire: or, in defect of an air-pump, he directs us to let the solid, if its texture will safely permit, continue for some time in boiling-water, over the fire. To a neglect of this observation, the Doctor thinks, among other things, is owing that difference found in the accounts of the specific gravity of the *Calculi humani*. And when the air is thus extracted out of wood, as oak, fir, &c. and the roots, stalks, leaves, and seeds of vegetables, they prove specifically heavier than water. *Philos. Transf. N^o. 369. p. 223.*

The

The Hydrostatical Balance.

A TABLE of the specific Gravities of Bodies compared with Water. *

	Weigh'd In Wa- in Air. ter.		Proportion.
A Gate ———	251	156	2.64
A piece of alum-stone ———	280 $\frac{1}{2}$	152 $\frac{1}{2}$	2.18
Amber ———	306	12	1.4
Antimony, good, and supposed Hun- garian ———	391	295	4.7
Bezoar stone ———	187	61	1.48
A piece of the same ———	56 $\frac{1}{2}$	22	1.64
A fine oriental one ———	172	60	1.53
Another ———	237	61	1.34
Calculus humanus ———	2570	1080	1.72
Another ———	302	97	1.47
Calx of lead ———	138 $\frac{1}{2}$	123	8.94
Cinnabar, native ———	197	171	7.57
Cinnabar, native, and very sparkling	226	194	7.6
Cinnabar common ———	802	702	8.1
Cinnabar of antimony ———	197	169	7.3
Coco-shell ———	331	85	1.34
Copper-ore ———	1436	1090	4.15
Copper-ore, rich ———	413	314	4.17
Copper-stone ———	65 $\frac{1}{2}$	49 $\frac{1}{2}$	4.09
Coral, white ———	336	204	2.54
Another piece, fine ———	139	85	2.57
Coral, red ———	129 $\frac{1}{2}$	80 $\frac{1}{2}$	2.63
Cornelian ———	148	103	3.29
Crabs eyes, native ———	77 $\frac{1}{2}$	36 $\frac{1}{2}$	1.89
Crabs eyes, artificial ———	90 $\frac{1}{2}$	54	2.48
Crystal ———	256	140	2.21
Gold-ore, not rich, but brought from the East-Indies ———	1100	682	2.63
Another lump of the same ———	1151	717	2.61
Granat, Bohemian ———			4.16
Granati minera ———	217	147	3.1
Ivory ———	173 $\frac{1}{2}$	83	1.11
Lapis Calaminaris ———	477	380	4.72
—— Hematites, English ———	1574	1156	3.16
—— Judaicus ———	261 $\frac{1}{2}$	164	2.69
—— Lazuli, one piece ———	385	256	2.78
—— Manati ———	452	293	2.16
A fragment of the same ———	218 $\frac{1}{2}$	123	2.22

Another

The Hydrostatical Balance.

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	Weight in Air.	In Water.	Proportion.
Another — — — — —	345	197	2.33
Another from Jamaica — — — — —	2011	1117	2.27
Lead-ore — — — — —	686	590	7.14
Another piece from Cumberland, rich	1872	1586½	7.54
Manganese, a piece — — — — —	321	230	3.53
Marcasite — — — — —	814	631	4.65
Another from Stalbridge — — — — —	243	189	4.5
Another more shining than ordinary	287	227	4.78
Mineral, Cornish, like a shining mar-			
casite — — — — —	145	129	9.6
Ore of silver, choice from Saxony — — — — —	458	366	4.22
Another piece — — — — —	1120	960	7.
Osteocolla — — — — —	195	108	2.24
Rhinoceros's horn — — — — —	8563	4260	1.99
Rock-crystal — — — — —	256	140	3.20
Slat, Irish — — — — —	779	467	2.49
Sulphur vivum — — — — —	371	185	2
German, very fine — — — — —	306	152	1.98
Talc, a piece like Lapis Amianthus	596	334	2.28
Venetian — — — — —	082	508	2.73
Talc { of Jamaica — — — — —	1857	1238	3
Tin-glass — — — — —	468	419	9.55
Tin-ore, New-Englsh — — — — —	812	613	4.8
Tin-ore, black, rich — — — — —	1293	984	4.18
Another piece, choice — — — — —	2893	2314	5
Tutty, a single piece — — — — —	104	83	5
Vitriol, Engl. a very fine piece — — — — —	1093	512	1.88
Vitrum Antimonii — — — — —	357½	282½	4.76
Unicorn's horn, a piece — — — — —	407	195	1.91

x 6.56

Grains.

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* Later trials have furnish'd us with the following table of the specific gravi-

ties of solids and fluids.

A TABLE of the specific Gravities of several solid and fluid Bodies.

Fine gold, —————	19,640	Cast brass —————	8,000
Standard-gold —————	18,888	Steel, } soft —————	7,738
Quick-silver —————	14,000	the same } hard —————	7,704
Lead —————	11,325	piece } spring temper ————	7,809
Fine Silver —————	11,091	Iron —————	7,645
Standard-silver —————	10,535	Tin —————	7,320
Bismuth —————	9,700	Glass of antimony —————	5,280
Copper —————	9,000	A pseudo-topaz —————	4,270

Vol.

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The Hydrostatical Balance.

STATICS.

A diamond	3,400	Serum of human blood	1,190
Clear crystal glass	3,150	Pitch	1,150
Island crystal	2,720	Spirit of salt	1,140
Fine marble	2,700	Spirit of urine	1,120
Rock-crystal	2,650	Human blood	1,040
Common green glass	2,620	Amber	1,040
Stone of a mean gravity	2,500	Milk	1,030
<i>Sal gemma</i>	2,143	Urine	1,030
Brick	2,000	Dry box-wood	1,030
Nitre	1,900	Sea-water	1,030
Alabaster	1,875	Common water	1,000
Dry ivory	1,825	Camphire	996
Brimstone	1,800	Bees-wax	955
<i>Danzick vitriol</i>	1,715	Linseed oil	932
Alum	1,714	Dry oak	925
Borax	1,714	Oil olive	913
<i>Calculus burnians</i>	1,700	Spirit of turpentine	874
Oil of vitriol	1,700	Rectified spirit of wine	866
Oil of tartar	1,550	Dry ash	800
Bezoar	1,500	Dry maple	755
Honey	1,450	Dry elm	600
Gum arabic	1,375	Dry fir	550
Spirit of nitre	1,315	Cork	440
<i>Aqua fortis</i>	1,300	Air	100½



TABLE of the Specific Gravities of several Solids and Fluids.

Cold brass	10,600	Gold	19,300
Steel	4800	Standard gold	18,880
the line	14,000	Thick silver	11,200
piece	11,200	Thin silver	10,500
Iron	7,800	Standard silver	10,500
tin	7,300	Bismuth	9,800
glass of anatomy	2,500	Copper	8,800
A glass to be used in the	2,500		

A N

Hydrostatical Discourse,

By way of

Answer to the Objections of Dr. MORE, and others, against some Explanations of particular Experiments:

With farther Considerations thereon.

SECT. I.

Finding no cause to alter my judgment, as to the solutions I have given, of some of my experiments, attack'd by Dr. More; I here design to vindicate them from his objections. *Mechanical solutions of phenomena. what?*

And, first, the Doctor having made a description of my pneumatic engine, pretends, that the ascent of the sucker, after it is depress'd, and clogg'd with a weight, is not mechanically accounted for, by the gravity and pressure of the atmosphere; because, says he, "if this solution were truly mechanical, he must have assign'd the true mechanical cause of the gravity of all the parts, and of the whole atmosphere."

To this I answer, that in delivering my experiments about the effects of the air, I did not intend to write a whole system, or so much as the elements of natural philosophy; but having sufficiently prov'd, that the air we live in, is not destitute of weight, and has an elastic power; I endeavour'd, by those two principles, to explain the phenomena exhibited in our engine; without recourse to a *Fuga vacui*, an *Anima mundi*, or any such unphilosophical principle. And since such kinds of explanations have, of late, generally been call'd, mechanical, as being grounded upon the laws of mechanics, I thought, as I permit, so I might be allow'd, the use of that term; and to entitle my explanations, mechanical, in the usual sense of that expression. I am not obliged to treat of the cause of gravity in general, since many propositions of *Archimedes*, *Sevinus*, and others, who have

STATICS. written of statics, are confess'd to be mathematically, or mechanically demonstrated; tho' those authors do not assign the true cause of gravity, but take it for granted, as a thing universally acknowledg'd. And, if in each scale, of an ordinary balance, a pound weight, for instance, be put; he who shall say, that the scales hang in equilibrium, because the equal weights balance one another; and, in case an ounce be added to one of the scales, and not to the opposite, he who shall say, that the former is depress'd, because urg'd by a greater weight than the other, will be thought to have given a mechanical explanation of the equilibrium of the scales, and their losing it; tho' he cannot give a true cause why either of those scales tends towards the center of the earth. Since, then, to assign the true cause of gravity, is not required, even in statics, tho' one of the principal, and most known parts of mechanics; why may not other propositions, and accounts, that suppose gravity in the air, and prove it too, be look'd on as mechanical?

The Doctor, however, is pleas'd to grant me almost as much as I need desire, as to the truth of the hypothesis whereon my explanations are founded. The principal thing which I suppose, in my hydrostatical writings, is, that in water, tho' stagnant, the upper parts actually gravitate upon the lower; or press upon them, even when they do not sensibly depress them. This hypothesis, the Doctor allows, agreeable to the principles of the mechanical philosophy: and, accordingly, having shew'd, that in a suspended tube of water, the whole liquor gravitates upon the bottom of it; and, consequently, that all the parts thereof do so, the upper upon the lower, "provided" says he "there be no immaterial principle in nature."

Now, I as freely, as the Doctor himself, assert an incorporeal Being, that made and governs the world. All that I have endeavour'd at, in explaining what happens among inanimate bodies, is to shew, that supposing the world to have been at first made, and to be continually preserv'd by God's divine power and wisdom; and supposing his general concurrence to the maintenance of the laws he has establish'd in it; the phenomena I consider, may be solv'd mechanically; that is, by the mechanical properties of matter; without recourse to nature's abhorrence of a vacuum, to substantial forms, or to other incorporeal creatures. And, therefore, if I have shewn, that the phenomena, I attempted to solve, are explicable by the motion, magnitude, gravity, shape, and other mechanical affections of the small parts of liquors; I have done what I pretended: which was not to prove, that no angel, or other immaterial creature, could interpose in these cases; for, concerning such agents, all that I need say, is, that, in the cases proposed, we want not their assistance; and, therefore, have no occasion to fly to it, in solving our phenomena.

*That the upper
parts of fluids
gravitate upon
the lower.*

But the Doctor, it seems, would have the gravitation of the elements, in their proper places, suspended by an incorporeal principle; and hereto he is led by this experiment, which, he says, is most manifestly repugnant to our hypothesis. He conceives, then, that in a bucket of water, with a perfectly cylindrical cavity, whose diameter is of 62 parts, there is forcibly kept

kept at the bottom, by means of a stick, a round piece of wood, whose diameter amounts but to 61 of those parts ; and that as soon as ever the stick is remov'd, the piece of wood will emerge to the top, and float : " which," says he " is impossible, if all the parts of the water, did not only " jointly press the bottom of the vessel, but each press'd the other directly " downwards."

But, first, since according to his computation, the area of the interval between the sides of the vessel, and the edges of the round board, is 123 of such parts, whereof the area of the board amounts to 3721 ; 'tis evident, that there must be room enough for the water to pass between the sides of the vessel, and the edges of the board ; which is supposed, on all hands, to be of some wood lighter *in specie* than water, since else it would not rise, upon with-drawing the stick.

Secondly, this round board is not suppos'd to be made exactly fit to the bottom of the vessel, and, consequently, the water may get in between them ; for which reason, 'tis necessary to keep the piece of wood forcibly down with a stick ; which were, otherwise, needless ; and, consequently, this interpos'd water, will communicate with the upper, along the sides of the vessel ; which latter may, according to the laws of hydrostatics, by means of that interposed, exercise its pressure upwards, against the lower surface of the wooden plate.

Thirdly, the Doctor supposes an imaginary plane of water to be parallel to the bottom of the vessel, and to pass along the bottom of the board ; so that of the water that lies between this plane, and the bottom of the vessel, one part is cover'd by the piece of wood ; and the other, between the edges of that, and the sides of the bucket, is cover'd with the incumbent water only.

Now, 'tis manifest, that in water, those parts which are most press'd, will thrust out of their place, those that are less press'd. 'Tis also evident, that the part of the imaginary plane, cover'd by the round piece of wood, must be press'd by a less weight than the other part of the same plane ; because the wood being, bulk for bulk, lighter than water, the aggregate of the wood and water, incumbent on the cover'd part of the same plane, must be lighter *in specie*, than the water alone, that is incumbent on the uncover'd part of the same plane ; and, consequently, this uncover'd part being more press'd than the other part of the plane, the heavier must displace the lighter ; which it cannot do, but by thrusting up the board ; as it does when the external force that kept it down, is removed. And this greater pressure against the bottom, than against the top of bodies immers'd in water, specifically heavier than themselves, is a true reason of their emersion.

'Tis true, that according to the Doctor's computation, if the solid cylinder, consisting of the wooden plate, and all the water directly incumbent on it, were put into an ordinary balance ; it would, there greatly outweigh the hollow cylinder of water alone, that rests upon the uncover'd part of the imaginary plane. And this is what seems to have deceiv'd the

Doctor :

Strat. Doctor. But there are many hydrostatical cases, wherein the phenomenon depends not so much upon the absolute weight of the compared bodies, as upon their respective and specific gravity; on account whereof, a small pebble, for instance, that weighs not a quarter of an ounce, will readily sink to the bottom of a river, on whose surface a log of a hundred pound weight will float. 'Tis a rule in hydrostatics, that when two portions of water, or any other homogeneous liquor, press against each other; the prevalency will go, not according to the absolute weight, but the perpendicular height of those portions. And, accordingly, we find, that if a slender pipe of glass, being fill'd with water, have its lower orifice unstopp'd at the bottom of a vessel of water, which contains much more of that liquor than the pipe; yet if the water in the tube were, for instance, two feet high, and that in the vessel but one; the water in the pipe will readily subside, till it comes almost to a level with the external water; tho' it cannot do so, without raising the whole mass of water stagnant in the vessel.

*Demonstrated by
experiments.*

We took an open-mouth'd glass, or jar, three inches and a half in diameter, and somewhat less in depth, its cavity being cylindrical; into this, having put some water to cover the protuberance that is left at the bottom of such glasses, we took a convenient quantity of bees-wax, and having just melted it, we pour'd it cautiously into the glass, warm'd before-hand to prevent its cracking, till it reach'd to a convenient height. This vessel, and the contain'd liquors, we set aside to cool, that the wax might shrink from the glass, and, consequently, have a little interval every where between the concave superficies of the vessel, and convex of the hardned wax; when, carefully pouring some water between the glass and the wax, so that it fill'd all the interval left between those two bodies, both at the bottom and the sides, the wax was made presently to float; being visibly rais'd up from the bottom, and its upper part appearing a little above the level of the water, as it ought, according to the true principles of hydrostatics. For water, being somewhat heavier, *in specie*, than wax, and that which was poured into the bottom, and stagnated there, being press'd by the collateral water, every way interposed between the concave part of the glass, and convex of the wax; (so that this collateral liquor answer'd what I call a hollow cylinder of water, in the Doctor's experiment) that part of the stagnant water which was press'd upon by the wax, being less press'd than the other part of the same stagnant water was by the water incumbent on it; this latter must displace the former; which it could not do, but by raising up the wax that rested upon it: yet this collateral water was so far from being heavier than the wax impell'd up by its pressure, that both the collateral and the stagnant water together, being weigh'd, amounted to little above a fourth of the weight of the wax; which happen'd by reason of the narrowness of the vessel: but if it had been wider, the experiment, I doubt not, would have succeeded, tho' the wax had out-weigh'd the collateral water ten times more than in our experiment it did. But, that the solid body exceeded almost four times the weight, not only of the collateral, but the stagnant liquor

quor too, sufficiently overthrows the Doctor's reasoning: the fallacy whereof will further appear from hence, that tho' we gradually pour'd in water, as long as the vessel would contain any; the cylinder of wax was, indeed, lifted higher and higher from the bottom of the glass, but did not appear rais'd more than at the first, above the upper surface of the water; which argues, that it was not at all the quantity of the lower water that continually increased; but the pressure of the collateral water, which continued still at the same height, with respect to the wax, that caus'd the elevation of the body.

And, to manifest yet more clearly the Doctor's mistake, I devised the following experiment. We took a round plate of lead, about the thickness of a shilling; and having stuck it fast to the bottom of the cylinder of wax, we successively placed upon the upper part of the wax, several grain-weights, till the wax subsided to the bottom: by this means, the glass being, at first, almost fill'd with water, there swam about an inch of that liquor above the upper surface of the wax. And, lastly, we took off, by degrees, the grain-weights that we had added, till we saw the wax, notwithstanding the adhering lead, rise, by degrees, to the top of the water; above which, some part of it was, visibly, extant.

From this experiment I argue, that, according to the Doctor's supposition, here was incumbent on the wax, a cylinder of an inch in height, and of the same diameter, or breadth, with the round surface of the wax; but, upon the removing part of the water that lay at the bottom, when the wax began to rise, there was incumbent no greater a weight than that of the collateral water; and as much of the upper and stagnant, as was directly incumbent upon that collateral water. But now, according to the Doctor's reasoning, this cylinder of water incumbent on the wax, being an inch deep, and above three inches broad, it must press the wax with a far greater weight than that which the lateral, and hollow cylinder of this stagnant water, could exert upon the rest of the collateral water; yet the height of this aggregate of collateral water, being the same with that of the wax, and the water swimming upon it; the difference of the pressure was so small, that, barely taking off a weight of four or five grains, the wax would, notwithstanding the pressure of the water incumbent on it, be impell'd up, and made to float; and, by the like weight put on again, it would be made to sink; and, by another removal of such a weight, it would, tho' slowly, re-ascend. And this phenomenon depends so much upon a mechanical balance of pressure, that even four grains would not have been necessary to make the wax rise or sink, were it not for some little accidental impediments, that are easily met with in such narrow glasses; for, otherwise, in a larger vessel, we have made the same lump of wax readily sink, or float, by putting on, or taking off, a single grain, or less.

Hence it appears, that, for the regulation of hydrostatical phenomena, nature has her balance too, as well as art; and that, in the balance of nature, the statical laws are nicely observ'd.

And,

An Hydrostatical Discourse.

And, still further to demonstrate, that, in stagnant water, the upper parts gravitate upon the lower; we took a very slender pipe of glass, whose cavity was narrower than that of an ordinary goose-quill, that heterogeneous liquors might not be able to get by one another in it. This pipe, near one end, was bent upwards, like a syphon, that it might have a short leg parallel to the longer; into this crooked pipe we put a little oil, and then held it perpendicularly in a deep, wide-mouth'd glass, fill'd with water, and a lump of wax, of the bigness and shape of that before-mention'd; so that the pressure of the incumbent water upon the open orifice of the shorter leg, might impel the oil into the longer, above the surface of the water in the vessel. The pipe being thus held upright, 'twas easy to take notice, by a mark fixed on the outside, to what height the oil reach'd in it.

Now, if we conceive a horizontal plane, parallel to the bottom of the vessel, to pass by the basis of the floating wax; 'tis evident, that, of this imaginary plane, the part on which the wax rests, is as strongly press'd by the weight of the wax, as the lateral part of the same plane is by the weight of the water incumbent on it; and, consequently, that part of this plane, which is placed directly over the orifice of the shorter leg of the pipe, is no more press'd, than any equal portion of that part of the same plane cover'd by the wax. This body, being taken out of the water, the liquor subsided a great way in the vessel; and so did, proportionably, the oil in the longer leg of the pipe. And, lastly, having weigh'd out as much water as we found the wax to amount to; this liquor was, instead of the wax, pour'd into that which remain'd in the glass: whereupon, the oil in the longer leg of the pipe, was again impell'd up to the former mark, to which the wax had rais'd it. Whence we may gather, that the water newly put in, tho', in the air, it weigh'd no more than the wax did, yet press'd the water that lay beneath the fore-said imaginary plane; and, consequently, that which was directly over the shorter leg of the pipe, as much as the wax before had done. And, since we have already prov'd, that the wax considerably press'd that plane; it cannot be deny'd, that the water did, in like manner, press that plane: and, consequently, that water may gravitate in water, as well as a solid body, such as is wax.

But, to rectify that plausible mistake, which has long deluded both philosophers and mathematicians, who think a body does not actually gravitate, when it does not descend; we have seen, that the immersed wax, and the brass-grains which lie on it, actually press, or gravitate, upon the subjacent water, and bottom of the vessel, on which 'tis incumbent; and, consequently, its pressure, not being surmounted by that of the collateral water, which is unable to raise it, must be as great as that of this collateral water. Therefore, when, upon the removal of a single grain, the wax, with its incumbent weight, is made to ascend, and that but very slowly; 'tis evident, that 'twas so far from not gravitating before, because it did not actually descend, that it retain'd its gravity even whilst it ascended; as may appear, not only by the slowness of its motion upwards, proceeding from

from its being in nature's scale very little less heavy than it need be, to balance the pressure of the collateral water; but by this also, that if but a single grain be laid on, when it begins to rise, its ascent will be check'd, and hinder'd; which could not be done by the addition of so inconsiderable a weight, if the wax, and the adhering metal, did not, even during their ascent, retain their former gravity; tho' that were frustrated as to the act of descending, or so much as keeping their station, by the prevailing pressure of the collateral water. So that, since the wax, and adhering metal amounted to considerably above four thousand grains; it did, in the balance of nature, weigh, whilst ascending, not so much as a four thousandth part less than it did, whilst it was actually descending.

Upon the whole, then, Dr. More, had he thoroughly consider'd the matter, need not have concluded the account of his experiment, as he does, by saying, "this is so evident a demonstration against the gravity of the parts of water downwards; that unless it prove true, I shall never assent to any reasoning of my own, for the future, nor to that of any other man whomsoever." But, I hope, he will consider, as well as I, that a man may be very happy in other parts of learning, who has had the misfortune to mistake in hydrostatics; a science which very few scholars have been at all vers'd in.

As for the last experiment in my hydrostatical paradoxes, the Doctor seems to suspect the matter of fact; or, supposing it true, accounts for it from his hylarchic principle, and the interposition of the valve of the instrument; but what considerable interest the imaginary retraction of the valve, or the air itself, can have in this phenomenon, I confess I do not discern: for, I think, the experiment would succeed, when try'd *in vacuo*, tho' all the atmospherical air were annihilated.

However, to avoid all mistakes, and disputes, that may arise upon account of the valve employ'd in our experiment; a quantity of quick-silver, being, by suction, rais'd into a very slender glass-pipe, whose upper orifice was stopp'd with the finger, to keep the mercury from falling out; we thrust the open end of the pipe, with the mercury in it, into a deep glass of water, till the little cylinder of mercury had, beneath the surface of the water, attain'd to a depth, at least fourteen times as great as the height of the mercurial cylinder; when, the finger being removed from the upper orifice, the glass-pipe will be open at both ends, and nothing can hinder the quick-silver from falling to the bottom, but the resistance of the cylinder of water that is under it; which cylinder can resist but by virtue of the weight, or pressure, of the stagnant water above it, tho' but collaterally placed: yet this water being, by the pipe, whose upper part is higher than the surface of that, and accessible only to the air, kept from pressing against the mercury any where but at the bottom of the pipe; and being about a fourteenth part of the weight of an equal bulk of mercury; it is able, at that depth, to make the subjacent water press upward against the mercury, which is but a fourteenth part as high as the water is deep, with a force equal to that of the gravity, wherewith the mercury tends

Water made to support a body of a much greater specific gravity than itself.

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downwards. And, to manifest that this phenomenon depends merely upon the equilibrium of the two liquors; if you gently raise the lower end of the pipe towards the surface of the water, this liquor, being not then able to exercise such a pressure as it could at a greater depth, the mercury preponderating, will fall out to the bottom of the tube. But if, when the quick-silver is at the first depth, instead of raising the pipe, you thrust it down farther under the water; the pressure of that liquor against the mercury, increasing with its depth, will not only sustain the mercury, but impel it up in the pipe to a considerable distance from the lower orifice, and keep it about the same distance from the surface of the water, which is laterally above it. And this experiment may not only serve for the purpose for which I here alledge it; but, also, if duly consider'd, and apply'd, very much illustrate and confirm the explanation, formerly given, of the seemingly spontaneous ascent of the clogg'd sucker in our exhausted air-pump.

The case of divers, with regard to the pressure of the water they sustain, at great depths.

The last argument which the Doctor urges against the gravitation of water, in its proper place, as they speak, is deduced from what happens to divers, who, in the midst of the sea, tho' salt-water be much heavier than fresh, do not find themselves compressed by the vast load of the incumbent water.

But if observations about diving were made by philosophers, and mathematicians, or, at least, by intelligent men; we should, I do not doubt, have accounts of it, very different from the current reports. A learned physician, of my acquaintance, upon diving leisurely, perceived a constriction of his thorax, by the action of the surrounding sea-water.

A Spanish prelate, who liv'd long in America, speaking of the Indians, employ'd by their inhuman masters in fishing for pearls, tells us, "it is impossible that men should live for any long season under the water, without taking breath; so that they die commonly thro' vomiting blood, and of the bloody flux." And a general of the English in the East-Indies, sent on an embassy to the emperor of Japan, speaks thus of some female-divers he met with in his voyage. "All along the coast, and so up to Ozaca," says he, "we found women-divers, who lived, with their household and family, in boats upon the water. These women," he says, "would catch fish by diving, which they missed by net and line, and that in eight fathoms depth. Their eyes, by continual diving, grow as red as blood; whereby a diving woman is distinguished from all others." However, the reason why so little damage is sustain'd by diving, seems, in my opinion, owing to the uniform pressure of the surrounding fluid, and the robust texture of a human body*.

The

* This is remarked by Sir Isaac Newton, that since fluids, in pressing bodies immersed in them, do not change the external figures thereof, they will not change the situation of the internal parts among one

another; and, therefore, that if sensation proceeds from the motion of the parts of animals; fluids will not damage the animals immersed therein, nor excite any sensation in them, farther than as their bodies

The swims of smaller fish appear very unable to resist compressure, being much more thin and delicate to the eye, than a piece of fine *Venice* paper; yet having caused one of these bladders, above an inch in length, and proportionably wide, to be taken out of a roach, and anointed with oil, to keep it supple, and preserve it from being pierced, or soften'd, by the water; and having, by a weight of lead, fasten'd to the neck of it, let it down to the bottom of a cylindrical tube of water, sealed at one end, and made large, and about fifty-six inches long; we could not perceive that, by the weight of all the incumbent water, it was manifestly compress'd, or that it discover'd the least wrinkle, or other depression of that very thin membrane, tho' fill'd but with air. And this trial was made more than once with the same success. Yet that this proceeded rather from the strength of the bladder, that was able to resist the weight of a taller pillar of water, than from the levity of water in the upper part of the tube on that in the lower, we shew'd, by presently letting down a mercurial gage, by a string, to the bottom of a tube: for the weight of the incumbent water forced up some of the mercury out of the open leg of the syphon into the seal'd one; and, consequently, compress'd the air included there: and the uncompressed air, being three inches and $\frac{1}{2}$ in length, we judg'd it, at the bottom of the tube, to be about $\frac{1}{2}$ impell'd up by the intrusion of the mercury. And, to satisfy myself, and others, that if the incumbent water had been heavy enough, it would, visibly, have affected the bladder, in spite of any *Principium hylarchicum*; we sunk it in a crystal glass, that had a long cylindrical neck, and was exactly fitted with a stopple; then a competent quantity of air being left above the water, the stopple was warily, and by degrees, thrust down; and so, lessening the capacity of the glass, compressed the air that was next it, and, by the intervention of that, the water under it. And tho' there did not, upon a slight compression of the outward air, appear any sensible effect upon the bladder that was at the bottom of the water; yet, upon a farther intrusion of the stopple, the pressure being increased, the immersed bladder discover'd two considerably deep wrinkles, that presently disappear'd upon drawing up the stopple; which, being thrust in again, the depressions were again to be seen on the swim. And, having convey'd a mercurial gage into the same glass, we estimat'd, by the condensation of the air in the seal'd leg of it, that the bladder had been expos'd to a pressure equal to that of a column of about forty feet of water.

This may lessen our wonder, that bodies, of so firm a texture as those of lusty men, support the pressure of the water at such depths as divers usually remain; since we see what resistance can be made by so exceeding

bodies may be condensed by compressure. *Newton. Princip. p. 264, 265.* And *Borelli*, having shewn, that a quantity of sand, contain'd in a very hard or rigid vessel, cannot possibly be divided, or entred, by a wedge; and that water, in like manner

contained in a bladder, every way equally compressed, can neither be contracted, bent, or otherwise distorted; hence deduces the reason why divers are not sensible of the pressure of the water. *De motib. nat. à gravitate factis. Prop. 29. 34.*

Vast pressures of a fluid sustain'd by weak and tender bodies.

STATICS. thin and delicate a membrane, distended only with the air, in comparison of the strong membranes and fibres of a man, fill'd, besides air, with more firm parts. 'Tis also considerable, what great weights may be sustain'd in the air, by such fibres or tendons, and by other fibres interwoven into membranes, in comparison of what an ordinary man would expect. And, not only upon account of the stable parts of a human body, but of the spirits too, it may resist very violent pressures of a fluid, without any manifest confusion, or dislocation of parts, or even sense of pain; as appears from the great effects which gusts of wind have upon trees, houses, &c. tho' a man will withstand the impetuosity of such a strong wind, and walk directly against it, by virtue of the vigour of his muscles and spirits, without being thrown down, or bruised, by so violent a current of air, and without so much as complaining of pain; and this, tho' the wind that beats against him, acts as a stream, and does not uniformly compress him, but invade only the fore-part of his body. Thus, also, in the lifting up of heavy weights by lusty men, we may see the slender tendons of the hand loaded with an hundred and fifty pounds, or more, without having their fibres so far compressed, or stretched, as to make the person complain of pain. A human body, therefore, is an engine of a much firmer structure, than mere scholars usually take notice of. And, I doubt, whether, if the structure of a man were not considerably firm, he could, especially in a deep sea, support the pressure of the water, tho' not immediately apply'd, without pain. For, having, several times, convers'd with a man who got his living by diving for ship-wreck'd goods; he assured me, that, when he stay'd at a considerable depth, as ten or twelve fathoms, under the surface of the sea, he felt a great pain in both his ears, which often put him to shifts to lessen it; and this, by his manner of describing it, I concluded, arose from the incompetent resistance of the air, which he found, by manifest tokens, to be greatly compressed by the upper water. This relation, from such a person, not only confirms our explanation, but likewise warrants us to doubt whether the common reports that are made concerning divers, ought to be rely'd on, without further examination.

We took a common flesh-fly, of a middle size, and having put it into the shorter leg of a bent glass, which we caused to be hermetically sealed, we added as much mercury as fill'd that leg, and a part of the other; leaving little more than an inch of air between the quick-silver, and the seal'd end, that there might be room both for the fly, and the condensation of the air; and then, with a little rammer, fitted for the purpose, we caus'd the mercury, in the open leg, to be thrust against that in the seal'd one; which necessarily crowded the air, near the fly, into less room; so that it seem'd condens'd into about a third part of the space which it possess'd before, and which it regain'd, when the rammer was withdrawn: and tho' this were done more than once, yet the fly appear'd not sensibly hurt; and I perceiv'd her, whilst she was pent up, to move her legs, and to rub them one against the other, as 'tis usual with that sort of insects in the free air.

Another

Another experiment, to the same purpose, we made with water; tho' this wet the wings of the fly, and soon after, by mischance, drown'd it: but we had, first, an opportunity to compress the air into a third, if not a fourth part, of its former dimensions; yet the fly continued to move several of her parts, and, especially her legs, very vigorously; as if nothing troubled her, but being, as it were, glued to the inside of the glass by her wetted wings. And this, I hope, will keep the resistance of divers, to the surrounding water, from seeming incredible; since such flies were able to resist, and, for ought appear'd, without harm, or pain, the pressure of the crowded particles of the air: tho' we guess'd it to have been as much compress'd by the force of the rammer, as it would have been by a cylinder of water, of between 50 and 60 feet high.

Hence, too, we may be assisted to conceive, how great a difference there is, whether the same pressure be exercised by a solid, or by a fluid body. For, according to our estimate, the pressure against the body of the fly was as great, as if a slender pillar of marble, having the fly for its base, and 18, or 20 feet in height, had rested upon the little animal.

We also took some ordinary black flies, of a middle size; and having placed one of them, with the head upwards, and left some distance betwixt her and the seal'd end of the glass tube, we pour'd in quick-silver very slowly, and cautiously, lest the force of so heavy a body, acquired by its velocity in the fall, shou'd, more than the mere weight of the fluid, oppress her: at length we got in as much mercury as the tube would receive; and then, holding it upright, we watch'd whether the fly would make any motions; and finding that she did manifestly stir, notwithstanding the incumbent mercury, we measur'd the height of the mercurial pillar, reaching from the middle of her body, to the top of the fluid, and found it to be about eight inches; and the quick-silver being pour'd out, the fly appear'd very lively, and vigorous.

We repeated the experiment with one of the best flies we could take, when their season was almost over, and of the same size with the former; when, ordering the matter so, that the mercury incumbent on her, appear'd to be of a greater height than that of the tube before employ'd, we saw her move one, or other of her legs, several times, tho' the tube was held upright; and therefore, measuring the height of the mercury above her, we found it to amount to above sixteen inches; then freeing her from this pressure, we observ'd, that she immediately found her legs again, and moved, up and down, briskly: tho, when she was afterwards oppress'd, with 23, or 24 inches of the same quick-silver, she gave no signs of life. But having got another fly, of about the same bigness; tho, when she was at the bottom of the quick-silver, she seem'd so compress'd as not to have any motion, yet, upon being taken out of the glass, she presently display'd her wings: tho' the pillar of mercury, that press'd upon her, amounted to above 27 inches.

Another

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Another fly, that seem'd but about half so big, as one of those hitherto mention'd, being well placed, with some mercury under her, in a glass pipe, held upright, sustain'd a mercurial pillar of 25 inches; and tho' she was not observ'd to move, under so great a weight, yet, when once that was taken off, she appear'd unhurt; and, probably, would have escaped under a much greater weight, if the tube, which was too large, had not, already, employ'd all the stock of mercury we then had at hand. Since, then, so small an animal, as a fly, may survive so great a pressure, and remain able to move such long and slender bodies, as her legs, when press'd against by above 16 inches of mercury; and, consequently, by a weight, equivalent to a pillar of water, of more than 18 feet and a half; which being 590 times her own length, and many times more her height, it appears, that a diver, six foot tall, to have as many times his height of water above him, as our fly might have had, and yet continue to have moved under it, must dive to near an hundred fathoms; which is a depth vastly greater than, from what I could learn by enquiry, the divers, either for coral, or pearl, descend to.

Thus, then, we have accounted for a phenomenon, which Dr. More thinks insoluble, without his hylarchic principle. There would, indeed, be much more weight in what he objects, if our assertion, of the gravitation of water in water, were, like the *Principium hylarchicum*, a mere hypothesis, advanced without any positive proof: but our doctrine is directly proved, by particular experiments; to elude the force whereof, so ingenious a person, is obliged to call in a principle that is not physical.

But, whatever power he is pleas'd to suppose at the bottom of the sea, to suspend the pressure of the incumbent water, I think, that supposition must give place to experience; which shews, there really is a great pressure exercis'd by the water at the bottom of the sea. A gentleman, who has been often president of the royal society, assured me, that a friend of his, having let down a pewter bottle into a deep sea, with weight enough to sink it, that he might try whether any sweet water would strain into it; found, when he pull'd it up again, the sides of it very much compress'd, and, as 'twere, squeez'd inwards by the water. An acquaintance of mine, trying to cool his liquor, when he sail'd thro' the torrid zone, by letting the containing vessels to a great depth into the sea, was, at first, amaz'd to find the corks, with which the strong stone bottles had been well stopp'd before, so forcibly, and so far thrust in, that they could scarce have been so violently beaten in with a hammer. And, an ingenious person, who practises physic in the *Indies*, told me, he try'd, in a very deep part of the sea, whether any fresh water would strain into stone bottles, thro' a thick cork, strongly stopp'd in; and having let it down with a convenient weight to 100 fathoms, was much disappointed, when he drew it up, by finding that the pressure of the water, at so vast a depth, had quite thrust down the cork into the cavity of the bottle: an effect which he scarce could have expected from the stroke of a mallet.

And,

And, to shew, *ad oculum*, that water may gradually, as it grows deeper, press against the stopple of a bottle, tho' the vessel be inverted; we took a glass vial, furnished with a cylindrical neck, and its cavity large, in proportion; into this we put as much quick-silver as would, in the neck, make a short mercurial pillar, of between half an inch, and an inch; then a piece of very fine bladder, dipped in oil, was so tyed over the orifice of the glass, that no mercury could fall down, or get out, nor water get in at the orifice; and yet the bladder, by reason of its great limberness, might be easily thrust up towards the cavity of the vial, or depress'd by the weight of the mercury. This little instrument, first furnish'd with a weight of lead, to sink it, being inverted, the mercury descended into the neck, and closed the orifice as exactly as a stopple; but, with its lower part, depress'd the bladder beneath the horizontal plane, that might be conceiv'd to pass by the orifice: then the glass, being, for a while, kept in the water, and, by a string, let further down into the same glass vessel, fill'd to about two feet in height; the pressure of the liquor against the orifice of the vial, by degrees drove up the bladder, and the mercurial stopple into the cavity of the neck; as was manifest by the ascent of the quick-silver: and when the instrument was leisurely drawn up again, the weight of this mercury made it subside, and plump up the bladder, as before.

Meeting, casually, with an ingenious mechanic, who devised proper accommodations, and a boat, wherewith he could continue, for a great while, at a considerable depth under water; he assured me, that when he was about four or five yards deep in the river *Thames*, his breast and abdomen were so compressed, that, there being hardly room enough left for the free motion of his lungs, he could scarce fetch his breath; and was obliged to be drawn quickly up: and that, to remedy this, he caused a kind of armour for the chest and back, to be made of copper; and tho' the metal defended him from any mischief in those parts, yet, in others, where only leather was interposed, when he came to the depth of about six fathoms, he found a great pressure upon his legs and arms, and all the other parts, against which the water was able to thrust the leathern suit inwards: and this pressure he found pretty equal; so as to receive no great inconvenience from it; being able to continue under water, tho' not at any great depth, for about two hours. He farther declared, from his own experience, that the ambient water endeavour'd to press him, and his diving suit, every way inwards. I have, also, been assured, that a profess'd diver, when he descended in his bell, to very great depths, has often had the blood squeez'd out at his nose and eyes.

Upon the whole, it appears, that water actually presses against bodies under it, whether specifically lighter, or heavier than it self.

SECT. II.

Water actually
weigh'd in wa-
ter, by common
scales.

TO my particular method of actually weighing water in water, it has been objected, by a late writer of hydrostatics, that there is a mistake in it. I shall here, therefore, endeavour to clear this matter, and set my thoughts about it in a fuller light.

My opinion is, that water, as it is a heavy fluid, always retains its gravitation, and power of pressing; (by which, I mean, a tendency downwards, whatever be the cause of that gravity) whether it have a body under it, either specifically heavier, or lighter than it self, or one equi-ponderant to it. For I see not what should destroy, or abolish this gravity, tho' many things may hinder some effects of it. And, therefore, I suppose that water retains its gravity, not only in air, but in water too, and in heavier liquors; and, consequently, by virtue of this, that liquor presses upon them. But, if a surrounding fluid have, upon account of its specific gravity, an equal, or a stronger tendency downwards, than water, it will, by virtue of that, be able to impel up this liquor, or to keep it from actually descending; so that a portion of water, supposed to be included in a vessel of the same specific weight with water, will, placed in a greater quantity of the same water, neither rise nor fall, tho' it retains its gravity there; only this gravity is kept from making it actually descend by the contrary action of the other water, whose specific gravity is supposed equal: as when a just balance is loaded with a pound weight, in each of the scales, tho' neither of the weights actually descend, being hinder'd by its counterpoise; yet each retains its whole weight, and, with it, presses the scale it rests on: so that our included portion of water, really presses the subjacent water, tho' it does not actually depress it. Nor do I think that the only way of judging whether a body gravitates, is to observe, whether it actually descends; since, in many cases, its gravity may be proved, by the resistance it makes to heavy bodies, which would otherwise raise it; as appears by equal weights in a balance. And, for want of this distinction, I have known even learned men, treating of hydrostatics, mistake the question.

Now, the adversaries I had to deal with, both in print, and in discourse, deny'd, that in standing water, the upper press'd, or gravitated upon the lower parts. And tho' they could not but grant, that the whole weight of the water gravitated upon the bottom of the vessel; yet they would have the parts of it to do so *actione communi*, as they speak; and fancied I know not what power of nature, to keep the homogeneous portions of water, as well as other elements, from pressing one another, whilst it is in its proper place. Against this opinion, it was alledg'd, (besides other things, which I found many, otherwise good scholars, were not fitted to understand) that if a glass vial, or bottle, well stopp'd, were deeply immersed under water, it would strongly tend upwards; but if it were dextrously unstopp'd, when 'twas thus immers'd, so that the water cou'd get in; allowing for the weight of the glass itself; 'twould, by the water that crowds

crowds in, and thrusts out the air, be made strongly to tend downwards, and continue sunk. But this not satisfying, because 'twas pretended, that the reason of the empty bottle's emerging, when stop'd, was the positive levity of the air that fill'd it; and that the sinking of it, when unstop'd, was from the recess of the same air, which, by the intruding water, was driven, with large bubbles, out of the bottle: I thought this evasion might be obviated, by contriving an experiment, wherein the water should be plentifully, and suddenly admitted into the glass, and yet no air expell'd out of it; so that, if then the glass, which was sustain'd before, should fall to the bottom, with a gravitation amounting to a considerable weight, in respect of its capacity, the sinking of it could not be ascribed, as before, to the recess of the air, endow'd, as they suppose, with positive levity; but to the weight of the water admitted, which, when thus weigh'd, would be environ'd with water of the same kind: and to shew, that this water might have a considerable weight, notwithstanding the place it was in, I employ'd a pair of scales, after the manner recited in the experiment, page 288, Vol. II.

However, therefore, my expressions disagree with those of my adversary, the distance of our opinions is not so wide, as it seems at first sight: for, he allows, as well as I, that the superior parts of water do, by their gravity, press the inferior; but this he would not have amount to so much as to mean, that water weighs, or gravitates in water. But, if he thinks, that, in my experiment, I meant to propose a method of making water descend in water, and weigh it in that liquor, with a pair of scales, just as I would a piece of lead, or a portion of mercury; which are bodies much heavier *in specie*, than water; either he mistakes my intention, or I did not sufficiently declare it. What I design'd to shew, and, I think, have shewn, was, that by the help of an ordinary balance, it may be made appear, that water, admitted into the glass bubble, I employ'd, caus'd it to weigh much heavier than it did before that liquor enter'd into it; and that this new weight, manifested by the balance, was not due, as my adversary supposes, to a recess of the air.

To this experiment, our author objects the following. "Take a piece of wood, lighter *in specie* than water, and add weight to it, by degrees, till it become of the same weight with water; fasten it, with a string, to a balance, and weigh it in water, and you will find the whole weight supported by the water." But this is so far from overthrowing my opinion, that it agrees very well with it. For, suppose the weight, added to the light wood, be lead, it cannot be said, that the metal loses its native ponderosity, whilst it rests in the water; and the reason why it descends not, is, that it, and the wood whereto it is joined, are hinder'd by the counterpoise of the collateral water, which, by its pressure, would raise the surface of the water whereon the floating body rests, if it were not hinder'd by the weight of these incumbent solids: and this resistance of theirs, to the endeavour of the water upwards, being exercised only upon account of their gravity, shews, that they do, in my sense, gravitate, tho' not pre-gravitate.

STATICS.

The relative
levity of bodies
under water.

'Tis obvious, that if wood, wax, or other bodies, lighter in *specie* than water, be detain'd under it, they will, upon removal of the force, emerge to the top. And this they do so readily, and, as it seems, spontaneously, that the generality of philosophers, both ancient and modern, ascribe it to an internal principle, which they call positive levity.

But this principle was not, always, so universally receiv'd among philosophers, as in later ages; *Democritus*, and several of the antients, admitting no absolute, but only a relative levity: which opinion, some of the moderns have attempted to revive.

The experiment usually urged, to prove the positive levity of wood in water, seems, to me, too slightly made, to be acquiesced in. 'Tis true, indeed, if a flat board, for instance a trencher, have its broad surface kept by a man's hand upon the horizontal bottom of a bucket, full of water; when the hand that detain'd it, is removed, the trencher will commonly soon ascend to the surface of the water. But, I do not perceive, that a decisive experiment, of this kind, is easy to be made with such materials: for, the wood, whereof both the trencher, and the bottom of the bucket consist, are supposed to be lighter, in *specie*, than water; and, consequently, they must be of a porous, and lax texture; whilst the more solid woods, as *Lignum vite*, brazil, &c. whose texture is closer, sink in water. If, therefore, there be not much care used, to bring the surfaces of the trencher, and the bottom of the bucket, to a true flatness, and smoothness, the experiment will not be accurately made; and, perhaps, tho' it be mentally, yet it is scarce practically possible to bring such porous bodies, as the lighter woods, to such a contact as is necessary in this case. And, were that actually done, I should not expect, that the trencher would ascend. For, in my opinion, the cause why, in ordinary instances, wood, wax, and other bodies, specifically lighter than water, being let go at the bottom of a vessel, full of that liquor, emerge to the top, is chiefly, that there is no such exquisite congruity, and contact, between the lower superficies of the wood, and the upper of the bottom of the vessel; but that the lateral parts of the water, being impell'd by the parts of the same liquor incumbent on them, are made to insinuate, and get between the lower parts of the wood, and the bottom of the vessel, and so lift, or thrust upwards, the wood, which, bulk for bulk, is less heavy than the water that extrudes it.

But, as the whole of the argument of those I dispute with, consists in a supposition, that because the trencher is placed on the bottom of the vessel, no water can come between to buoy it up; whence they conclude, it must ascend by an internal, and positive principle of levity; I made the experiment after another, and, if I mistake not, a better manner.

We took, then, two round, cylindrical plates of black marble, which had those superficies that were clapp'd together, ground very flat, and carefully polish'd, that they might touch in as many parts as possible; and that the upper being taken up, the other might stick to it, and ascend with

it:

Shown by experiments, which overthrow the doctrine of positive levity.

it: and the better to keep out the water, the internal surfaces were, before they were put together, lightly oil'd; which did not hinder them from most easily sliding along one another, as long as the contiguous surfaces were kept horizontal: then a blown bladder, of a moderate size, was fasten'd to the upper marble, and both of them were let down to the bottom of some stagnant water; where, by the help of an easy contrivance, the lower marble was kept level to the horizon. And now the patrons of positive levity would have concluded, that the bladder, being a body much lighter than wood; and being in an unnatural place, beneath the surface of the water, should, of its own accord, and with impetuosity, emerge: but I expected a contrary event, because the bladder, being ty'd to the upper marble, so that both of them might, in our case, be consider'd as one body; the water could not impel them up, since the close contact of the surfaces of the two marbles, kept the water from insinuating between them; and, consequently, from getting beneath the upper marble, and pressing against the lower superficies of it. And, to shew that this was the reason why the bladder did not emerge, I caused part of the oil'd surface of the upper marble to be slid off from that of the lower; which, by reason of the smoothness and slipperiness of the surfaces, was very easy to do. But the contact still continuing in a greater part of the surface than was requisite, more and more of the upper marble was gradually slipt from the lower; till, at length, when the marbles touch'd but in one half of their surfaces, the endeavour of the water to force up the bladder full of air, being stronger than the resistance which the contact but of part of the surfaces of the stones, was able to make, they were suddenly disjoin'd; and the bladder, by the extruding water, impetuously shot up above the top of the water.

With the same marbles, we made several other experiments of this kind, most commonly letting them down both together; but once or twice, at least, by placing the upper marble, under water, upon the lower, already fix'd to the bottom of the vessel.

That 'twas not the weight of the upper marble, nor want of lightness, whether positive or relative, of the air included in the bladder, that kept it from ascending, is manifest by the impetuous ascent of it upon disjoining the marbles; and by this, that the bladder would lift up, from the lower parts of the water, not only the upper stone, when it touched not the other, but a weight of seven or eight pounds hanging at it.

We, also, took a bladder, in great part freed from its air; and tying the neck of it very close, that none of the remaining air might get out, we fasten'd a considerable weight to it; and, by the help of this, sunk the bladder to the bottom of a wide-mouth'd glass, full of water, that the surface of the liquor might be considerably higher than the upper part of the bladder. This wide-mouth'd glass we included in a great receiver, which was carefully cemented on to the engine. The principal design of the experiment was to shew, that tho' the air included in the bladder, was very far from being able, by its absolute levity, to lift up so great a weight

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But if, instead of hanging so great a weight at the neck of the bladder, we fasten'd only a moderate piece of lead, such as would only serve to sink the bladder, and keep it at the bottom of the water, so that the aggregate of the bladder, air, and metal, was but a little heavier than an equal bulk of water; then, upon the first stroke of the pump, which could withdraw but a small part of the air in the receiver, that in the bladder suddenly expanding itself, the whole would immediately be impetuously extruded by the water; tho', after some reciprocations, it floated in its due position; but, upon the return of a little outward air, it would immediately subside.

It may be objected, that the ascent of the weight was not effected by the pressure of the water; because rarity and levity, being similar qualities, the great rarification of the air might, proportionably, increase the levity of it; and, consequently, enable it to perform much greater things than it could do before. I shall answer this objection by the following experiment.

About the neck of a conveniently shaped vial, able to hold a few ounces of water, I caus'd to be carefully ty'd the neck of a small bladder, whence the air had been well express'd; so that the bladder, being very limber of itself, and, probably, made more so, as well as more impervious to the air and water, by the fine oil we had caus'd it to be rubb'd with, lay upon the orifice of the vial, like a skin clapp'd together, with many folds and wrinkles. This done, we let down the vial into a vessel full of water; and the vial being poised for that purpose, sunk perpendicularly in the liquor, till its neck was partly above, and partly beneath the surface of the water: then, covering the external glass with a large receiver, we caus'd the air to be pumped out; and as the pressure of that was gradually withdrawn, the air in the floating vial expanded itself into the bladder, and unfolded the wrinkles of it, till, at length, it became full blown, without altering the erect posture of the glass it rested on. But this

this great expansion, being made above the water, and, consequently, in a medium, not heavier than the included air, gave that highly rarify'd air no such increase of levity, as shew'd that it made so much as the neck of the glass rise higher in the water, than it did before. Nor did we observe, that the return of the air into the receiver, by reducing the air in the bladder to its former unrarify'd state, made the glass sink deeper than before. But when the experiment was try'd with the same glass and bladder, at the bottom of the water; upon pumping out the air, the bladder, being dilated under water, was, after a while, carried up to the top, and took up with it about eight or ten ounces, that, to clog it, had been fasten'd to the bottom of the vial.

But, to examine whether a portion of air, by being rarify'd, or expanded, acquires a new and proportionable degree of positive levity; we took a bladder, of a moderate size, which was very fine and limber, that it might be the lighter, and more easily distended. The greatest part of the air, being squeezed out of the bladder, the neck of it was tied up very close, that no air might get in or out of it. This limber bladder was hung at one of the scales of a balance, whose beam had been purposely made more than ordinarily short, that the instrument might be suspended, and continue capable of playing in the cavity of a great receiver, into which we convey'd it; having, first, carefully counterpois'd the bladder with a metalline weight, in the opposite scale. This done, the air was pumped out, and as that was withdrawn, the bladder was gradually expanded by the spring of the internal air; till, at length, when the receiver was well exhausted, it appear'd to be quite full. But, notwithstanding this great dilatation of the included air, it did not appear, by the depression of the opposite scale, to be grown manifestly lighter than it was at first. And the bladder seem'd, also, to retain the same weight, after it had, by the air that was let into the receiver, been compressed into its former flaccid state.

To determine whether a fish rises or sinks in water, by contracting, or expanding itself; take a bolt-head, with a wide neck, almost fill it with water, and put therein a large live fish; then draw out the neck of the glass exceeding slender, and fill that also with water. Observe the motion of the fish; and if, upon his sinking, the water, at the slender top, subsides; we may infer, he contracts; and if, upon his rising, the water be raised, we may conclude, he dilates himself.

I employ two sorts of experiments, to shew, that a small quantity of inclosed air, may, by its pressure, have a considerable effect upon bodies under water, notwithstanding the interposition of that liquor. For this pressure may be manifested by what it directly, and positively, performs upon bodies cover'd with water; and, also, by the phenomena that regularly ensue upon the removal of the inclosed air, or the weakening of its spring.

We took a square glass-vial, capable of holding twelve ounces of water; the neck of this we luted on carefully, and strongly, over the orifice of

The pressure of the air's spring on bodies under water.

Manifested by experiments.

STATICS. of the small pipe, at which the air passes in our engine, out of the receiver into the pump: then, covering this glass with a great receiver, we luted it strongly to the engine, and, at the top, pour'd in as much water as sufficed to surround the internal receiver, if I may so call it, and cover it to a pretty height. This done, we exactly closed, with a turn-key, the hole in the great receiver, at which the water had been pour'd in, that no air might pass that way. And, lastly, we began to pump out the air contain'd in the internal receiver; that the air, which, by the above-mention'd pipe, had communication with the external air, might no longer, by its pressure, assist the glass to resist the pressure which the incumbent, and inclosed air, by virtue of its spring, constantly exercises upon the subjacent water, and, by its intervention, upon the sides and bottom of the internal receiver. At the first exsuction, which could not be supposed to have well emptied the internal glass, this vessel was, by the pressure of the superior air upon the surrounding water, broken into numerous pieces. The same experiment, with a like success, was repeated with a stronger internal glass.

Having squeez'd out of a moderately sized bladder, the greatest part of its air, we tied the neck of it very close; and then, fastening to it a competent weight, we placed it at the bottom of the tallest, and largest glass we could cover with our great receiver; that so, tho' the incumbent air were pump'd out, none of the water might escape with it, but still retain the same height above the bladder. Having, then, poured upon the bladder as much water as would swim high above the upper part of it; we cover'd this glass of water with a great receiver, which, being carefully cemented on to the engine, the pump was set on work; and as the air which, by its spring, press'd upon the surface of the included water, was, by degrees, pumped out; so the air, imprison'd in the bladder, gradually expanded, at the bottom of the water, as if no such liquor had interposed between them, otherwise than by its weight; upon whose account it must be allow'd a little to hinder the expansion of the bladder, in proportion to the height it has above it. Thus the immersed bladder was, at length, full blown, by the dilatation of the air inclosed in it; and, by its intumescence, made a considerable part of the water run over the sides of the glass that before contain'd it all. And when access was given again to the external air, the internal being compressed, the bladder was presently reduced to its flaccid state.

We took a small fine bladder, whose neck was strongly ty'd up, when it was about half full of air; this we put into a short brass-cylinder, the lower of whose bases was closed with a brass-plate, and the other left open; this open orifice we afterwards stopp'd, but not exactly, with a cylindrical plug, that was somewhat less than it, and was, by a rim at the top, hinder'd from reaching too deep into the cavity of the cylinder, that it might not damage the bladder which lay beneath it: upon the plug we placed a conical weight of lead; and this pile of several things, being so placed upon our engine, that we could cover it with a great receiver, we carefully

fully cemented on, such a vessel, and at the top of it pour'd in so much water as would serve to fill the vacant part of the brass-cylinder, and the cavity of the engine to such a height, that it cover'd all the leaden weight, which was several inches high, except a rim fastened to the top of it, for the conveniency of removing it. This being done, the pump was work'd; and, long before we had exhausted the air of the receiver, that which was inclosed in the lank bladder had, by degrees, display'd so vigorous a spring, that it rais'd up the weight, which lay upon it, to a considerable height, and kept it there, till the air was let in from without, to assist its deprefure by the lead, which amounted to no less than about twenty-eight pounds.

We took a cylindrical copper-vessel, of a considerable height; into this, being first almost fill'd with water, we put a square glass-vial, capable of containing nine or ten ounces of water, and exactly stopp'd with a cork, and a close cement; this vial, by a competent weight, was detained at the bottom of the water, from whose upper surface it was considerably distant; then the copper-vessel, being placed upon the engine, and included in a great receiver, well cemented on; the air was, by degrees, pumped out: but, before it was quite exhausted, the glass, at the bottom of the water, was, by the spring of the air included in it, burst into many pieces, not without great noise, and a kind of smoke, or mist, that appeared above the surface of the water: and another glass, of the same sort, was broke, after the same manner, in another vessel.

Now, the consequence that naturally results from the three last experiments, is, that, since barely upon withdrawing the pressure of the included air, the air residing in the immersed bodies, did, by virtue of its spring, expand itself so forcibly, and perform such considerable things; the air above the water must have exercised a very strong pressure upon the surface of it; for, it must have been, at least, equivalent to that force of the immersed air, the exercise whereof it was able totally to hinder.

And, from hence, it may be easily deduced, that the weight of the atmosphere acts upon bodies under water; tho' the interposed liquor is vastly heavier, *in specie*, than air: for, we have just now proved the pressure of inclosed air, which consists in its spring, upon bodies under water. And, 'tis manifest, that the strength of the spring of this lower air, we make our trials with, is caused by the weight of the upper, which bends and compresses those little springy particles whereof our air consists; so that the weight of the atmosphere, being equivalent to the spring of the lower air, must press upon the surface of the subjacent water, with a force equal to the spring of that part of it which is contiguous to the water.

It has proved a great impediment to the reception of the doctrine of the weight and pressure of the atmosphere, that, if it could, really, exercise so great a force as we ascribe thereto, it would, unavoidably, oppress all the bodies exposed to it: so that neither men, nor other animals, would be able to move under so great a load, or withstand its force.

Fluids press in a different manner from solids,

This,

STATICS.

Whence, the weight of the atmosphere is not prejudicial to the bodies of animals.

This I grant to be a plausible objection, but the force of it will be taken away by the following considerations put together.

And first, the power of pressing, that we ascribe to the air, is not a thing deduced from doubtful suppositions, or bare hypotheses, but from real and sensible experiments. And, therefore, since we have clear and positive proofs of the pressure of the air; tho' we could not explain how men and other animals are not destroy'd by it; yet we ought rather to acknowledge our ignorance in a doubtful problem, than deny what experience manifests to be true. Thus the attractive virtue, and other powers of the loadstone, are freely acknowledg'd, even by those who confess themselves unable to explain them; tho', if experience did not satisfy us of them, they were liable to many more considerable objections, than any urg'd against the pressure of the air.

But those who make this objection against the great pressure of the air, seem not aware, that we were conceiv'd, and born in places expos'd to the pressure of the atmosphere: and, therefore, how great soever that pressure appears to be, it ought not to crush us now: since, when we were but embryos, or newly born, we were able to resist it; and not only live, but grow to all dimensions, in spite of it.

If there were any place about the moon, or other celestial globes, which some suppose to be inhabited, that had no atmosphere, or equivalent fluid, about it, and where men, yet, could be generated a-new; and one of these men should be transported thence, and set down upon our earth, there might be made an experiment proper to decide our controversy. In the mean time, I doubt, since nature is not observ'd to make things superfluously strong, such a human body, not being design'd to resist any weight or pressure of air, would be of so tender and compressible a make, that it would easily be crush'd inwards, by our atmospherical pressure. And tho' we cannot give an instance of this kind, yet we make trials somewhat like it, in our pneumatical engine. For when we place water in our receiver, and pump out the air that was above it, there will be generated a multitude of bubbles; some of which, when the air is carefully withdrawn, swell to a strange, and scarce credible bulk. These bubbles being generated where the air cannot press upon them, such dimensions are so natural to them, that if the receiver be stanch, and no other unfriendly accidents intervene, they would probably be durable; since I have known the spring of highly-dilated air continue for many months: and a bladder would, for no less time, continue blown and fill'd, in our vacuum, by a little air left in it, when the external was withdrawn from it. Yet the large bubbles, when once the outward air is suffer'd to come in upon them, are thereby so violently compress'd, that in a trice they shrink into dimensions too small to keep them visible. And if I could have succeeded in my attempt of producing living bodies in our vacuum, I suppose the success would have confirm'd our doctrine.

You will tell me, that so great a weight and pressure, as I assign to the atmosphere, must needs make a man feel pain; and if not dislocate some
of

of the parts, at least press the whole body inward. But, 1. being accus-
tom'd to this pressure from our very birth, and even before it, we are
hinder'd from taking notice of it; those pressures only being sensible to us,
that are made so by some additional cause, which, by making a new im-
pression, excites us to observe it. Thus, we are not sensible of the weight
of the clothes we wear; and so a healthy man is not sensible of the heat in
his heart, because 'tis constant there, and the sentient parts of the heart
have been used to it, tho' that heat has been often very considerable; but
when, in live dissections, a man puts his finger into the heart of an ani-
mal, which probably has a fainter, or at least no stronger degree of heat,
than a human heart, he will feel in his fingers, accusom'd to the air, a
manifest degree of heat. 2. It has been proved, that a cubic inch of air,
for instance, has a spring sufficient to resist the weight of the whole atmo-
sphere, as far as it is expos'd thereto; otherwise it would be more
compress'd than in fact it is. 3. A very little portion of air, tho' it much
sooner loses its spring by expansion than a greater, yet will resist further
compression, as much as a greater. 4. The pores of the parts of animals,
whether fluid or consistent, as in their blood, gall, urine, heart, liver, &c.
there are included a multitude of aerial corpuscles; as appears from the
numerous bubbles afforded by such liquors, and the swelling or expansion
of the consistent parts in our exhausted receiver. 5. To this we may add,
that besides the bones, whose solidity is not question'd, a much greater
part of the human body than is usually imagined, really consists of mem-
branes and fibres, and the coalitions and contextures of them; and that
these substances are, by the most wise author of things, made of a much
closer and stronger texture, than those, who have not try'd, will be apt to
think. Lastly, there is a far greater difference than men commonly sus-
pect, between the effects of the pressures made upon bodies by solid
weights, and those they suffer from surrounding fluids.

Hence 'tis not necessary that the pressure of the atmosphere, tho' as
great as we suppose it, should oppress and crush the bodies of men, who
live under it; for the solidity of the bones, and the strong texture of the
membranes and fibres, with the spring of the aerial particles, (which abound
in the softer as well as in the fluid parts of bodies) that is equivalent to the
pressure of as much of the atmosphere, as can exercise its pressure a-
gainst them; make the frame of a human body so firm, that it may well
resist the pressure of the outward air, without having any part violently
dislocated; whilst this external pressure is exercised only by the air, which
being but a surrounding fluid, presses equally on every side. And be-
cause our bodies have been produced in the atmosphere, and from our very
birth expos'd, without intermission, to the pressure of it; our having been
continually accusom'd to this pressure, and the firmness of our structure,
keep us from being sensible of the weight or pressure.

Multitudes of men have had occasion to pass over high mountains,
and I myself have been upon the *Alps* and *Apennines*; yet I never met
with, nor heard of any one, who took notice of the difference, as

to the weight of air sustain'd; or who complain'd, that, when come down to the foot of the mountain, they felt any greater compressure from the air, than at the top. Yet, numerous experiments witness, that on more elevated parts of the earth, which have a less height of the atmosphere incumbent on them; the weight and pressure of the air is not so great as below. And, on very high mountains, 'tis not unlikely that this difference may be very considerable; since, when the *Torricellian* experiment was made near *Clermont* in *France*, upon the *Puy de Domme*, which is none of the highest mountains in the world, they found the difference of the mercury at the top and bottom, to amount to about three inches; and, consequently, if the trial had been made with water instead of quick-silver, the difference would have been about three feet and a half, perpendicular height. And, on much higher mountains, the difference of the height of the mercurial cylinder at the top and bottom, would be much greater. But, at the bottom of some very deep well, or mineral groove, which may, without improbability, be supposed placed at, or near the foot of one of these mountains; if we conceive the barometer to be let down, the variation would be much more considerable; yet we find not that the diggers in the deepest mines, of mountainous countries, are sensible of being depressed, or compressed, by any unusual weight. But, to build only on matter of fact; it appears by the preceding observations, that when a man was at the bottom of the hill, he had an additional weight of air pressing upon his head, above what he sustain'd at the top, equal to the height of an imaginary vessel of water, having his head for its basis, and three feet and a half high; which is so considerable a weight, as could not but have been, of itself, very uneasy to support. And the same must be, proportionably, applied to the other parts of his body. Whence, we may infer, that such a weight was not felt by the man it compressed, because the pressure was exercised after the uniform manner of fluid bodies.

And this may suffice to shew, that there is no necessity the compressure of the atmosphere should make it impossible to live in it. But, to clear this matter still farther, we made the following experiments.

Experiments to illustrate and confirm this doctrine.

Into a short hollow cylinder of brass, we put a fine bladder, ty'd so close at the neck, that none of the air, whereof it was about half full, would, probably, get out. This we did, to the end that the hen's egg we were to bed in it, might lie soft, and have its sides almost cover'd with the limber flaccid bladder, and contained air; then we cover'd the remaining part of the egg with another bladder, that nothing hard might come to bear immediately upon the shell; when, putting a wooden plug into the cylinder, and a weight upon the plug, tho' slowly and warily, lest the quick descent of the weight should make the plug break the egg; we, in the last place, cover'd the cylinder with a large receiver, and the air being drawn out, that air which was ty'd up in the bladder, by degrees, expanded itself so strongly, as to lift up the plug and the incumbent weight, to a considerable height, and keep it there, till the external air was re-admitted.

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Now, since the air, in such cases, expands itself, vigorously, every way, it must press against the egg with the same force that it did, proportionably, against the bottom of the plug; and that force was more than sufficient to lift up the weight, which, together with the plug, amounted to about thirty pounds: yet the egg, being taken out, appear'd perfectly whole, and no way harmed. But the same egg was afterwards crush'd to pieces, by laying, warily, small weights, one after another, upon it, till they amounted to about four pound weight.

We took a glass-bubble, about an inch and a half in diameter, blown at the flame of a lamp, that it might be far more easy to break than the thinnest vial; this bubble we included between bladders, as we did the egg in the former experiment; and then, having warily put the plug into the cylinder, so as to press upon the bladder that surrounded the glass, we leisurely put the weights upon the plug, till they, together with it, amounted to thirty pounds, or more; which being removed, the plug was removed; and the glass-bubble, tho' it were, perhaps, as thin as fine white paper, was taken out whole.

But, lest the great resistance of so thin a glass, which yet was not hermetically seal'd, should be ascribed to its spherical figure; we employ'd, instead of it, the shell of an egg, whence, by a hole made at one end, the yolk and white had been taken out. This empty and imperfectly closed shell, we treated as the glass-bubble, in the last experiment; and, notwithstanding the great leaden-weight that rested upon the plug, it escaped without the least crack.

And, to shew that what we observ'd about the nature of the compression of fluid bodies, will hold as well in water as air; we put an hen's egg into a limber bladder, almost full of water; and tying the neck very strait, that nothing might get in or out, we so placed the bladder in the brass-cylinder, that the egg might not be immediately touch'd by any thing that was hard; then putting the plug into the cylinder, we warily and leisurely heaped upon it convenient weights of lead, till they amounted to about seventy-five pounds: notwithstanding all which, the egg was taken out sound, and uncrack'd; and, probably, might have supported a much greater pressure.

Nor were eggs the only bodies we endeavour'd to crush after this manner: the trial having been, also, made upon a piece of butter, of a very irregular shape, it came out unalter'd.



A N

Essay-Instrument,

Wherewith to examine if COIN be adulterate or counterfeit.

*An easy method
of determining
the specific gra-
vities of fluids
and solids.*

HAVING, formerly, used a little instrument, consisting of a glass-bubble, with a long slender stem, to compare the specific gravities of several fluids, by its degrees of sinking in them; and, coming to consider its uses more attentively, I found it, also, proper to discover the specific gravities of several solids. For, every solid, heavier than water, loses in water as much of the weight it had in air, as a bulk of water, equal to the immersed solid, would weigh in the air. And, consequently, as gold is by far the most ponderous of metals; a piece of it, and one of equal weight of any other metal, being produced, the gold must be the least in bulk. If both of them, therefore, be weigh'd in water, the gold must lose less of its former weight than the other metal; because both grow lighter herein by the weight of a bulk of water, equal to each respectively. Hence, I concluded, that my floating instrument would be made to sink deeper by an ounce, for instance, of gold hanging at it under water, than by an ounce of brass.

This instrument may be made of glass, copper, silver, or almost any other solid body, that is fit to float on water, with a piece of coin hanging at it, and of a texture close enough to keep out the water. It consists of three parts, the ball, the stem, and the place for the coin. The ball BCDE, if of metal, consists of two thin concave plates, folder'd together in the middle; and at the most distant parts from the juncture, there ought to be left two opposite holes, one in each plate, for the two other parts of the instrument. The ball should not be exactly round, but of any fit shape, to keep the instrument erect and steady in the water; and it must contain air enough to prevent the whole instrument, when loaded, from

from sinking below the top of the stem. The stem A B, must be solder'd on to the ball, at the upper hole thereof. It may be either hollow, or solid; but it ought to be made very slender, and considerably long. In the lower hole of the ball, is solder'd the undermost part of the instrument, which I call the stirrup G. The screw F, is a very short piece of brass, with a broad slit in it, capable of receiving the edge of the coin; which, with a turn or two of a slight small side-screw, may be kept fast in. The stirrup G, is made of wire, which, a little below the bottom of the ball, is bent round, so as to stand horizontally, that the coin may be laid upon it. 'Twould be convenient to have the under stem and screw made separate, that the ball of the instrument, being sufficiently large, there may be room for two or three flat round pieces of metal K, with each of them a hole in the middle, fitted to the size of the stem, to serve as balast; being placed near the lower part of the ball, when the screw may be thrust on to support them. Fig. 28.

To adjust this instrument for the examination of a guinea, you must, by the help of the stirrup, or screw, hang a guinea, known to be genuine, at the bottom of it; and having stop'd the orifice of the stem, if hollow, that no water may get in, immerse the instrument gently, and perpendicularly in a vessel of fair water, almost to the top of the stem; then letting it alone, if, being settled, it continues in the same station, and posture, the work is done. If it emerge, add a little weight to it, either at the bottom, or the top, that the guinea may depress it as low as is required; but if it sink quite under the water, lighten it, by filing off from the balast-plates, above-mentioned, or otherwise. Then let a mark H, where the surface of the water touches the stem; and taking out the instrument, substitute in the place of the guinea, a little round plate of brass, of the same weight, or a grain or two heavier, in the air; and putting the instrument into water, as before, let it settle, and make another mark I, at the intersection of the stem, and the horizontal surface of the water. Applied to the examination of coin. Fig. 29.

But there may happen a case, wherein the practical application of our instrument will be unsecure. For, if a falsifier of money have the skill, by washing, or otherwise, to take off much of the substance of the coin, without altering or impairing either the figure, or the stamp; the coin will not depress the instrument to the usual mark, whence the guinea may be judg'd counterfeit, tho' it be only too light. However, it presently shews, that the proposed guinea is abused; and, if the want of weight appear, by the instrument, to be very great, 'tis a strong presumption, that it is rather wash'd, than counterfeit. But the balance will presently resolve the doubt: for, if the suspected coin has its due weight in the air, the great lightness of it in water, seems to proceed from its not being of the requisite fineness; and, if it want much of its due weight in the air, 'tis probably wash'd, &c. rather than adulterated.

It may be very convenient, for those who have frequent occasions to examine various sorts of coin, to have a different instrument adjusted to each. But if the ball be made large, and with a stem long and slender enough, we may

STATICS. may soon, by changing the balast-plates occasionally, fit the same instrument to examine coins of different metals, and weights. Thus one of these, made of copper, serves me for guineas, crown-pieces, and half-crowns; and may easily be made to serve for several foreign coins.

*And other mix'd
metals.*

After the same manner, may pewter be compared and examined. For, having once observed, how far the instrument is sunk, by a determinate weight of it, which is known to be good in its kind; and to contain its due proportion of lead and tin; if you load the instrument with an equal weight of any other mass of pewter, and it sink deeper, the proportion of lead, very probably, exceeds in the mixture; tho' 'tis possible to debase pewter with mineral substances, whose specific gravities are not well known: but to do it with lead, seems to be the most gainful way.

This instrument, may, also, assist us to make a near estimate of the fineness of gold, and its different alloys with silver, or other determinate metals: for, being fitted to sink to the top of the pipe, with a certain weight of the purest gold, as suppose an ounce, make a mixture containing a known proportion of the metal wherewith you alloy the gold, suppose, of nineteen, or fifteen parts of gold, and one of silver; and letting the instrument settle in the water, mark the place where the surface of the fluid cuts the stem. Then putting in another mixture, wherein the silver bears a greater proportion to the gold, as, suppose the former be an eighteenth or a fourteenth part of the latter, you may observe how much less than before, this depresses the instrument: and so proceed to as many degrees of alloy, as can be distinguished on the stem; always observing, that the weight of each mass, in the air, be exactly the same with that of the pure gold; which we suppose an ounce.

By the same method may be examined the different alloys of pure silver, with copper, or other metal, specifically lighter than silver; and, with a slight variation, 'twill not be difficult to estimate in what degree, several coins, whether silver, or gold, are debased, by the known ignobler metal mixed in the piece proposed; and this with greater exactness, than by the touch-stone, or much more troublesome and chargeable methods. And the same is applicable to other mixtures of metals.



Statical Hygroscope.

S E C T. I.

HAVING had occasion, amongst other subjects relating to the air, to consider its moisture, and dryness, I easily discern'd, they had no small influence upon several bodies; and, among the rest, upon those of men; as the air we breathe, either passes from one of these fluid qualities to the other; or from one degree to another, in the same quality.

Various bodies proposed, to discover the dryness and moisture of the air.

I, therefore, immediately consider'd of a proper way, to discover the changes of the air, as to moisture and dryness; with their several degrees. For this purpose it seem'd, to me, that, if a statical hygroscope could be contriv'd, it would be very convenient; and fit, both to determine the degrees of the moisture, or dryness of the air; and to transmit the observations made of them to others. And considering, further, that among bodies, otherwise well qualified for such a purpose, that was likeliest to give the most sensible informations of the changes of the air, which, in respect of its bulk, had the largest surface expos'd thereto; I pitch'd upon a fine sponge, as a thing which is portable, not easily dissipated, but, by its readiness to imbibe water, seem'd likely to attract the aqueous particles it might meet with in the air, and which, by its great porosity, has much more of superficies, with regard to its bulk, than any body, not otherwise less fit for the purpose.

But I here propose, not the best hygroscope that might be contriv'd, only one that is simple, cheap, contained and set up in a little room, easy to be made and transported, statical, commodious, and applicable to various uses.

In considering, what bodies were the fittest to be employ'd for statical hygrosopes, I made tryal of more than one, that seem'd not the least promising. I know, that common, or sea-salt, will greatly relent in moist air; and salt of tartar much more: but then, these salts will not easily part with the aqueous corpuscles they have once imbibed; and are, in other respects, less convenient than a sponge. I made tryal, also, with lute-

strings,

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A sponge made choice of.

But, upon the whole, I found no body so convenient for my purpose, as a sponge: to try the porosity whereof, we weigh'd out a dram of the fine sort; and having suffer'd it to soak up what water it could, we held it in the air, till it dropp'd so very slowly, that we counted a hundred between the falling of two succeeding drops; then putting it into the balance wherein it had been weigh'd before, we found, that as its dimensions were increased to the eye, so its weight increased upon the scale; amounting now, to above two ounces and two drams; so that one dram of sponge, tho' it seem'd not altogether so fine as the portion we had chosen for our hygroscope, imbibed and retained seventeen times its weight of water.

And turn'd into a hygroscope.

Now a sponge is easily turn'd into a hygroscope; for, having weigh'd it when the air is of a moderate temperature, it only requires to be put into the scale of a good balance, suspended on a fix'd and stable supporter. For the sponge, being carefully counterpoised, at first, with a metalline weight, which alters not, sensibly, with the changes of the air, it will, by its rising or falling, shew how much the neighbouring air is grown dryer or moister, in the place where the instrument is kept. The weight of the sponge may be greater, or less, according to the size and goodness of the balance, and the accuracy required in the discovery. For my part, tho' I have, with very tender scales, long employed but half a dram of sponge, I found it answer my expectation: and tho', when I used a bulk several times as great, in a stronger, but proportionably less accurate balance, I had ill success; yet, after trials with different quantities of it, I preferr'd that of a dram; as not being so heavy as to overload the finer sort of goldsmith's scales, and yet sufficient to discover changes considerably minute; turning, discernably, with a sixteenth or twentieth part, and, manifestly, with half a quarter of a grain.

Experiments and observations made with it.

With such hygrosopes as these (wherein the balance ought to be still kept suspended and charged) I made several trials, sometimes in the spring, sometimes in the autumn, and sometimes, also, in the summer and winter. Having once kept a dram of sponge suspended, during a whole

whole spring, and a great part of the preceding winter, and subsequent summer, I was well rewarded by the observations it afforded me.

Now, the use of a hygroscope is either general, or particular.

The general use is, to estimate the changes of the air, as to moisture and dryness, by such ways of measuring them, as are easy to be known, provided, and communicated. 'Tis obvious that our statical hygroscope has the advantage of the common sorts, in these several respects.

The use of hygrosopes.

There is, however, one great imperfection, which all that I have been acquainted with, are liable to; the want of a standard of the dryness and moisture of the air; by relation to which, hygrometers should at first be adjusted, and afterwards compared with one another: tho' this inconvenience may possibly be remedied. In the mean time, even our slight instrument will, in many respects, prove very serviceable.

The first use of the statical hygroscope, to show the different variations of weather in the same month, day, and hour.

And first, it will, on many occasions, be useful to shew, both that the air is usually less moist at one part of the artificial day, than at any other; and at what particular time of the day, or night, it most commonly is so. I remember, when the weather was at a stand, I usually observed, that the sponge had manifestly gain'd in the night, tho' it were kept in a bed-chamber, and grew lighter again between the morning and noon. But this observation, which was made towards the end of winter, would not hold, in case frosty nights, or some other powerful cause intervened. 'Twere proper, also, to observe, whether there be not a correspondence betwixt the hygrometer, and barometer; and, if there be, in what kind of weather, or constitution of air, it is most, or least to be discerned. And this enquiry seems the more dubious, because the same changes of the atmosphere may, upon differing accounts, have either the like, or quite contrary operations upon these two instruments. For, in summer, when the atmosphere is usually heavier, the hygroscope is usually lighter: some strong winds, as with us the north-west, may make both the atmosphere, and barometer lighter; whereas, southerly winds, especially if accompanied with rain, often make the atmosphere lighter, and the sponge heavier. On the other side, I observe, that easterly winds, especially when they begin to blow in winter; tho', by reason of their dryness, they make the hygroscope lighter; yet, at least here, at the west-end of London, they commonly shew the air, by the barometer, to be heavier. It were, likewise, fit to be observed, particularly by those who live on the sea-coast, whether the daily ebbing, or flowing of the sea, do not sensibly alter the weight of the hygroscope. 'Twould, also, be very convenient, to take notice, at what times of the day or night, *ceteris paribus*, the air is most damp and most dry; and not only in several parts of the same day, but in several days of the same month; especially on those, wherein the full or new moon happens. And this seems a more hopeful way of discovering, whether the full moon diffuses a moisture in the air, than those vulgar traditions of the plumpness of oysters, shell-fish, the brains of some animals, the marrow in their bones, &c. It may also be noted, whether monthly spring-tides, especially when

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*A second use, to
show how much
one year, and
season, is dryer
and moister
than another.*

they happen near the middle of *March* or *September*, have any sensible effect, upon our instrument.

The hygroscopes made of an eaten beard, after some months, begin to dry up and shrink; so that their sense of the varying degrees of the moisture of the air, is not so sudden as before; and the informations they give of the degrees of it, especially towards the outmost bounds of their power, to shew the air's alterations, become more and more irregular. But the durableness, and other convenient qualifications of our sponge, making its capacity of doing service more lasting, may the better help us to compare the greatest moisture and dryness, both of the same season, and of the seasons of one year, with the correspondent ones of another. And, if the weight of the sponge, at a proper time, when the temperature of the air is neither considerably moist, nor considerably dry, be taken for a standard; a person may, by computing for how many days, at such an hour, and how much, at that hour, it was heavier or lighter than the standard; and, also, by comparing the result of such an account, in one year, with the result of the like account in another, be assisted to make a more particular, and near estimate of the differing temperature of the air, as to moisture and dryness, in one year than in another; and in any correspondent season, or month, assigned in each of the two years proposed. And, how far the comparison, or continuance of such observations, both in the same place, and also in different countries, and climates, may be useful to physicians, in reference to those diseases, where the moisture and dryness of the air have much interest; and the husbandman, to foresee what seasons will prove friendly, or unkind, to particular soils and vegetables; it must be the work of time to teach us: in the interim we have no reason to despair, that the uses to be made of such observations, may prove considerable. And if, by the result of many observations, men may be enabled to foresee the temperature of a year, or even of a season, it may advantage other professions of men, who receive much profit, or prejudice, by the great dryness, or excessive moisture of the seasons. Not to mention those who cultivate hops, saffron, and other plants, that are tender, and bear a great price; such a foresight, as we are speaking of, may be of great use to shepherds, who, in several parts of *England*, often sustain great losses, by the rot of sheep; which usually happens through excess of moisture, in certain months of the year. And, in order to lay foundations, whereon to build predictions, it may not be amiss to register the number, magnitude, and duration, of the considerable spots, that may at this, or that time of the year, happen to appear, or be dissipated, on, or near the sun; or, to take notice of any extraordinary absence of them; and to observe whether their apparition, or dissipation, produce any changes in the hygroscope. This, indeed, I should not venture to propose, but that eminent astronomers have casually observed great dryness to attend the extraordinary absence, or fewness of the solar spots. And such persons as are astrologically given, may extend their curiosity, in the use of this instrument, to observe, whether eclipses of the sun and moon, and the great conjunctions of the superior planets, have any remarkable effect upon it.

The third use, to discover and compare the changes of the temperature of the air, made by winds, frosty, snowy, and other weather.

I have found, by removing the sponge into the wind, that it soon receiv'd a very considerable alteration, in point of weight; as it, also, did, when remov'd out of a room, into a garden, where the sun shone: for, tho' the season were not warm, it being then the month of *January*, yet, in three quarters of an hour, the sponge lost a 24th part of its weight. We may, also, in some cases, usefully substitute a broad piece of sheep's-leather, by displaying it to the wind. For this having, by reason of its thinness, in proportion to its breadth, a very large superficies immediately exposed to the wind, we found it to be considerably alter'd thereby; so that half an ounce of it, well-prepared, being kept, for an hour, in a place where the sun-beams might not beat upon it, did, in a strong wind, vary an eighteenth part of its original weight. But tho' I think it very possible to make such observations of the temperature of particular winds, as will frequently prove so true as to be useful, at least, to those who live in the places where they are made; yet I am of opinion, that, to be able to settle general rules, to determine, with any certainty, the quality of winds, according to the points of the compass, from whence they blow, great wariness will be required: and he who has not some competent skill in physics, and cosmography, must be very subject to mistake in forming his rules.

For, winds that blow from the same quarter, are not, in some countries, of the same quality they are in most others; but participate much of the nature of the region over which they blow, in their passage to us. At *Archangel* they observe, that tho' a northerly wind, almost every where else, without the tropics, produces frost in winter, it is there attended with a thaw; probably because this wind comes over the sea, which lies north from that place: and on the contrary, a southerly wind, blowing over a thousand, or twelve hundred miles, of frozen land, rather increases the frost, than brings a thaw. The northern winds, which are elsewhere drying, are said in *Egypt* to be moist. And we are told, that the northern wind in *Egypt*, is the moistest. *Monsieur de Serres*, speaking of the changes of the air, with regard to husbandry, in several parts of *France*, informs us, that about *Thalouze*, the south-wind dries the ground, and the north gives rains. On the contrary, from *Narbonne* to *Lyons*, all over *Provence* and *Dauphiné*, the latter causes dryness, and the other brings moisture.

But, farther, the vehemence, or the faintness of the wind, though blowing over the same country, may much diversify its operation on the hygroscope; and the same wind, which, when it blows but moderately, appears moist by the hygroscope, may, when vehement, or impetuous, make the instrument grow lighter; dissolving and driving away more vapours, by the agitation of parts it makes in the sponge, than is balanced by those aqueous vapours that are brought along with it. And frosty weather, as we found often, made the hygroscope grow lighter, even at night; but snowy weather, which lasted not long, added something to the weight of the sponge: and it has been observ'd, that mists and foggy weather add a weight to it.

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Nay, 'tis probable, that a transient cloud, in fair weather, may be manifestly observable by our instrument. For, the 9th of *September*, being for the most part, a very fair sun-shiny day, and tho', about ten a clock in the morning, the sun shone brightly, the sponge began to preponderate; which unexpected phenomenon made the person, in whose chamber the instrument was, look out at the window, where he discover'd a cloud that darken'd the sun; but, after a while, this being past, the balance return'd to an equilibrium. And, I have more than once, or twice, observed, especially in summer, that when the air grew heavier, the hygroscope either continued at a stand, or, perhaps, grew lighter; as if, when such cases happen, the effluvia that get into the air, either from the terrestrial, or some other mundane globe, were not fit, like vapours, to enter and lodge in the pores of the sponge; and so were corpuscles of another nature; with which, when we find by the baroscope, that the air is plentifully stock'd, it may be worth while to observe, whether any, and what kind of meteor, will commonly be signified, and produced.

A fourth use, to compare the temperature of different houses, and different rooms in the same house.

As it is of great use, both in respect of mens health, especially if they be of a tender, or sickly constitution; and for the conveniency of the keeping flesh, several sorts of wares, &c. that are subject to be damaged by moist air; so this may be readily and manifestly derived from our instrument. For, by removing it into several houses, or into several parts of the same house, and letting it stand, for a competent time, in each, we have often observed a notable difference; of which I shall here give two or three instances.

Having, upon the 10th of *October*, taken a piece of fine sponge out of a cabinet, and clip'd it till it weigh'd just half a dram, in a nice pair of scales, and a warm room; we afterwards removed them into a neighbouring room, destitute of a chimney. This statical hygroscope, was, *October* 12. at night, remov'd into the former room, and the sponge found to have gain'd three grains, and an eighth; and, consequently, more than a tenth part of its first weight: but, being suffer'd to stand in this warm room; in less than twelve hours, it lost a grain, and about one eighth of its former weight; though the time it stood in this room were, for the most part, night, and the weather rainy.

At another time, we took a piece of very fine sponge, which had formerly weigh'd just a dram; but having been, for many months, kept in a very warm room, it was grown much lighter: for, removing it into an upper chamber, in a neighbouring house, and weighing it in tender scales, in the evening 'twas found to want four grains, and three quarters, of a dram; and tho' there was a fire in the room, and the scales stood not far from it, yet, in a short time (the day being foggy and rainy) the sponge visibly depress'd its scale three eighths, and, the next morning, was found to want but one grain and a half, of a dram; so that it had gain'd about three grains and a quarter: and the following evening, being the second of *January*, it weigh'd one dram, a grain, and almost a half. So that in about one natural day, the sponge had acquired six grains from the moisture of the air; that

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that is, a tenth part of its first weight; and a greater proportion to the weight it had the day before. The third of *January*, the weather being yet moist, the weight exceeded two grains: but about three or four of the clock, in the afternoon, it began to lose of that great weight, which diminish'd more by the next morning; the weather having chang'd that night, and become frosty.

A dram of sponge, that had, for several weeks, been kept in a dry room, was (*January 10.*) carried out into a room, where a fire is not usually kept; the weather being extraordinary foggy: the next morning it was brought into the former room; and though, now the weather was clear, it appear'd to have gain'd about eleven grains in weight; yet it presently lost two grains, by standing in this room.

In small chambers, I have often observed a moderate fire to alter the weight of the instrument, placed at a distance from it, after it had been well kindled for a very little while; but, in wet weather, if the fire were not seasonably recruited, with fresh fuel, the decay of it would soon begin to be discernable by the instrument.

Supposing the alteration of weight in our sponge, to depend, only, upon the degree of the moisture of the air; if a convenient part of the room be chosen for the hygroscope, and it be kept constantly there, 'tis easy, from time to time, to perceive when 'twill be requisite to increase, or moderate the fire, so as to keep the sponge at that weight it was of, when the temperature of the air of the chamber, as to dryness and moisture, was such as is desired. I have, sometimes, observed, with pleasure, how manifest and great a change, in the weight of our sponge, would be made, when the room was wash'd, and, for a considerable time after; tho' a large fire was kept in it, to hasten its drying.

Besides these several uses of our hygroscope, there may, possibly, be many others; and, perhaps, by a little alteration, and improvement, it might be made capable of shewing some difference betwixt steams of differing natures; as those of water, spirit of wine, chymical oils; or new kinds of substances in the air, wherein I suspect there may sometimes be dispersed great plenty of unknown corpuscles. For, I have, more than once, observed, the hygroscope not to be affected with the alteration of weather, answerably to what the manifest constitutions, or variations of it, seem'd plainly to require. Whether unobserv'd particles perform'd this, by making the other steams, in point of figure, or size, unfit to enter the minute pores of the sponge; or by dissipating, or otherwise procuring the avolation of more of the watery particles, than they could countervail, I shall not now examine. And, perhaps too, by associating this instrument with the thermoscope, baroscope, &c. it might be so improved, as to help us to foresee many considerable things, that either are, themselves, changes of the air, or consequences thereof: for instance, sickly and healthful constitutions of the air; barren, or plentiful seasons, in particular places or countries; strong hurricanes, earth-quakes, inundations, and their ill effects; and especially those accidents that depend much upon the surcharge of the air, with other

STATICS. other exhalations, and moist vapours, which before-hand operate sensibly upon our instrument; and therefore may be discernable by it, long before they arrive at that height, which makes them formidable meteors. And, if it were but the foretelling approaching rain; this very thing may, on many occasions, prove very serviceable, and recommend our instrument, which often receives much earlier impressions from the steams that float in the air, than our senses do; so that I have been able to foretel a shower of rain, especially in dry weather, for a considerable time before it fell.

And here I must advertise, with regard to our instrument, as well as the barometer, that if a theory, or hypothesis, in itself rational, be found agreeable to what happens the most usually in observation; it ought not to be rejected, or laid aside; tho', sometimes, particular instances may call it in question. For, 'tis very possible, that the theory, or hypothesis, may be as good as a wise man would require, about so mutable a subject as the weather. And the cause assign'd by the hypothesis, may, really, act suitably to what that requires, tho' a contrary effect ensue, by reason of this cause being accidentally over-ruled, by some more powerful agent, which for that time invades the air. Thus we know, that tides do, in general, correspond with the motions of the moon; and generally ebb and flow at such times, and in such measures, as the theory, grounded on that correspondence, requires: yet seamen find, that in this, or that particular harbour, or mouth of a river, fierce contrary winds, great land-floods, and other casual causes, sometimes, both very much disturb the regular course of the tides, and either increase, or lessen them.

SECTION II.

Instances of the power of the moisture in the air, at all seasons.

Upon animal substances.

STill farther, to recommend the use of hygrosopes, I shall here shew, that the quality they acquaint us with, may be very considerable, not only upon mens healths, but upon subjects far less tender, and less curiously contriv'd, than human bodies.

That the moisture of the air may have no small influence, and, usually, a bad one upon mens healths, tho' experience did not so often teach it, I should directly infer, from its operation upon the dry and firm parts of animals; and even in those cases, where, for want of time, or other impediments, it cannot produce any sensible degree of putrefaction.

The skins of animals may easily be invaded by the moist particles of the air, because of the great number of their pores; which may be concluded from their hairiness, their sweat, or both. And I lately observed, that sheep's-leather imbibes the moisture of the air, and increases in weight upon it, as plentifully as almost any body I have expos'd to it.

But to shew, that much closer membranes, and which nature made to be impervious to such a liquor as urine, may be affected by the vapours of the air; I shall add, that having purposely taken pieces of fine, well-blown bladders, and, as far as appear'd, of a very close texture, and counterpois'd them in a good balance, I found they would considerably increase their

their weight in moist, and lose it again in dry weather; so that I might have employ'd the most membranous part of a bladder, to make a statical hygroscope. Statics

And tho' bones are the firmest and most solid parts of animals; and, as it were, the pillars by which their fabric is sustain'd; yet, even these may be pierc'd, and sensibly affected, by the moisture of the air. For, having caus'd the skeleton of a human body, to be so set together, that by the help only of slender wires, artificially order'd, the natural motions which the muscles make of the bones, might be well imitated; I observ'd, that tho' in dry, and fair weather, the flexures of the limbs might be readily made; yet, in very moist weather, the joints were not easily bent; as if the parts were grown stiff and rigid: which seem'd to proceed from the moist particles of the air, which having plentifully insinuated themselves, at the pores, into the bones, had every way distended them, and thereby made the parts bear harder against one another, at the junctures, or articulations.

But, farther, the moisture of the air may exercise a considerable force, even upon inanimate, and inorganical bodies. Even well-season'd wood, will suffer a kind of divulsion of a multitude of its parts, and be manifestly distended by aqueous corpuscles getting into its pores. Having procur'd a piece of sound and season'd wood, of about an inch in diameter, to be turn'd exactly cylindrical, and also a ring of brass, or ivory, to fit it; so that it might, with ease, be put on, and taken off: we set the piece of wood in fair water, and left it to soak there for several hours; at the end of which, it was visibly swell'd: so that the ring which was adjust'd to it before, was manifestly too little to go again upon it; or, with its orifice, to cover the whole basis of the distended cylinder; which, afterwards, being dry'd in the air, shrink into a capacity fit to enter the ring again. And, in this experiment, I took notice, that the great distension of the wood, was not produced all at once, or soon after it was put into the water, but it swell'd by degrees, and lay soaking there for several hours, before it arriv'd at its utmost dimensions; the aqueous corpuscles requiring, it seems, so much time to insinuate themselves, sufficiently, into the wood: which argues, that the internal parts were, likewise, affected, tho' when these came to swell, they had a great thickness of wood about them, to hinder their dilatation. Upon vegetables.

And to manifest, that even in summer, there are aqueous vapours floating in the air; within some days of *Midsummer*, and in clear sun-shiny weather, we took a pint glass-bottle, and having put into it a convenient quantity of water, plac'd it; and four ounces of beaten sal-armoniac in one scale of a good balance, and a counterpoise in the other; and then, putting the salt into the water, I observ'd, that tho' for a while the equilibrium remain'd, yet when the frigorific mixture had sufficiently cool'd the outside of the bottle, the roving vapours of the air, that chanc'd to pass along the surface of the vessel, were, by the contact of that cold body, arrested, and turn'd into a kind of dew, which, from time to time, made the scale, that held the glass, preponderate more and more: and, at length, the drops growing

STATICS. ing larger, ran, in small rivulets, down the sides of the glass. So that in less than an hour, the condens'd steams amounted to near a dram, which weight was, afterwards, much increas'd within about two hours more.

And that the multitude, figures, and motions of these particles, in the form of vapour, may enable them to display no small force in their operations on some bodies, appears from the breaking of the strings of musical instruments, first brought to a strong tension, in rainy weather. For the vapours, which then wander through the air, insinuating themselves into these strings, distend and swell them, and thereby endeavour to shorten them, so forcibly, that they often break with a smart noise and great violence: and if we consider how great a weight some of the bigger strings of base-voils, that have been observed to break in moist weather, require to stretch them till they break; this effect of the moist air will appear to require a very great force.

And even upon metals and minerals.

Besides, an ancient musician, and a great organ-maker, inform'd me, that the wire-strings of musical instruments will, for the most part, be out of tune in wet weather; the strings then, generally, affording their notes sharper than usual; and that, upon great changes of weather, several organs would (after they had been long tuned) grow out of tune, the wooden pipes swell, and the metalline ones be untuned.

But if bodies be of such a constitution, as to assist the operation of the moist air, the penetrancy and efficacy of it may be still more considerable. For, there are some kinds of those marcasites that yield vitriol, which, whilst they lie under ground, or are cover'd with the sea-water, on shores, retain a stony hardness, and are often taken for mere stones; yet credible persons have, casually, observ'd, that these, being exposed to the air, would, in tract of time, be so penetrated by the moist particles of it, as to swell and burst. That this will happen to such kind of stones, by the help of rain, experience has perswaded me; and that it may, also, happen even to very hard and stone-like marcasites, when they are merely exposed to the air, I am apt to think, from some tryals of my own. For, from shining marcasites, tho' kept but in my chamber-window, I have had vitriolate efflorescencies, which seem'd to be produced by the action of the piercing moisture of the air upon the mineral. And I remember, that very hard and heavy lumps of a marcasitical substance, which seem'd to be stony, were so disposed to be wrought on by the air, that tho' kept in my own chamber, or other cover'd places, yet in a short time were so penetrated by the moist corpuscles of the air, that they burst into many pieces, which fell from one another; and several of the divided portions might, easily, be crumbled betwixt one's fingers. And of some of these I have observ'd, that a vitriolate substance was produced more plentifully in their innermost parts, than on or near their outside. So that, considering how great an external force would have been requisite to make such a comminution of minerals thus solid and hard, 'tis obvious, that the air's moisture is capable, when it meets with fitly-disposed bodies, to exercise a far greater force than is usually imagin'd.

And

And thinking it probable, that ropes themselves would considerably imbibe and part with the moisture of the air, and that so as to shrink in rainy weather, tho' clogg'd with a weight fasten'd at the lower end; I first caus'd a rope, that was about twenty yards in length, but of no great thickness, to have one of its ends fasten'd to an immoveable body, at a convenient height from the ground, and a pully to be so fasten'd to another stable body, at the distance of eighteen or twenty yards from the first, that the rope, resting upon the pully, lay almost horizontally. But to the end of that part of the rope, which, from the pully, reach'd within two or three feet of the ground, was fasten'd, by a ring, a leaden weight of fifty pound. To this was, also, fasten'd a light index, placed horizontally, whose end moved along an erected board, which by transverse lines was divided into inches and parts of inches, reaching a considerable way upwards and downwards, that the index might, within those bounds, have room to play, according to the alterations of the weather.

It being now summer, we placed this apparatus in a garden, tho' partly under a pent-house, that the rope might be more expos'd to the air, than it would have been within doors; and in two or three days, the weight had brought the rope to the utmost stretch that was able to give it: after which, it began manifestly to shrink and lengthen, according to the weather. So that once looking, over-night, upon the suspended weight, and marking how low it reach'd upon the divided board; when I came again the next morning, a great part of the night having been rainy, I found the cord so shrunk, that the weight was rais'd above five inches: yet the day growing dry and windy, and sometimes warm, the weight had, at night, stretch'd the rope more than the moisture had contracted it before.

Afterwards having procur'd a far greater weight, I substituted it in the place of the other; and having suffer'd it to stretch the rope as far as it could, I register'd some observations about it; two whereof I shall here set down.

June 4. At half an hour after nine at night, I look'd upon the hundred weight that hung at the bottom of the rope, the weather being then fair; and a mark being put at that part of the erected board, where the bottom of the weight touch'd, I perceiv'd the sky a while after to be cloudy and overcast, but without rain: wherefore going to view the weight again, I found it in an hour and a quarter to have risen a quarter of an inch, or more.

June 6. Yesterday the weight was observ'd to rest at the eleventh inch of the erected board. This morning about eight, I found it risen about half a quarter of an inch above the eighth; the morning being cloudy, tho' the ground very dry and dusty. The weather being overcast, within less than an hour afterwards, (some scatter'd drops of rain then beginning to fall) I found the weight risen about half an inch above the eighth mark*.

Hence

* Dr. *Furin*, to gain materials for an exact meteorological history, desires the curious of all parts to keep a diary of the weather, and to send in their accounts of it, annually, to the secretaries of the Royal Society; where, being compared with those made

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Hence the force of the air's moisture is not small, since it could raise an hundred weight, tho' the chord measured, in diameter, but about the third part of an inch.

made by the order of that illustrious body, the result may be publish'd in their Transactions. As such a work, for its great usefulness, is highly desirable, we shall here give the particulars, which the Doctor desires should be principally inserted in such a journal.

And, 1st, he requests, that the heights of the barometer, thermometer, and the state of the hygroscope, be observ'd, at least once every day; from what point of the compass the wind blows, and with what force; the appearance of the sky, and the quantity of rain or snow, that fell since the preceding observation was made.

2. That in case of a storm, its beginning, increase, height, decline, and cessation, be accurately remark'd, with the precise times wherein each happen'd, and the heights of the barometer corresponding to those times.

3. That the common barometer, artificially fill'd, be made choice of, the tube being $\frac{1}{2}$ or $\frac{3}{4}$ inch in diameter; and the vessel containing the stagnant mercury, 8 or 10 times wider than the tube.

4. If a portable barometer be employ'd, the Doctor recommends those of that excellent artist Mr. Hanksbee, in Crane-Court, Fleetstreet; who also (says the Doctor) makes most exquisite thermometers.

5. If any other thermometer be used, the Doctor desires that its situation, structure, scale, and the name of the person who made it, may be set down in the diary; and that this instrument be placed in a room that looks north, where a fire is seldom or never made.

6. That they may be the better com-

pared together, the Doctor farther desires, that the first column may exhibit the day and hour of the observation, old stile; that the second may shew the height of the mercury in the barometer, in inches of an *English* foot, and their decimal parts; that the third column may contain the degree and decimal parts of a degree of the spirit in the thermometer; that to the fourth be assign'd the wind, with its degrees of force, which may be express'd by the figures 1, 2, 3, 4, and 0 for a perfect calm. That the fifth may contain the appearance of the heavens, and a short history of the weather. That the last may give the height of the rain or snow when dissolv'd, which fell since the preceding observation, in *English* inches, and their decimal parts.

The quantity of rain, this very learned gentleman tells us, may be easily computed by means of a funnel two or three feet wide, set in a close narrow-mouth'd vessel to receive the water falling in an open place, and a cylindrical measure divided into inches and decimal parts. But the diameter of the cylindrical measure should be 10 times less than that of the funnel; whence an inch height of water in the measure, will appear to have fallen to the height of $\frac{1}{10}$ inch upon the funnel, and so for other decimal parts of an inch. At the end of every month and year, the Doctor desires may be set down the mean mensural and annual height of the barometer and thermometer, as likewise the sum of all the heights of the rain that has fallen each month or year. See *Philos. Trans.* N^o 379. p. 427.

Hygrosopes Consider'd.

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A Specimen of this Diary take as follows.

Dies & Hora 1723.	Barom. alt.	Therm. alt.	Vent.	Tempestas.	Pluvia.
Nov. St. V.	dig. dec.	gr. dec.			dig. dec.
1. 8 a. m.	29.75	49.6	S.W. 1	Cœlum nubibus obduct.	0.035
4 p. m.	29.56	47.3	S.W. 2	Imbres interrupti. Sol per vices inter- currens	0.043
2. 7 ½ a. m.	29.24	48.5	S. 1	Pluvia fere perpetua	0.725
3. 9 a. m.	29.95	49.7	N. 1	Cœlum sudum	0.032
5 p. m.	30.4	49.2	N. 1	Cœlum sudum	0.000
4. 7 a. m.	29.9	47.0	S.W. 1	Nubes sparsæ	0.000
10	29.7	46.2	S.W. 2	Imbres intercurrentes	0.103
12	29.4	45.0	S. 3	Cœlum nubibus un- dique fere tectum	0.050
3 p. m.	28.8	46.0	S. 4	Nubes sparsæ	0.000
5	28.6	47.2	S.W. 4	Eadem Cœli facies	0.000
7	28.9	48.0	S.W. 2	Pluit	0.000
9	28.9	48.2	0	Pluvia fere perpetua	0.305
5. 7 a. m.	29.7	53.4	N. E. 1	Sudum. Gelu.	0.250



Ddd 2

Fire

Fire and Flame

Weigh'd in a
B A L A N C E.

SECT. I.

*Experiments to I.
 show that flame
 may incorporate
 with solid bodies,
 so as to increase
 their weight and
 bulk.*

A Piece of copper-plate, not near so thick as a half-crown, weighing two drams and twenty-five grains, being placed, horizontally, in a crucible, whose bottom had a little hole in it, for the fumes to get out at; so that it could not be remov'd from its position, nor be easily made to drop down, and lose its level, tho' the crucible were turn'd upside down; about an ounce and half of common sulphur, was put into a taller and broader crucible, and that wherein the copper stuck, inverted into the orifice of it, that so the sulphur being kindled, the flame might reach the plate, and have some vent beyond it, at the hole above-mention'd. The brimstone burn'd about two hours, in which time it seem'd all to have been resolv'd into flame; no flowers of sulphur appearing to have sublim'd into the inside of the upper crucible. And tho' the copper-plate were at a considerable distance from the fired sulphur, yet the flame seem'd to have actually penetrated it, and to have made it visibly swell, and grow thicker; which appear'd to be done by a real accession of substance; since, after we had wiped off some little adhering *sordes*, and, with them, several particles of copper, that stuck close to them, the plate was found to weigh near thirty-two grains more than at first; and, consequently, had increas'd its former weight, by above a fifth part.

2. Having, by refining one ounce of sterling silver, with salt-peter, reduced it to seven drams, or somewhat less; we took a piece of it, that weigh'd fifty-eight grains, and ordering it, as the copper-plate, in the former experiment. After the flame of above one ounce and a quarter of sulphur had, for about an hour and half, beat upon it, the silver plate seem'd,

to the eye, somewhat swell'd; and the lower surface of it, next the flame, was brought to a great smoothness; the weight being increas'd to one dram, five grains, and three quarters; which acquired weight, falling so short of that gain'd by the copper, the difference may, perhaps, be attributed to the closeness, and compactness of the silver, argued by its being heavier, *in specie*, than copper; or to the greater disposition of the pores of copper, to be wrought on by the fiery menstruum*.

Nor is it strange, that, the particles of flame, which are either of a saline, or a piercing terrestrial nature, being wedg'd into the pores, or being brought to adhere, very fast, to the little parts of the bodies, exposed to their action, the addition of so many little particles, that want not gravity, should, because of their multitude, be considerable upon a balance; tho' a few of them would have no visible effect.

3. Into a very shallow crucible, we put one ounce of copper-plates, and set it in a cupelling furnace, where 'twas kept for two hours; and then being taken out, we weigh'd the copper, which had not been melted, (having first blown off all the ashes) and found it had gain'd thirty grains.

That fire may do the same.

4. We took an ounce of copper filings, and putting them upon a very shallow crucible, and under a muffler, we kept them there for about three hours; and afterwards taking them off, we found them of a very dark colour; not melted, but caked together, in one lump; and increas'd in weight, the ashes and dust being blown off, no less than 49 grains: part of which increase, above that obtain'd by the copper plates, may, probably, be due to the longer time that, in this experiment, the filings were kept in the fire.

5. We weigh'd out one ounce of small lumps of hartshorn, that had been burn'd till they appear'd white; and having put them into a crucible, and kept them in a cupelling furnace, for two hours, we found, when they were taken out, that they had lost six or seven grains of their former weight; perhaps, because the internal parts of some of them, might not be exquisitely calcined, but retain'd some oleaginous, or other volatile substance; or because, having omitted to burn them well, before they were weigh'd, they might, since their first calcination, have imbibed some moist particles of the air: for having kept them awhile in the scale, wherein they were weigh'd, they did, within two or three hours, make it preponderate. At the same time with this hartshorn, we, also, put in one ounce of well-heated brick, and kept that, likewise, in the furnace, for above two hours; at the end of which,

* M. Muschenbroeck, counterpoizing a prism of iron, that weigh'd three pound, in scales which would turn with $\frac{1}{25}$ grain, afterwards heated the prism red hot; when examining it again, by the same balance, he still found it exactly three pound: whence he justly concludes, that it had

gain'd in weight from the fire. For, the experiment being made in the open air, and the metal being expanded by heat, 'tis plain, that the specific gravity of the heated metal, was decreas'd by the expansion, so as to be then lighter, with respect to the air. *De materiâ subtili*, p. 8, 9,

weigh-

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weighing it, whilst it continued hot, we did not find it to have either sensibly got or lost; but, some time after, it seem'd, upon the balance, to have imbibed a little moisture from the air.

6. Upon a good cupel, we put an ounce of *English* tin, of the better sort; and having placed it in the furnace, under a muffle, tho' it presently melted, yet it did not forsake its place, but remain upon the concave surface of the cupel, till, at the end of about two hours, it appear'd to have been well-calcin'd; and then being taken out, and weigh'd by itself, the ounce of metal was found to have gain'd no less than one dram.

7. An ounce of lead was put upon a cupel, made of calcined hartshorn, and placed under a muffle, after the cupel was first made hot, and then weigh'd. This lead did not enter the cupel, but was turn'd into a kind of litharge on the top of it, and broke the cupel, whereby some part of the latter was lost in the furnace; yet the rest, together with the litharge, weigh'd seven grains more than the lead and heated cupel, when they were put in.

8. We took a cupel about two ounces in weight, made of ten parts of bone-ashes, and one of charcoal-ashes, mix'd together with ale. This was, by itself, put in a cupelling furnace, under a muffle; and when 'twas thoroughly and highly heated, we weigh'd it in that state. This done, 'twas immediately again plac'd under the muffle, and kept there for about two hours; at the end of which time, 'twas taken out, red-hot, and presently put into the same balance, as before; where, having blown off the adhering ashes, I found that, tho' when 'twas first taken from under the muffle, we had but two ounces, and two grains, yet now, the weight being put into the opposite scale, it gain'd, very near 21 grains. And here note, that 'twas not without some cause, that I weigh'd the cupel red-hot: for I had a suspicion, that notwithstanding the dryness of the bone, it might receive some little alteration of weight, by imbibing the moist particles wandering in the air; and accordingly, leaving the cupel counterpois'd, to cool in the balance, in a short time, it began sensibly to preponderate; and suffering it to continue there, for nine or ten hours, I found it about three grains heavier than before.

9. Steel being a metal that, as experience inform'd me, will, very easily, be wrought on by fluids of a saline nature, 'twas reasonable to expect, that flame would have a greater operation on it, especially if it were, before-hand, reduced to small parts, than on any of the bodies hitherto described. And, accordingly, four drams of the filings of steel, being kept two hours on a cupel, under a muffle, acquired one dram, six grains and a quarter, increase of weight.

10. A piece of refined silver, being put upon a cupel under a muffle, and kept there for an hour and a half, was taken out, and weigh'd again; and as before it weigh'd three drams, thirty-two grains, and a quarter, it now weigh'd in the same scales, three drams, thirty-four grains, and a half.

We

We repeated the experiment with half an ounce of the filings of silver, well refin'd with lead, in our own laboratory, and kept them about three hours upon the cupel; when taking them out, we found them of a less pleasant colour, than before, and melted into a lump, which weigh'd four drams, six grains.

11. We took a dram of the filings of zink, or spelter, and, having put it upon a cupel, under a muffler, we kept it there, in a cupelling fire, for about three hours; then taking it off, we found it to be caked into a brittle dark-colour'd lump, which look'd as if the filings had been calcined. This being weigh'd in the same scales, gain'd full six grains.

12. Among our various trials, upon common metals, we thought fit to make one or two upon a metal brought us from the *East-Indies*, and there called *Tutenag*. Two drams of the filings of tutenag, being put upon a cupel, and kept under the muffler for about two hours, the filings were not melted into a lump of metal; but look'd as if powder'd ceruse and minium had been mix'd together; some of the parts appearing distinctly white, and others red; the calx being put into the balance, appear'd to have gain'd $28\frac{1}{4}$ grains. Another time, the experiment being repeated, with the like circumstances, we found that two drams of filed tutenag, gave almost the like increase of weight. So that this *Indian* metal, seems to have acquir'd more in the fire, in proportion to its weight, than any we have hitherto made trial of.

13. To confirm, by a clear experiment, that in all cupellations, the lead then employ'd, does not, together with the baser metals, to be purg'd off from the silver, or gold, fly away in smoke; we took two ounces of good lead, and one dram of filings of copper; and having caus'd a cupel to be ignited, and suddenly taken out of the furnace, and weigh'd, whilst 'twas very hot, we presently put it back, together with the two metals laid on it, into the furnace: where, having been kept for about two hours, it was taken out again, and found to have nothing on the surface of it, worth weighing distinctly, in the scales, wherein the cupel, with what was sunk into it, amounted to four ounces, three drams, and eleven grains; which wanted but nine grains of the whole weight of the cupel, and the two metals, when they were, all three together, committed to the fire. So that tho' we make a liberal allowance for the increase of weight, that may, with any probability, be supposed to have been attain'd by the cupel, and what was put upon it, yet it will easily be granted, that much the greater part of the metals, was not driven off in fumes, but enter'd into the substance of the cupel.

After having shewn, that either flame, or the analogous effluvia of the fire, will be incorporated with metals, and minerals, exposed naked to its action, I thought proper to try whether this flame, or igneous fluid, were subtil enough to exercise any such operation upon the like bodies, shelter'd from its immediate contact, by being included in close vessels.

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*Even tho' the
bodies be not im-
mediately expo-
sed to it.*

14. We took an ounce of new filings of steel, and having included them betwixt two luted crucibles, kept them, for two hours, in a strong fire, and suffer'd them to continue there till the fire went out; the crucibles being unluted, the filings appeared hard caked together, had acquired a dark colour, somewhat between black and blue, and were increased five grains in weight. At another time, an ounce of filings of steel, being put between two crucibles, in the same manner, after they had been kept about an hour and half in the fire, were taken out, and found to have gain'd six grains in weight.

15. We put two ounces of copper-plates into a new crucible, over which a lesser was whelm'd, and luted the junctures; after the same manner, two ounces of tin were included betwixt crucibles; and also, two ounces of lead: and all these, being put into the cupelling furnace, were kept in a strong fire, for about an hour and a half; and then being taken out, the copper-plates, tho' they stuck together, were not quite melted; and seem'd, some of them, to have acquired scales, like copper put into a naked fire; and the two ounces had gain'd eight grains in weight. The lead had broke thro' the bottom of the crucible, and thereby hinder'd the design'd observation. The tin acquired six grains in weight, and was, in part, brought to a pure white calx; but much more of it was melted into a lump, of a fine yellow colour, almost like gold, but deeper.

16. We took one ounce of very thin copper-plates, and putting them betwixt two luted crucibles, we kept them in the cupelling furnace for about three hours; and then disjoining the vessels, we found the metal cover'd with a dark and brittle substance; which, when scaled off, disclosed a finely colour'd metal, that, together with these burnt scales, amounted to twenty-one grains above the weight first put in.

17. We took a piece of fine block-tin, in weight half a pound; this we put into a choice glass-retort, and kept it for two days in a sand furnace; which gave heat enough to keep the metal in fusion, without cracking the glass: then taking out the mixture, we carefully weigh'd it in the same scales, and found the superficies a little alter'd, as if it were disposed to calcination; and the weight to be increas'd about two grains.

*And also after
they have been
calcined.*

18. We farther try'd how much some bodies would increase in weight, by the fire, when they had already been exposed to the vehemence of it. One ounce of calx of tin, made *per se*, being put in a new cupel, and kept under the muffler for about two hours, was taken out hot, and put into the scales, where the powder appear'd to have gain'd in weight, one dram, thirty-five grains, by the operation of the fire; which made it also look much whiter than it did before.

19. Out of a parcel of filings of steel, that had been before exposed to the fire, and had considerably increas'd its weight thereby; we took an ounce, and having exposed them at the same time with the calx of tin, and for the same time kept them in the fire, we took them out at the two hours end, and found the weight to be increased, two drams, twenty-two grains. The filings were very hard caked together, and the lump being broken, look'd almost like iron.

20. An

20. An ounce and four drams of copper, which had endured a violent fire for an hour and half, being included betwixt two crucibles, and exposed to a strong fire for two hours, and suffer'd to continue there till the fire went out, appear'd, thereby, to have gain'd ten grains in weight, and to have, upon the superficial parts of the plates, several dark-colour'd flakes; some of which stuck to the metal; but more, upon handling it, fell off.

21. We took eight ounces of block-tin, which, being cut into bits, was put into a good round vial, with a long neck; and then warily held over quick-coals, without touching them, till it was melted; after which, it was kept almost continually shaking, to promote the calcination, for near an hour; the metal being all the while in fusion, and the glass kept at some distance from the coals. For the greatest part of this time, the orifice of the vial was cover'd with a cap of paper, to keep the air, and steams of the coals, from getting into the neck: the vial, being afterwards remov'd from the fire, was broken, and the metalline lump had a little darkish calx, here and there, upon the upper surface; but much more beneath, where it had been contiguous to the bottom of the glass. Then putting all this, (carefully freed from little fragments of broken glass) into the same balance, with the same counterpoise I had used before, I found an increase of weight, amounting to eighteen grains, which the tin had acquired by this operation.

22. We then separated the calx; and having melted the metal in a crucible, that, by pouring it out, it might be reduced into thin plates, capable of being cut in pieces, and put into such another vial as the former, we weigh'd it again, together with the reserv'd calx, but found, that notwithstanding all our care, we had lost three grains of the eighteen gain'd before. This done, we put the metal into another vial; which having a stopple of paper, to keep out smoke and air, was held at some distance from the coals, only whilst the tin was melting; being afterwards warily laid upon them, and kept there for two hours; at the end of which, 'twas again taken off, and the metal weigh'd with the same counterpoise, and balance, as before; and then it amounted to eight ounces, twenty-four grains; and had much more separable calx, than at the first time.

Nor did I much wonder, that the weight should be increas'd, in this last operation, but nine grains, in two hours; and, in the former twice as many, in half the time; since, during the two hours the glass was kept in one posture: but, in the first operation, it was, almost, perpetually shaken, during the time 'twas kept in fusion; for 'tis observ'd, that the agitation of melted minerals, will much promote the effect of the fire upon them, and conduce to their calcination.

23. But, to prevent all suspicion of any increase of weight, in the metal, arising from smoke, or saline particles, getting in at the mouth of the vessel, I made the experiment in glasses, hermetically seal'd, as follows. Eight ounces of good tin, carefully weigh'd, we hermetically seal'd up in

STATICS. a new small retort, with a long neck, by which 'twas held in the hand near a charcoal fire, that kept the metal in fusion; being now and then shaken for almost half an hour; in which time, it seem'd to have acquir'd, on the surface, such a dark colour, as argued a beginning calcination; and it both emitted fumes that play'd up and down, and, also, afforded two or three drops of liquor, in the neck of the retort. The glass was, at length, laid on quick-coals, where the metal continued above a quarter of an hour longer in fusion; but, before the time was come, that I intended, to suffer it to cool, in order to its removal, it suddenly broke, into a great multitude of pieces, and with a noise, like the report of a gun. In the neck we found some drops of a yellowish liquor, which had an odious and peculiar taste; its smell also, was very fetid, not unlike the distill'd oil of fish.

24. Some tin, which had been, before, partly calcined in a glass, being melted again in a crucible, we weigh'd out just eight ounces, and these we put into a bolt-head of white glass, with a neck above twenty inches long, which being hermetically sealed, after the glass had been a while kept over the fire lest it should break by the rarification of the air, the metal was kept in fusion for an hour and a quarter. Being unwilling to venture the glass any longer, it was taken from the fire, and when grown cold, the seal'd end was broken off; but before I could have the bottom cut out, I observ'd, that the upper surface of the metal was very darkly colour'd, and very irregularly rough; and the lower part had, between the bottom, and the under side of the lump, a pretty deal of loose dark-colour'd calx, tho' the neighbouring surface, and some places of the lump it self, look'd, by candle-light, of a golden colour. The lump, and calx together, were weigh'd in the same scales carefully; when we found the weight to have increas'd above twenty-three grains; tho' all the calx we could easily separate, being weigh'd by it self, amounted not to eighty grains.

25. Two ounces of filings of tin, were carefully weigh'd, and put into a little retort, whose neck was afterwards drawn slender to a very small apex; then the glass was placed on kindled coals, which drove out fumes at the small orifice of the neck, for a pretty while. Afterwards, the glass, being sealed up at the apex, was kept in the fire for above two hours; and then being taken off, was broken at the same apex: whereupon I heard the external air rush in, because, when the retort was sealed, the air, within it, was highly rarified. Then the body of the glass being broken, the tin was taken out, consisting of a lump, about which there appear'd some grey calx, and some very small globules, which seem'd to have been filings melted into that form. The whole weigh'd two ounces, and twelve grains. In the neck of the retort, where it was join'd to the body, there appear'd a yellowish, and clammy substance, thinly spread, which smelt almost like the fetid oil of tartar.

26. We took an ounce of the filings of zink, carefully weigh'd; and having, as carefully, put them into a round bolt-glass, we caus'd the neck to

to be drawn out very slender, and then order'd it to be set upon quick-coals, where, being kept for four hours, there appear'd, for great part of the time, smoke to ascend from the zink, and get out at the unstopp'd apex. And I observ'd, that the upper part of the glass was lined with a darkish grey sublimate. The glass being dexterously cut asunder, we took out, not only the filings of zink, some of which were melted into little globules, but the *stones* too; yet, weighing all these in the same scales we had used before, we found above five grains wanting of an ounce.

27. Having carefully weigh'd out four ounces of good lead, cut into little pieces, I put them into a small retort with a long neck, wherein was, afterwards, left only an orifice, not much bigger than a pin's-head; this glass was kept over, and upon the coals, for above two hours; and then, supposing the danger of the glass's breaking to be over, we seal'd it up at the little orifice, and kept it on the coals for two hours longer. Before we broke the glass, I perceiv'd the pieces of lead to have been melted into a lump, whose surface was dark and rugged, and part of the metal to have been turn'd into a dark-colour'd powder, or calx. All this being taken out of the retort, was weigh'd in the same balance, whereon the lead appear'd to have gain'd, by the operation, somewhat above thirteen grains.

28. To shew that metals are not the only bodies capable of receiving an increase of weight from the fire, we seal'd up two drams of little bits of good red coral, hermetically, in a thin bubble of glass, and kept them warily, at several times, over and upon kindled coals; and, at length, being taken out, found them of a very dark colour, and to have gain'd, in weight, about $3\frac{1}{2}$ grains.

29. And having taken very strong fresh quick-lime, provided on purpose for choice experiments, and expos'd it, before the air had time to flake it, upon the cupel, to a strong fire, where it was kept for two hours; I found that it had increased, in weight, beyond my expectation: for, being seasonably put into the balance, the lumps that weigh'd, when expos'd, but two drams, amounted to two drams and twenty-nine grains. Hence it appears, that, notwithstanding a body may, for many hours, or even for some days, be expos'd to a very violent fire; yet it may be still capable of admitting, and retaining fresh corpuscles: so that, tho' well-made lime be usually observ'd to be much lighter than the stones whereof 'tis made; yet the lightness does not necessarily prove, that, because a burnt lime-stone has lost much of its matter by the fire, it has, therefore, acquired no matter from the fire; but only infers, that it has lost far more than it has got. And this may give ground to suspect, that, in most of the foregoing trials, the access of the fiery particles was greater than the balance discover'd; since, for ought we know, many of the less fix'd particles of the expos'd body, might be driven away, by the vehemence of the heat, and, consequently, the fiery corpuscles that fasten'd themselves to the remaining matter, might be numerous enough, not only to give the additional weight, found by the scales; but to make amends for all the fugitive particles that had been expell'd by the fire. And, since

STATICS. so fix'd a body as quick-lime, is capable of being wrought upon by the fiery effluvia; so that they come to be, as 'twere, incorporated with it; perhaps, in other calcin'd or incinerated bodies, the remaining calces or ashes may retain more than the bare impresson of the fire. By this experiment, and those made in seal'd retorts, which shew, that what is afforded by fire, may, in a corporeal way, invade, adhere, and add weight even to fix'd and ponderous bodies; there is a large field open'd, for the speculative to apply the discovery to many phenomena of nature and chymistry.

S E C T. II.

*Glass pervious
to ponderous
parts of flame.*

TO obviate some scruples, that might be entertain'd upon this circumstance of our experiments, that the glasses employ'd about them were not expos'd to the action of mere flame, but held upon charcoal; I attempted to make them succeed with a body so thin, and disengaged from gross matter, as mere flame is allow'd to be.

1. Supposing, then, that good common sulphur, by reason of its great inflammability, and the vehemency and penetrancy of its flame, would be a very fit fuel for my purpose; I provided a small double vessel, so contriv'd, that the one part should contain as many coals as were necessary to keep the sulphur melted; and the other, which was much smaller, and shaped like a pan, should contain the brimstone requisite for our tryal; and that these two should be, with a convenient lute, so join'd to one another, that all being clos'd at the top, except the orifice of the little pan, the fire and smoke of the coals having their vent another way, no fire should come at the retort to be employ'd, but the flame of the burning brimstone. Then two ounces of filings of tin being carefully weigh'd out, and put into a glass retort, provided for such trials, and made fit to be easily seal'd up at the neck; the sulphur, which ought to be of the purer sort, was kindled, and the glass, by degrees, expos'd to it; where that continued near two hours before the metal melted: after which, the retort was there kept on near an hour and a half more, with the metal melted in it; then perceiving a pretty deal of darkish calx at the bottom, and partly, too, upon the surface of the far greater portion of the metal, which now lay in one lump; the part of the retort which had been seal'd, being broken off, we first took out the calx, and next the lump, and putting them into the scales, they had been formerly weigh'd in, we found them to have acquired four grains and a half. The experiment was afterwards repeated with the same quantity of filings of the same metal, and at the end of the operation, which in all lasted somewhat above three hours, having broken off the seal'd neck of the retort, we found that a large proportion of dark-colour'd calx had been produced: and this being weigh'd with the uncalcin'd part of the metal, the two ounces, we first put in, appear'd to have acquir'd no less than eleven and a half.

Such

Such superstructures may, possibly, in time, be built on this and the like experiments, that I shall here obviate even a scruple, which appears too sceptical. But, considering that tho' it were very improbable, yet possible, that the increase of weight acquired by bodies exposed in glass vessels to the fire, might proceed not from the corpuscles of fire, but from the particles of the glass itself, loosen'd by the power of so intense an heat, and forcibly driven into the inclosed body; I took two glasses, the one shaped into a little retort, and having weigh'd them, kept them for a considerable time upon kindled coals, and then weigh'd them again, I could gather little of certainty from the experiment; (the retort at one time seeming to have acquired above half a grain in the fire) only that there was no likelihood at all, so considerable an increase of weight, as we several times obtain'd in closed vessels, should proceed from the glass itself, and not from the fire.

2. We carefully weigh'd out an ounce of the filings of block-tin, and put them into a glass retort, whose neck was afterwards drawn out slender; we also provided such a convenient metalline lamp, that the flame of the spirit burnt in it, might not melt or crack the glass. This lamp, tho' furnish'd with a cotton wick, afforded no foot; because, as long as it was well supplied with liquor, it remain'd unburnt. These things being in readiness, the retort was warily approach'd to the flame, and the metal thereby in a short time melted. After which, the glass being kept exposed to the same flame, for near two hours, the seal'd apex of the retort was broken off; when there appear'd to have been produced a considerable quantity of calx, that lay loose about the remaining part of the tin, which, upon its growing cold was harden'd into a lump. This and the calx being taken out of the retort, with care, that no little fragment of glass should impose upon us, it was weigh'd in the same scales as formerly, and found to have gain'd $4\frac{1}{2}$ grains, besides the dust that stuck to the inside of the retort; of which we reckon'd enough to make about half a grain more: so that of such a fine and pure flame as of this totally ardent spirit, enough to amount to five grains was arrested, and, in good measure, fix'd, by its operation on the tin.

3. For confirmation of the last experiment, wherein we had employ'd the inflammable spirit of sugar, we made the like with highly rectify'd spirit of wine; only substituting an ounce of lead instead of one of tin. The event was, that after the metal had been, for above two hours, kept in the flame, the seal'd neck of the retort being broken off, the external air rush'd in with noise; and we found above seven scruples of lead turn'd into a greyish calx; which, together with the rest of the metal, being weigh'd again, six grains appear'd to be gain'd by the operation. These seven scruples of calx being weigh'd in air and water, I found, that tho' the absolute gravity of the metal had been increased by the particles of flame which stuck fast to it; yet this aggregate of lead, and extinguished flame, had lost much of its specific gravity: for lead is to water of the same bulk, as about $11\frac{1}{2}$ to 1; whilst this subtil calx of lead was to water of the same bulk, but as 9 to 1.

Now

STATICS.

Corollaries from
this discovery.

Now whence can this increase of absolute weight, observ'd in the metals expos'd to the mere flame, be deduced, but from some ponderous parts of the flame? And how could these parts invade those of the metal inclosed in a glass, otherwise than by passing thro' the pores of that glass?

But I do not, by these experiments, pretend to make out the porosity of glass, any farther than with regard to some of the ponderable parts of flame; for, otherwise, glass is not easily penetrable.

That flame may
act as a men-
struum, and
make coalitions
with the bodies
it works on.

Upon the whole, it appears, 1st, That flame may be a menstruum, and work on some bodies, not only by making a notable comminution, and dissipation of their parts, but, also, by a coalition of its own particles with those of the fretted body; and thereby, permanently, add substance and weight thereto. Nor is it repugnant to flame's being a menstruum, that, in our experiment, the lead and tin expos'd to it, were but reduced to powder, and not dissolved in the form of a liquor, and kept in that state. For, besides that the interposed glass hinder'd the igneous particles from getting thro' in plenty enough, 'tis not necessary that all menstrua should be such solvents. For whether it be, that the menstrua we think simple, may be compounded of very different parts, whereof one may precipitate what is dissolved by the other, or for some other cause; it is certain, that some menstrua corrode metals, and other bodies, without keeping any considerable part dissolved; as may be seen, if you put tin to a certain quantity of *Aqua fortis*, which will, in a very short time, reduce it, almost totally, to a very white substance, that, when dry, is a kind of calx. And so, with a due proportion of oil of vitriol, abstracted from quick-silver, by a strong fire, we have several times reduced the main body of the mercury into a white powder, whereof but an inconsiderable part was dissoluble in water. And such a white calx I have had, by the action of another fretting liquor on a body not metalline. But whether, as it seems probable, it be from the vehement agitation of the permeating particles of flame, that violently tear asunder the metalline corpuscles; or, from the nature of the igneous menstruum, which being, as 'twere, strain'd thro' glass itself, must be strangely minute; 'tis worth observing how small a proportion, in point of weight, of the additional adhering body, may serve to corrode a metal, in comparison of the quantity of vulgar menstrua requisite for that purpose. For, tho' we are obliged to employ, to make the solution of crude lead, several times its weight of spirit of vinegar, or a large proportion of *Aqua fortis*; 'twas observ'd in our experiment, that tho' the lead was increased but six grains in weight, yet above a hundred and twenty of it were fretted into powder; so that the corrosive body appear'd to be but about the twentieth part of the corroded.

That calces of
metals are the
magisteries of
them.

2^{dly}, Another consequence, deducible from our discovery of the perviousness of glass to flame, may be this, that there is cause to question the truth of what is, generally, taken for granted about calcination; and, particularly, of the notion that chymists have entertain'd about the calces of metals and minerals. For, it does not appear, by our trials, that any proportion, worth regarding, of moist and fugitive parts, was expell'd in the

the calcination; but it appears very plainly, that, by this operation, the metals gain'd more weight than they lost: so that the main body of the metal remain'd entire, and was far from being either elementary earth, or a compound of earth and fix'd salt. From which very erroneous hypothesis, the chymists infer the sweet vitriol of lead, which they call *Saccharum Saturni*, to be but the sweet salt of it, extracted only by the spirit of vinegar, which does, indeed, plentifully concur to compose it. Whence, I conclude, that the calx of a metal, even made *per se*, that is, by fire alone, may be, at least in some cases, not the *Caput mortuum*, or *Terra damnata*, but a magistery of it. For, in the sense of the most intelligible of the chymical writers, that is properly a magistery, wherein the principles are not separated; but the bulk of the body being preserved, it acquires a new and convenient form, by the addition of the menstruum, or solvent, employ'd about the preparation. Besides, I have, several times, from the calx of lead, reduced actual lead. And, having once taken but about a third or fourth part of the calx of lead, produced by the flame of spirit of wine, I found, that, without any flux-powder, or other addition, but merely by the application of the flame of highly rectify'd spirit of wine, there would, in a short time, be obtain'd a considerable proportion of malleable lead; whereof the part I examined, was truly so.

3dly, One use, among the rest, we may make of the foregoing discovery, regards a controversy among the corpuscular philosophers. For some of them think, that when bodies are exposed in close vessels to the fire, tho' the igneous corpuscles do not stay with the bodies they invade; yet they, really, get thro' the pores of the interposed vessels, and permeate the included bodies in their passage upwards: whilst others will not allow the igneous corpuscles, which they take to be but vehemently agitated particles of terrestrial matter, to penetrate such minute pores as those of glass; but suppose the operation of the fire to be perform'd by the vehement agitation made of the small parts of the glass, and by them propagated to the included bodies; whose particles, by this violent commotion, are greatly alter'd.

The effects of fire upon bodies, in close glasses, not wholly owing to the agitation of the glass.

But tho' it be probable, that the brisk agitation communicated by the small parts of the glass to those of the body contain'd in it, may contribute much to the effect of the fire; and tho', by the small increase of weight we found in our exposed metals, 'tis very likely that far the greater part of the flame was excluded by the close texture of the glass; yet, on the other side, 'tis plain, that igneous particles pass'd thro' the glass, and not only divided and agitated the included bodies, but manifestly and permanently adhered to them.

4thly, We receive this further information from our experiments, that bodies very spirituous, fugitive, and minute, may, by being associated with proper particles, tho' of quite another nature, so change their former qualities, as to be arrested by a solid and ponderous body, to that degree, as not to be driven away from it by a fire intense enough to melt and calcine metals. For the foregoing trials seem plainly to discover, that

Particles extremely volatile may, by associating with others, lose their volatility.

STATICS.

that even the agitated parts of flame, minute enough to pass thro' the pores of glass itself, were, some way, entangled among the metalline particles of tin and lead; and thereby brought to be so fix'd, as to endure the heat that kept those metals in fusion, and gradually reduced them to calces: a phenomenon that one would not easily look for; especially considering how simple a texture that of lead, or tin, may be, in comparison of the more elaborate structures of many other bodies. And this phenomenon, which shews us what light and fugitive particles of matter, may permanently, concur to the composition of ponderous and fix'd bodies, will, perhaps, afford useful hints to the speculative; especially, if this strict combination of a spirituous and fugitive substance, with such as, being gross and unwieldy, are less fit than organized matter, to entangle, or detain them, be apply'd, as it may be, with advantage, to those aggregates of spirituous corpuscles, and organical parts, that make up the bodies of plants and animals. And this hint may suggest a considerable inference to be drawn from the operation of the sun-beams on appropriated subjects; supposing it to prove like that of flame on tin and lead*.

* The following queries of Sir Isaac Newton, give us the best light we have into the nature of fire and flame. "Is not," says that great philosopher, "fire a body, heated so hot, as to emit light copiously? For, what else is a red-hot iron, than fire? And, what else is a burning coal, than red-hot wood? Is not flame a vapour, fume, or exhalation, heated red-hot; that is, so hot as to shine? For, bodies do not flame, without emitting a copious fume; and this fume burns in the flame. The *Ignis fatuus* is a vapour, shining without heat: and, is there not the same difference between this vapour, and flame, as between rotten wood, shining without heat, and burning coals of fire? In distilling hot spirits, if the head of the still be taken off, the vapour, which ascends out of the still, will take fire at the flame of a candle, and turn into flame; and the flame will run along the vapour, from the candle, to the still. Some bodies, heated by motion, or fermentation, if the heat grow intense, fume copiously; and, if the heat be great enough, the fumes will shine, and become flame. Metals, in fusion, do not flame for want of a copious fume. Salt-petre fumes copiously, and thereby flames. All flaming bodies, as oil, tallow, wax, wood, fossil, coals, pitch, and sulphur, by flaming waste, and vanish into burning smoke; which smoke, if

the flame be put out, is very thick, and visible; and sometimes smells strongly: but, in the flame, it loses its smell, by burning: and, according to the nature of the smoke, the flame is of several colours; as that of sulphur, blue; that of copper, open'd with sublimate, green; that of tallow, yellow; that of camphire, white. Smoke passing thro' flame, cannot but grow red-hot; and, red-hot smoke can have no other appearance than that of flame. When gun-powder takes fire, it goes away into flaming smoke. For the charcoal and sulphur, easily take fire, and set fire to the nitre; and the spirit of the nitre, being thereby rarified into vapour, rushes out, with explosion, much after the manner that the vapour of water rushes out of an æolipile; the sulphur, also, being volatile, is converted into vapour, and augments the explosion. And, the acid vapour of the sulphur, namely, that which distils under a glass-bell, into oil of sulphur, entering violently into the fixed body of the nitre, sets loose the spirit of the nitre, and excites a great fermentation, whereby the heat is farther augmented, and the fixed body of the nitre is also rarified into fume; and the explosion is, thereby, made more vehement, and quick. For, if salt of tartar be mixed with gun-powder and the mixture be warm'd till it takes fire,

" fire, the explosion will be more violent,
 " and quick, than that of gun-powder a-
 " lone; which cannot proceed from any
 " other cause, than the action of the va-
 " pour of the gun-powder upon the salt
 " of tartar, whereby the salt is rarified.
 " The explosion of gun-powder arises,
 " therefore, from the violent action,
 " whereby all the mixture, being quickly
 " and vehemently heated, is rarified, and
 " converted into fume and vapour; which
 " vapour, by the violence of that action,
 " becoming so hot as to shine, appears
 " in the form of flame."

The following query is so beautiful, and
 opens such a glorious scene, that I should
 be tempted to add it, tho' it bore a less
 relation, than it does, to the present sub-
 ject.

" Do not " continues the same great au-
 " thor, " large bodies conserve their heat
 " the longest; their parts heating one a-
 " nother? And, may not a great, dense,
 " and fixed body, when heated beyond
 " a certain degree, emit light so copiously,
 " as, by the emission, and re-action of its
 " light, and the reflexions, and refractions
 " of its rays, within its pores, to grow
 " still hotter, till it comes to a certain pe-
 " riod of heat; such as is that of the sun?
 " And, are not the sun, and fixed stars,
 " great earths, vehemently hot; whose
 " heat is conserved by the greatness of the
 " bodies, and the mutual action, and re-
 " action between them, and the light
 " which they emit; and whose parts are
 " kept from fuming away, not only by
 " their fixity, but, also, by the vast weight
 " and density of the atmospheres, incum-
 " bent upon them; and very strongly com-
 " pressing them, and condensing the va-

" pours and exhalations which arise from
 " them? For, if water be made warm, in
 " any pellucid vessel emptied of air, that
 " water, in the vacuum, will bubble and
 " boil, as vehemently as it would in the
 " open air, in a vessel set upon the fire,
 " till it conceives a much greater heat.
 " For, the weight of the incumbent at-
 " mosphere, keeps down the vapours, and
 " hinders the water from boiling, till it
 " grow much hotter than is requisite to
 " make it boil *in vacuo*. Also, a mixture
 " of tin and lead, being put upon a red-hot
 " iron, *in vacuo*, emits a fume and flame;
 " but the same mixture, in the open air,
 " by reason of the incumbent atmosphere,
 " does not so much as emit any fume,
 " which can be perceiv'd by sight. In
 " like manner, the great weight of the
 " atmosphere, which lies upon the globe
 " of the sun, may hinder bodies there from
 " rising up, and going away from the sun,
 " in the form of vapours, and fumes, un-
 " less by means of a far greater heat than
 " that which, on the surface of our earth,
 " would very easily turn them into va-
 " pours and fumes. And the same great
 " weight may condense those vapours, and
 " exhalations, as soon as they shall, at
 " any time, begin to ascend from the sun,
 " and make them presently fall back a-
 " gain into him; and, by that action, in-
 " crease his heat, much after the same
 " manner that, on our earth, the air in-
 " creases the heat of a culinary fire. And
 " the same weight may hinder the globe
 " of the sun from being diminish'd; unless
 " by the emission of light, and a very
 " small quantity of vapours and exhala-
 " tions. *Newton. Optic. p. 316.-319.*



THE
P R E F A C E

It is the purpose of universal philosophy, which considers the pro-
PNEUMATICS.

per, before his eyes, in force. Indeed, the ancients had no notion of that part of philosophy, which seems to have never decayed of an engine which is drawn the air from vessels, and presses its vapour into them. Otto Gueric, the famous count of Magdeburg, first taught us to use Boerhaave's, but upon an experiment to do it, and, from a hint of that it was, that Mr. Boyle, improving upon the design of the vessel, originally conceived his air-pump: a machine, to which we are indebted for the solution of many phenomena in nature, and numerous experiments in the fluid, which, of all extended ones, seems to be the most familiar, and useful to us. A man of an ordinary capacity, possessed of such an engine as that, would have been surprised to know what use to make of it: but such a person as Mr. Boyle could not but apply it to many and fruitful purposes. In regard to style, regarded of all, by its name, he has included a great variety of proper figures, and after another, of which he has, before-hand, wrote down a long catalogue. And, as this engine was, at first, imperfect, but afterwards rendered completely perfect, as in different ways, I am induced to lay all the experiments made with it, by Mr. Boyle, found in the order wherein he wrote them, rather than to range them under joint heads. For, that they give the history of the air-pump, and its improvements, was more to him: but, of the other method had been taken, the advantage of these experiments, must have been lost in their different capacity, which would prove a greater inconvenience, than the joining different volumes, which, by means of this engine, is rendered, by means of the

PNEUMATICS

T H E P R E F A C E.

Pneumatics is that part of universal philosophy, which considers the properties, the use, and effects of the air; the fluid we breathe, and which is absolutely necessary to maintain the life of animals.

This is a subject, therefore, that cannot but be highly useful to inquire into: and herein Mr. Boyle appears to have exercised himself more than any philosopher before his time, or since. Indeed, the ancients had no notion of this part of philosophy; and seem to have never dream'd of an engine wherewith to draw the air from vessels, and prevent its return into them. Otto Gueric, the famous consul of Magdeburg, first, unless we except Friar Bacon, hit upon an expedient to do it; and, from a hint of this it was, that Mr. Boyle, improving upon the design of the consul, originally contriv'd his air-pump: a machine, to which we are indebted for the solution of many phenomena in nature; and momentous discoveries in the fluid, which, of all external ones, seems to be the most familiar, and useful to us. A man of an ordinary capacity, possess'd of such an engine as this, would have been perplexed to know what use to make of it; but such a genius as Mr. Boyle, could not but apply it to noble and serviceable purposes. In vessels of glass, emptied of air, by its means, he soon included a great variety of proper subjects, one after another; of which he had, before-hand, wrote down a long catalogue. And, as this engine was, at first, imperfect, but afterwards received considerable improvements, at different times, I am induc'd to let all the experiments, made with it, by Mr. Boyle, stand in the order wherein he wrote them, rather than to range them under several heads. For, thus they give the history of the air-pump, and its improvements from time to time: but, if the other method had been taken, the advantage of these experiments, must have been lost in their historical capacity; which would prove a greater inconvenience, than the seeming disorder wherein they now occur: especially considering, that this may easily be remedied, by means of the index.

The PREFACE.

Numerous are the experiments made by Mr. Boyle, with this engine ; but still there may be something left for future philosophers to do with it, besides repeating, varying, and confirming his trials. The air-pump, as 'tis now made, has many advantages over even the best which was employ'd by Mr. Boyle: it is so much more manageable, commodious, and exact, (especially as improved by that excellent mechanic, Mr. Hauksbee) its apparatus is so well suited for all exigencies of experiments ; and, the whole comes on such easy terms, in comparison of what it cost our generous philosopher, that, if it were only to be witness of the surprizing discoveries he made with it, 'tis pity any well-wisher to philosophy should be without it.

But, were this instrument in more general use ; and, would men of invention set themselves to contrive new experiments for it, who knows what farther light it might afford us ; and in producing how many other effects of nature, the air would appear to be concern'd ? The late Mr. Hauksbee made several experiments with it, which, perhaps, Mr. Boyle himself never thought of ; tho' he evidently contrived a large number ; which, for want of a fit opportunity, convenient glasses, and other apparatus, he could never see the event of. The subject, therefore, appears, far from being exhausted : the air-pump is a fruitful engine, and seems fitted to examine almost all the productions of nature : and, there are many things, in philosophy, that cannot be done without it. 'Twas this which lately open'd a new field of knowledge, and greatly assisted Mr. Boyle to introduce, or revive the mechanical philosophy ; this, evidently, help'd to overthrow the doctrine of nature's dread of a Vacuum, by which, the modish philosophy of his time, falsely accounted for abundance of phenomena ; to this is owing, the greatest part of those noble experiments, which appear under the following head of Pneumatics ; and, lastly, to this we owe some surprizing discoveries of Sir Isaac Newton, and a great share of the present system of philosophy.



Phyfico-

Phyfico-mechanical EXPERIMENTS,

To shew the

Spring and Effects of the AIR.

SECT. I.

THE air is so necessary to life, that most creatures, which breathe, cannot subsist, for many minutes, without it; and most of the natural bodies we deal with, being, as well as our own, almost perpetually contiguous to it, the alterations thereof have a manifest share in many obvious effects, and, particularly, in distempers: wherefore, a farther inquiry into the nature of this fluid, will, probably, shew, that it concurs to exhibit abundance of phenomena, wherein it has, hitherto, seemed little concerned. So that, a true account of any new experiment, upon a thing whereof we have such a constant and necessary use, may prove advantageous to human life.

The origin of the air-pump.

With this view, before ever I was informed that *Otto Gueric*, the ingenious consul of *Magdeburg*, had practised a way, in *Germany*, of emptying glass vessels of the air, I had made experiments on the same foundation; but, as that gentleman first produced considerable effects by this means, I acknowledge the assistance and encouragement which the report of his performances afforded me.

But, as few inventions happen to be compleat at the first, so the engine employ'd by the consul, seem'd very defective in its contrivance; whence but little more could be expected from it, than those very few phenomena observed by the author, and related by *Schattus*. I, therefore, put *Mr. Hook*, upon contriving an air-pump, more manageable and convenient, that might not, like the *German-engine*, require to be kept under water: and, after some unsuccessful attempts, he fitted me with one, consisting of two principal parts; a glass vessel, and a pump to evacuate the air.

The

PNEUMATICS.

Fig. 30.

The air-pump
described.

The first is a glass A, with a large mouth, a cover thereto, and a stop-cock fitted to the neck below. This would contain 30 quarts of water. B C, the mouth of it, is about four inches in diameter, and surrounded with a glass lip, almost an inch high, for the cover to rest on; wherein D E, is a brass ring, to cover, and be cemented on to the lip B C. To the internal orifice of this ring, a glass stopple is fitted, to keep out the external air. In the middle of this cover is a hole H I, half an inch in diameter, incircled with a ring, or socket; to which is adapted a brass stopple K, to be turn'd round, without admitting the least air. In the lower-end of this, is a hole 8, to admit a string, 8, 9, 10; which also passes thro' a small brass ring L, fixed to the bottom of the stopple F G, to move what is contain'd in the exhausted vessel, or receiver. That the stop-cock N, in the first figure, might perfectly exclude the air, we fasten'd a thin tin-plate, M T V W, to the shank of the cock X, all along the neck of the receiver, with a cement made of pitch, rosin, and wood-ashes, poured hot into the cavity of the plate; and to prevent the cement from running in at the orifice Z, of the shank X, it was stop'd with a cork fix'd to a string, that it might be drawn out at the upper orifice of the receiver; and then the neck of the glass, being made warm, was pressed into the cement, which thus fill'd the interstices betwixt the tin-plate and the receiver, and betwixt the receiver and the shank of the cock.

The lower part of our engine consists of a sucking-pump, supported by a wooden frame, with three legs 111, so contrived, that, for the freer motion of the hand, one side of it may stand perpendicular; and a-cross the middle of the frame we nail'd a piece of board 222, to which the principal part of the pump is fixed. The pump consists of an exact strong concave cylinder of brass, fourteen inches long, its cavity three inches in diameter; to which a sucker, 4455, is adapted, made up of two parts; one of which 44, is less in diameter than the cavity of the cylinder, with a thick piece of tann'd leather nail'd on it, whereby it excludes the air. The other part, a thick iron plate 55, is firmly join'd to the middle of the former, and is a little longer than the cylinder; one edge of it being smooth, and the other indented, to receive the teeth of a small iron-nut 227, fixed by two staples to the underside of the board nailed a-cross 22, on which the cylinder stands; and it is turn'd by the handle 7.

The last part of the pump is the valve R, a hole at the top of the cylinder, and taper towards the cavity; to this is fitted a brass-plug, to be taken out as occasion requires. The engine being thus contrived, some oil must be pour'd in at the top of the receiver upon the stop-cock, to fill up the interstices of its parts, and that the key S, may turn with the greater ease. A quantity of oil, also, must be left in the cylinder, to prevent the air from getting betwixt that and the sucker; for the like reasons, some must, likewise, be apply'd to the valve.

And here 'tis proper to observe, that when we used oil, or water, separately, for this purpose, and they have not answered the end, a mixture of

of the two has afterwards proved effectual. And, that the air may not enter betwixt the brass-cover and the ring, 'twill be convenient to lay some diachylon-plaister on their edges with a hot iron. That no air, also, may remain in the upper part of the cylinder, the handle is to be turn'd till the sucker rises to the top; and then, the valve being shut, it is to be drawn down to the bottom; by which means, the air being driven out of the cylinder, and a succession from without prevented, the cavity of the cylinder must be empty of air; so that, when the stop-cock is turn'd to afford a communication betwixt the receiver and the cylinder, part of the air before lodged in the receiver, will be drawn down into the cylinder; which, by turning back the key, is kept from entering the receiver again, and may, by unstopping the valve, and forcing up the sucker, be driven into the open air; and so, by repeated exfuctions out of the receiver, and expulsions out of the cylinder, the vessel may be exhausted as the experiment requires*.

I. Upon

* The air-pump has received great improvements since the time of Mr. Boyle, and seems brought to its utmost degree of simplicity, and perfection, by the late, and the present Mr. Hawksbee. This instrument, as 'tis Fig. 31. now made, by Mr. Hawksbee, consists of two brass-cylinders, *a a a a*, twelve inches high, and two their internal diameter. The emboli are raised, and depressed, by turning the winch *bb*, backward and forward. This winch is fasten'd to a spindle, passing thro' a lanthorn, whose pins serve for cogs, laying hold of the teeth of the racks *cccc*; so that one is depressed, and the other elevated reciprocally. By this means the valves, made of limber bladder, and fix'd on the upper part of each embolus, and at the bottom of the cylinders, mutually exhaust and discharge the same air from the receiver: which becoming nearly empty, the pressure of the external air on the descending embolus is so great, that the power required to raise the other, need but little surmount the friction of the moving parts; whence this pump becomes preferable to all others. The bottoms of the barrels lie in a brass-dish *dd*, its sides two inches high, containing water to keep the leather-collars, on which the cylinders stand, moist; whereby the air is precluded. The cylinders are screw'd hereon by the nuts *eeee*, which force the frontispiece *ff*, down upon them; thro' which pass the two pillars *gggg*. Each pillar has an iron

belonging to it, passing from them in the form of a swan's neck *gg*; these irons being fastened to the hind-part of the frame, to prevent their shaking. Between the two barrels, rises a hollow brass-wire *bbbb*, communicating with each of them, by means of a perforated piece of brass, lying horizontally from one to the other. The upper end of this wire is fasten'd to another piece of perforated brass, screw'd on below the plate *iiii*, which is ten inches over; having a brass-rim soldered on it, that it may contain water. Between the middle, and the side of this plate, rises a small pipe *k*, about an inch and half high; thro' which, into the hollow wire, passes all the air into the barrels from the receiver. Upon the plate of the pump is always laid a wet leather, for the receivers to stand on. This leather prevents the air's getting into the glasses, whose edges are ground true; and serves for this purpose vastly beyond any cement whatever. Another excellence in this pump, is the gage *llll*, a glass-tube about thirty-four inches long, so placed, that it cannot easily be damaged, or prove inconvenient. Its lower orifice is immersed in a glass of quick-silver *mm*; on the surface whereof is a perforated piece of cork for the tube to pass thro'. On this cork is placed a board of box-wood, about an inch in breadth, and grooved in the middle, to receive the tube, which is looped on thereto, that it may rise and fall as the

G g g

the

PNEUMATICS.

Some phenomena
of the engine
solved.

I. Upon drawing down the sucker of our engine, whilst the valve is shut, the cylindrical space deserted by it will be left empty of air; and, therefore, upon turning the key, the air contain'd in the receiver rushes into the cylinder, till, in both vessels, it be brought to an equal dilatation; so that, upon shutting the receiver, turning back the key, opening the valve, and forcing up the sucker again, almost a whole cylinder of air will be driven out after this first exsuction; but, after every succeeding stroke, less air will come out of the receiver into the cylinder: so that, at length, the sucker will rise almost to the top of the cylinder, before the valve need be open'd. And if, when it is so exhausted, the handle of the pump be let go, and the valve be stopp'd, the sucker, by the force of the external air, which is an over-balance to the internal rarify'd air, will be forced to the upper part of the cylinder, and higher, in proportion, as the air is more exhausted*. We observed, also, that, whilst any considerable quantity of air remains in the receiver, a brisk noise is immediately produced, upon turning the key.

The spring and
pressure of the
air explain'd.

But to render our experiments the more intelligible, we must premise, that the air abounds in elastic particles, which being pressed together by their own weight, constantly endeavour to expand and free themselves from that force; as wool, for example, resists the hand that squeezes it, and contracts its dimensions; but recovers them as the hand opens, and endeavours at it, even whilst that is shut. It may be alledg'd, that tho' the air consists of elastic particles, yet this only accounts for the dilatation of it in pneumatical engines, wherein it hath been compress'd, and its spring violently bent; by an external force; upon the removal whereof, it expands, barely to recover its natural dimensions; whilst, in our experiments, the air appears not to have been compressed, before its spontaneous dilatation. But, we have many experiments to prove, that our atmosphere is a heavy body, and that the upper parts of it press upon the lower. And I found a dry lamb's bladder, containing two thirds of a pint, and compress'd by a pack-thread tied about it, to lose, in a very tender balance, $1\frac{1}{2}$ grain of

the mercury ascends or descends in the gage. To the upper part of this tube is cemented a brass-head, that fits into the perforated brass-piece, screw'd on under the plate, and communicating both with the receiver, and the hollow brass-wire *bbbb*. The box board is graduated into inches and quarters, from the surface of the quick-silver to twenty-eight inches high; and thence 'tis divided into tenths. By this means, the degrees of rarification may, at all times, be nicely observed in an experiment. The air-cock *a*, which lets in the air, is, likewise, a screw on the same perforated brass, in which the upper parts of the gage, and the hollow wire, are inserted. oooo

represents a receiver, standing on the plate of the pump; on whose upper part *pp*, thro' a box of leather-collars, passes a slip of wire, to take up, let fall, or suspend any thing in the receiver, without admitting the air.

* The original air in the receiver, is always to the remainder, as the sum of the capacity of the vessel of the pump, raised to the power, whose exponent is equal to the number of the strokes of the sucker, to the capacity of the vessel raised to the same power. See this demonstrated by M. Varignon. *Memoir. de l'Academ. A.* 1705. p. 397.

its former weight, by the recess of the air, upon pricking it. Supposing, therefore, that the air is not destitute of weight, 'tis easy to conceive, that the part of the atmosphere wherein we live, is greatly compress'd by those directly over it, to the top of the atmosphere. And tho' the height of this atmosphere, according to *Kepler*, scarce exceeds eight miles, yet latter astronomers extend it six or seven miles farther. The learned *Ricciolo* makes it reach fifty miles high. So that a column of air, several miles in height, pressing upon some elastic particles of the same fluid here below, may easily bend their little springs, and keep them bent; as if fleeces of wool, were piled to a vast height upon one another, the hairs of the lowest locks would, by the weight of all the incumbent parts, be strongly compress'd. Hence it is, that, upon taking off the pressure of the incumbent air, from any parcel of the lower atmosphere, the particles of the latter possess more space than before. If it be farther objected against this condensation of the inferior air, that we find this fluid readily yields to the motion of flies, feathers, &c. we may reply, that as when a man squeezes wool in his hand, he feels it make a continual resistance; so each parcel of the air, about the earth, constantly endeavours to thrust away such contiguous bodies as keep it bent, and hinder the expansion of its parts; which will fly out towards that part, where they find the least resistance. And, since the corpuscles whereof the air consists, tho' of a springy nature, are so very small, as to compose a fluid body, 'tis easy to conceive, that here, as in other fluids, the component parts are in perpetual motion, whereby they become apt to yield to, or be displaced by other bodies; and that the same corpuscles are so variously mov'd, that, if some attempt to force a body one way, others, whose motion hath an opposite determination, as strongly press it the contrary way; whence it moves not out of its place; the pressure, on all sides, being equal. For if, by the help of our engine, the air be drawn only from one side of a body, he, who thinks to move that body, as easily as before, will, upon trial, find himself mistaken.

2. Thus, when our receiver is tolerably exhausted, the brass stopple in the cover, is so difficult to lift, that there seems to be some great weight fasten'd to the bottom of it: for, the internal air being, now, very much dilated, its spring must be greatly weakned; and, consequently, it can but faintly press against the lower-end of the stopple, whilst the spring of the external air keeps it down, with its full natural force. And, as the air is gradually admitted into the receiver, the weight is manifestly felt to decrease; till, at length, the receiver being again filled with air, the stopple may be easily lifted.

It may seem surprizing, that we speak of the air shut up in our receiver, as of the pressure of the atmosphere; tho' the glass manifestly keeps the incumbent pillar of air from pressing upon that within the vessel. But, let us consider, that if a fleece of wool, by pressure, be thus directly reduced into a narrow compass, and convey'd into a close box, tho' the former force ceases to bend its numerous springy parts, yet they continue

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as strongly bent as before; because we suppose the including box resists their expansion, as much as the force that crowded them in. Thus the air, being shut up in our glass when its parts are bent by the whole weight of the incumbent atmosphere, though that weight can no longer press upon it; yet the corpuscles of the internal air, continue as forcibly bent, as before they were included. If it be said, that the continual endeavour it has to expand itself, ought then to break the glass, we must observe, that the expansive force of the internal air, is balanc'd by pressure of the external, which preserves the glass intire; as, by the same means, thin large bubbles, made with soapy water, will, for some time, continue whole in the open air.

3. And though, by help of the handle, which is a lever, the sucker may easily be drawn down to the bottom of the cylinder; yet, without such a mechanic power, the same effect could not be produced, but by a force able to surmount the pressure of the atmosphere: as in the *Torricellian* experiment, if the column of mercury be too high, it will subside, till its weight be a balance to the pressure of the air. Hence we need not wonder, that tho' the sucker move easily in the cylinder, by means of the handle, yet, if that be taken off, it will require a considerable force to raise or depress it. Nor will it seem strange, that if, when the valve, and stop-cock are exactly closed, the sucker be drawn down, and then the handle let loose, that the sucker, as of itself, re-ascends to the top of the cylinder; since the spring of the external air, finds nothing to resist its pressure upon the bottom of the sucker. And, for the same reason, when the receiver is almost emptied, tho', the sucker being drawn down, the passage from the receiver to the cylinder be open'd, and then stop'd again, the sucker will, upon the letting go the handle, be forcibly carried up, almost to the top of the cylinder; because the air within the cylinder, being equally dilated and weakned with that of the glass, is unable to resist the pressure of the external air, till it be crowded into so little space, that both their forces are in equilibrium. So that, in this case, the sucker is drawn down with little less difficulty, than if, the cylinder being destitute of air, the stop-cock were exactly shut. It must also be observ'd, that when the sucker hath been impell'd to the top of the cylinder, and the valve is so carefully stop'd, that no air remains in the cylinder, above the sucker; if, then, the sucker be drawn to the lower part of the cylinder, no greater difficulty is found to depress the sucker, when nearer the bottom of the cylinder, than when it is much farther from it. Whence it appears, that the pressure of the external air, is not increas'd upon the accession of the air driven out; which, to make itself room, forceth the contiguous air to a violent sub-ingression of its parts, as some suppose; for otherwise the sucker would be more resisted by the external air as it comes lower; more of the displaced air being thrust into it, to compress it.

Bladders dilated by the spring of the air.

4. We took a large lamb's bladder, well dry'd, and very limber, and leaving in it about half the air it would contain, we strongly tied the neck of

of it; then conveying it into the receiver, the pump was work'd; and after two or three strokes, the imprison'd air began to swell in the bladder, and continued to do so, as the receiver was farther exhausted, till, at length, the bladder appear'd perfectly turgid. Then, by degrees, allowing the external air to return into the receiver, the distended bladder shrunk proportionably, grew flaccid, and, at last, appear'd as full of wrinkles as before.

And to try whether the actual elasticity of the fibres of the bladder, had any share in this effect, we let down to the former, two smaller bladders, of the same kind; the one not tied up at the neck, that the air it contain'd might pass into the receiver; the other, with its sides stretch'd out, and press'd together, that it might hold the less air, and then strongly tied up at the neck; and, whilst the first, upon working the pump, appear'd, every way distended to its full dimensions, neither of the others were remarkably swell'd; and that whose neck was left loose, seem'd very little less wrinkled than when first put in.

We made, likewise, a strong ligature about the middle of a long bladder, emptied of its air in part, but left open at the neck; and, upon exhausting the receiver, observ'd no such swelling betwixt the ligature, and the neck, as betwixt the ligature and the bottom of the bladder, where air was included.

5. We hung a dry bladder, well tied, and blown moderately full, in the receiver, by a string fasten'd to the inside of the cover; and, upon exhausting the glass, the included air first distended the bladder, and then burst it, as if it had been forcibly torn asunder. *And burst by the same.*

This experiment was repeated with the like success; and the bladder bursting, long before the receiver was fully exhausted, gave a great report.

But it was often, in vain, that we try'd to burst bladders, after this manner, because they were commonly grown dry, before they came to our hands; whence, if we tied them very hard, they were apt to fret, and so become unserviceable; and, if tied but moderately hard, their stiffness kept them from being closed so exactly, that the air should not get out into the receiver. We found, also, that a bladder moderately filled with air, and strongly tied, being held for a while, near the fire, grew exceeding turgid; and, afterwards, being brought nearer to the fire, suddenly burst, with so loud and vehement a noise, as made us almost deaf for some time after*.

6. Having

* M. Amontons shews, that the same degree of heat, how small soever, may perpetually increase the force of the air's spring, provided that air be continually press'd by a weight still greater and greater; and that any parcel of air, how small

soever, may perpetually increase the force of its spring, by a small degree of heat; provided this air be more and more press'd continually. The same gentleman, also, found by experience, that the heat of boiling water, which he shews to be the great-

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The dilatation
of air by its
spring, measu-
red.

6. Having thus found, that the air hath an elastic power, we were desirous to know how far a parcel of that fluid might be dilated by its own spring.

We thoroughly wetted a limber lamb's bladder, in water, that the sides of it being squeezed together, no air might be left in its folds, and strongly tied the neck of it about that of a small glass, capable of holding five drams of water; the bladder being first so squeez'd, that the air it contain'd was wholly forced into the glass, without being compress'd there; then the pump being set on work, the air, in the vial, soon began to dilate, produc'd a small tumor in the neck, and gradually came further into the bladder; elevating the sides, and displaying the folds, till, at length, it seem'd blown up to its full extent; when the external air, being permitted to return into the receiver, the air that had fill'd the bladder, was thereby reduced into its former narrow receptacle, and the bladder became flaccid and wrinkled, as before. Then taking out the bladder, and glass, we fill'd them both with water, thro' a hole made in the top of the bladder; and found the weight of it to be five ounces, five drams, and a half. So that the air, at its utmost expansion, possess'd above nine times the space it did when first put into the receiver.

But to measure the expansive force of the air more accurately, we took a cylindrical pipe of glass, its bore about a quarter of an inch in diameter, its length about seven inches, and left it open at one end; but the other, where it was hermetically sealed, had a small glass bubble, to receive the air, whose dilatation was to be measur'd. Along the side of this tube we pasted a slip of parchment, divided into twenty-six equal parts, marked with black lines, to measure both the included air, and its expansion. Afterwards we almost fill'd the tube with water; when, stopping the open end, and inverting it, the air was permitted to ascend to the bubble; and, as the ascent was very slow, it gave us the opportunity to mark how much more, or less than one of those divisions, this air took up. Thus, after a trial, or two, we convey'd to the top of the glass, a bubble of air, apparently equal to one of those divisions; then the open end of the tube being put into a small vial, whose bottom was cover'd with water, we included both glasses in a small slender receiver, and caused the pump to be work'd. The event was, that, at the first exsuction of the air, there seem'd not any expansion of the bubble, comparable to what appear'd at the second; and, after a very few strokes, the bubble, reaching as low as the surface of the subjacent water, gave us cause to think, that it would have expanded much farther, had there been room. We, therefore, took out the little tube, and found that, besides the twenty-six divisions, the glass bubble, and some part of the pipe, to which the parch-

ment
greatest that liquor is capable of, tho' ever so long detain'd upon a vehement fire, increases the spring of the air as much as about $\frac{1}{3}$ of the weight of the atmosphere, shewn by the barometer, in spring, or autumn:

and upon this foundation, he ingeniously attempts to establish an uniformity in thermometers. See *Memoirs de l'Academ. A.* 1702. p. 204. A. 1703. p. 61, &c.

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ment did not reach, amounted to six divisions more. Whence it appears, that the air possess'd one and thirty times more space than before; and yet seem'd capable of a far greater expansion. Wherefore, after the same manner, we let in another bubble, that seem'd but half as big as the former, and found that, upon exhausting the receiver, it did not only fill up the whole tube, but, in part, broke thro' the water in the vial; and thereby manifested itself to have possess'd above sixty times its former space.

Finding, then, that our tube was still too short, we took a slender conical one, thirty inches long, hermetically seal'd at the slender end, and almost fill'd it with water; and conveying a bubble of air to the top of it, we put the open end in a vial, as before: then the cover, by means of a small hole made in it, for the glass-pipe to come out at, was cemented to the receiver; and the pump being set on work, the air manifestly appear'd extended below the surface of the water; and some bubbles were seen to come out at the bottom of the pipe, and break thro' the water. This done, we left off pumping, and observ'd, that at unperceiv'd leaks of the receiver, the air got in so fast, that it very quickly impell'd up the water to the top of the tube; excepting a little space, whereinto that bubble was driven, which had before possess'd the whole tube. This air, at the slender end, appear'd to be a cylinder of $\frac{1}{2}$ inch in length; but when the pipe was taken out, and inverted, it seem'd, at the other end, less in bulk than a pea. Then, with a small pair of scales, weighing the tube and water, we found they amounted to one ounce thirty grains and a half; and filling the tube with water, and weighing again the pipe and water, we found the weight increas'd only by one grain. Lastly, pouring out the water, and carefully freeing the pipe from it, we weigh'd the glass alone, and found it wanted two drams and thirty-two grains of its former weight. So that the bubble of air possessing the space but of one grain weight of water, it appear'd that this air, by its own spring, was rarified to one hundred fifty-two times its former dimensions; tho' it had been compress'd only by the ordinary weight of the contiguous air. The experiment, indeed, was made in a moist night, and in a room with a large fire; which did, perhaps, somewhat rarify the bubble of air.

It hath seem'd almost incredible, what *Mersennus* relates, that the air, by the violence of heat, may be dilated so as to take up seventy times its natural space: we, therefore, once more, convey'd into the tube a bubble of the same bigness with the former; and prosecuting the experiment as before, we observ'd, that the air did manifestly stretch itself so, as to appear, several times, far below the surface of the water in the vial; and that, too, with a surface very convex toward the bottom of the pipe. Nay, the pump being ply'd a little longer, the air reach'd to that place, where the tube rested upon the bottom of the vial, and seem'd to hit against and rebound from it. Whence 'tis probable, if the experiment could be so made that the expansion of the air might not be resisted, it would yet enlarge its bounds, and perhaps stretch itself to more than two hundred times.

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*The strength of
glass, and the
advantages of
figure in sustain-
ing a pressure.*

times its former bulk. And this may render many phenomena of our engine credible; since, of that part of the atmosphere wherein we live, what we call the free air, and presume to be uncompress'd, is crowded into so very small a portion of the space, it would, if unresisted, possess.

7. To discover the strength of glass, and what interest the figure of a body may have in resisting a pressure, we made the following experiments.

A round glass bubble, capable of containing five ounces of water, being purposely blown very thin, and with a slender neck, we moderately emptied the receiver, and nimbly applied the neck of the bubble to the orifice of the bottom of it; and after turning the key of the stop-cock, we made a free passage for the air to come out of the bubble into the receiver; which it did with great celerity; leaving the bubble as empty as the receiver itself. We then let in the external air, which now press'd only on the outside of the exhausted bubble, being prevented from getting within it; nevertheless, it continued as intire as before; the roundness of its figure enabling it, tho' almost as thin as paper, to resist a pressure equal to that of the whole incumbent atmosphere. And repeating the experiment, we found again, that the pressure of the air, thrusting all the parts inwards, made them, by reason of their arched figure, so support one another, that the glass would not break.

Fig. 32.

8. We took a glass alembic, containing between two and three pints; the rostrum C, being hermetically seal'd; and at the top of it was a hole, wherein we cemented one of the shanks of a stop-cock; so that the glass being inverted, the wide orifice stood uppermost; and to this was cemented a cover of lead: the other shank of the stop-cock was also, with cement, fasten'd into the upper part of the pump, which beginning to be work'd, the remaining air became by much too weak to balance the pressure of the external air, when the glass was, with a great noise, crack'd almost half round, along that part of it where it began to bend inwards; as in the line A B; and upon attempting to evacuate more of the air, the crack appear'd to run further, tho' the glass, where it was broken, seem'd above twenty times as thick as the bubble employ'd in the preceding experiment. Hence it may seem strange, that taking another glass bubble, alike in all respects, for ought appear'd, to that just mention'd, sealing it up hermetically, and suspending it in the receiver, the exsuction of the surrounding air did not enable the internal air to break or crack it: and this prov'd the case, tho' the experiment were tried several times, with bubbles of different sizes. But, perhaps, the heat of the lamp, wherewith such glasses are hermetically seal'd, might rarify the contain'd air, and weaken its spring.

Fig. 33.

9. Into the neck of a common four-ounce vial, we put a slender pipe of glass, and carefully fasten'd it, with a mixture of pitch and rosin, to the neck thereof. This vial, containing water that reach'd considerably higher than the lower end of the pipe, was put into a small receiver, in such manner, that the glass pipe, passing thro' a hole in the leaden cover of the receiver, was

was principally without the vessel; which being exactly closed, we work'd the pump: but at the very first stroke, and before the sucker was drawn to the bottom of the cylinder, there flew out of the vial, a large piece of glass, with a surprizing violence and noise, so as to crack the receiver in many places.

For farther satisfaction, we repeated the experiment in a round glass, that would contain six ounces of water; which we put into a small receiver, so that the bottom of it rested upon the lower part of the receiver, and the neck came out thro' the leaden cover of the same. This vial we included in a bladder, before it was put in, and the receiver being clos'd, so that the outward air could not enter but by breaking thro' the vial, into whose cavity it had free access by the mouth, the sucker was nimbly drawn down; upon which, the external air immediately press'd forcibly, as well upon the leaden cover as the vial; and the cover happening to be in one place a little narrower, than the edge of the receiver, it was depress'd, and thrust into it so violently, that getting a little within the lip of the glass, it thrust out the side, where it was depress'd, so as to split the receiver. And having fitted a wider cover to the same receiver, and clos'd both that, and the crack with cement, we prosecuted the experiment in the former manner with this success; that, upon suddenly depressing the sucker, the external air burst the vial into above a hundred pieces, many of them exceeding small, and with such violence, that we found a wide rent, and many holes made in the bladder.

And to shew, that these phenomena were the effects of a limited force, and not of such an abhorrence of a vacuum, as must, upon occasion, exercise a boundless power, we try'd several thicker glasses, and found that the experiment would not succeed; for the glasses were taken out, as entire as they were put in.

And here, by the way, we may observe, that every small crack will not render a roundish receiver useless in our experiments, because, upon evacuation of the internal air, the external on all sides pressing the glass towards the center, thrusts the edges of the crack closer together.

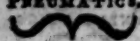
And, in case of considerable flaws, we successfully apply a plaister, made of quick-lime, finely powder'd, and nimbly ground, with a proper quantity of the scrapings of cheese, and fair water, enough to bring the mixture to a soft paste; which, when the ingredients are exquisitely incorporated, will have a strong, and fetid scent; and then, it must be immediately spread upon a linen cloth, and applied, lest it begin to harden.

10. We let down, into our receiver, a tallow-candle of a moderate size, and suspending it, so that the flame appeared in the middle of the vessel, we presently clos'd it up, and upon pumping found, that within little more, than half a minute after, the flame went out.

The flame of tallow and of wax in vacuo.

At another time, the flame lasted about two minutes, tho' upon the first exsuction it seem'd to contract itself in all its dimensions, and after two or three exsuctions, it appear'd exceeding blue, and gradually

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 receded from the tallow, till at length it seem'd to possess only the very top of the wick, and there it vanish'd.

The same candle, being lighted again, was shut into the receiver, to try how it would burn there, without exhausting the air; and we found that it lasted much longer than formerly; and before it went out, it receded from the tallow, towards the top of the wick, tho' not near so much, as in the former experiment.

We took notice, that when the air was not drawn out, a considerable part of the wick remain'd kindled upon the extinction of the flame, which emitted a smoke, that swiftly ascended directly upwards, in a slender and uninterrupted cylinder, till it came to the top, from whence it return'd, by the sides, to the lower part of the vessel; but when the flame went out, upon the exsuction of the air, we once perceiv'd it not to be follow'd by any smoke at all. And at another time, the upper part of the wick, remaining kindled after the extinction of the flame, a slender steam ascended, but a very little way, and after some uncertain motions, for the greatest part, soon fell downwards.

Joining together six slender tapers of white wax, as one candle, and having lighted all the wicks, we let them down into the receiver, and made what hast we could to close it up with cement. But, tho' in the mean while, we left open the valve of the cylinder, the hole of the stop-cock, and that in the cover of the receiver, that some air might get in to cherish the flame, and that the smoke might have a vent; yet the air sufficed not for so great a flame, till the cover could be perfectly luted on; so that before we were ready to employ the pump, the flame was extinguish'd. Wherefore, we took but one of the tapers, and having lighted it, clos'd it up in the receiver, to try how long a small flame, with a proportionable smoke, would continue in such a quantity of air; but we found, upon two several trials, that from our beginning to pump, the flame went out in about a minute. It appear'd, indeed, that the swinging of the wire, whereby, the candles hung, hasten'd the extinction of the flame, which seem'd, by the motion of the pump, to be thrown, sometimes on one side of the wick, and sometimes on the other. But, once refraining to pump, after a very few exsuctions, the flame lasted not much longer. And lastly, closing up the same lighted taper, to discover how long it would last, without drawing out the air; we found, that it burnt vividly for a while; but afterwards, began to diminish gradually in all its dimensions, tho' the flame did not, as before, retire itself by little and little towards the top, but towards the bottom of the wick, so that the upper part of it, manifestly appear'd for some time, above the top of the flame; which, having lasted about five minutes, was succeeded by a stream of smoke, that ascended in a strait line.

Kindled charcoal.
Fig. 34.

11. A spiral wire, fill'd to the height of about five inches, with wood-coals thoroughly kindled, being let down into the receiver, and the pump set to work; we observ'd, that upon the very first exsuction of the coals, the fire grew dim, and tho' the agitation of the vessel made them swing; yet, when we could no longer discern a redness in any of them,

we

we found that, from the beginning of the pumping, that is, about two minutes after the coals had been put in, glowing, to the total disappearing of the fire, there had pass'd three minutes. PNEUMATICS.

We then, presently, took them out, and found there had remain'd some little parcels of fire, rather cover'd, than totally extinguish'd; for, in the open air, the coals began to re-kindle, in several places. Wherefore, having, by swinging them about in the wire, thoroughly lighted them a second time, we let them down again into the receiver; and closing it, waited till the fire seem'd totally extinct, without working the pump, and found that from the time the vessel was closed, till no fire at all could be perceiv'd, there had elapsed four minutes.

Lastly, having taken out the wire, and put other coals into it, we, in the same room where the engine stood, let it hang quietly by a string, in the open air; and found that the fire began to go out first at the top, and outsides of the coals; but inwards, and near the bottom, it continu'd visible for above half an hour; a great part of the coals, especially the lowermost, being reduced to ashes before the fire was extinguish'd.

A piece of iron, of the bigness of a middle-sized charcoal, being, also, made red-hot throughout, we suspended it in the exhausted receiver; but could not observe any manifest change upon the exsuction of the air. The iron, indeed, began to lose its fiery redness at the top; but that seem'd owing to the upper-end's being somewhat more slender, than the lower; and the redness, tho' it were in the day-time, continued visible about four minutes; and then before it quite disappear'd, we let in the air, but no change ensued. Yet some little remainders of wax, that stuck to the wire, and were turn'd into fumes by the heat of the iron, afforded a more diffusive smoke when the air was drawn out, than afterwards; tho' allowance were made for the decreas'd heat of the metal. And lastly, notwithstanding a considerable extraction of the air, and the inconsiderable dissipation of the parts of the iron, the sides of the receiver were very sensibly hot, and retain'd a warmth for some time after the iron was taken out.

12. We suspended a piece of well-lighted match, in our receiver, with the lighted end downwards, when the fumes of it, almost, immediately fill'd, and darken'd the receiver. Wherefore, lest the vessel should be endanger'd, the pump was nimbly ply'd, and a great deal of air and smoke, mix'd together, drawn out; whereby the receiver growing more clear, we could discern the fire in the match, to burn, by degrees, more languidly; and, after no long time, it ceas'd to be discernible either by its light, or smoke. And tho' we continued pumping for a while longer, yet, upon admission of the external air, the fire, that seem'd to have been long extinguish'd, presently reviv'd, and began again to shine, and dissipate the adjacent fuel into smoke, as before. Lighted match.

13. We, afterwards, let down into the receiver, together with a piece of lighted match, a large bladder, well tied at the neck, and containing only about a pint of air, tho' capable of containing ten times as much.

This was design'd to try, whether the smoke of the match, replenishing the receiver, would hinder the dilatation of the internal air, upon the extraction of the external; and to discover whether the extinction of the fire in the match, proceeded from want of air, or, barely, from the pressure of its own fumes.

The event was, that, at the beginning of our pumping, the match appeared well lighted, tho' it had almost fill'd the receiver with smoke; but, by degrees, it burnt more dimly; tho', by nimbly drawing out the air, and smoke, the vessel became less opake: so that the longer we pump'd, the less air, and smoke, came out of the cylinder, upon opening the valve; yet the fire in the match, went out but slowly. And when, afterwards, we had darken'd the room, and, in vain, attempted to discover any spark of fire, we still continued pumping; and, at last, letting in the air, the fire quickly revived, yielded light, and plenty of smoke. Then we fell to pumping a-fresh, and continued it till long after the match went out again; so that in less than half a quarter of an hour, the fire was extinguished, beyond the possibility of a recovery by re-admitting the air. If the cylinder were emptied, when the receiver was full of smoke, immediately upon turning of the stop-cock, the receiver would appear manifestly darkened, to an eye viewing the light thro' it; and this darkness was less, as the receiver contain'd less smoke: it was also instantaneous, and seem'd to proceed from a sudden change of place and situation, in the exhalations, upon the vent afforded them, and the air they were mix'd with, out of the receiver into the cylinder. We also observ'd a kind of a halo, for a considerable time, about the fire, that seem'd to be produced by the surrounding exhalations. And, when the fumes seem'd most to replenish the receiver, they did not, sensibly, hinder the air, included in the bladder, from dilating itself, after the same manner it would otherwise have done: so that, before the the match was quite extinct, the bladder appear'd distended to six or seven times its former dimensions.

We, also, took a small receiver, capable of containing about a pound and a half of water, and, in the midst of it, suspended a lighted match; but tho' within a minute, from putting in the match, we had cemented on the cover, yet, before we began to pump, the smoke had so fill'd the receiver, as, apparently, to choke the fire. And finding it thus impossible to close up the vessel, and pump out the fumes soon enough to prevent the extinction of the fire, we used this expedient: as soon as we had pump'd once or twice, we suddenly turn'd the key, and thereby gave access to the excluded air, which rushing violently in, drove away the ashes, fill'd the glass with fresh air, and re-kindled the fire; and having, by this means, obtain'd a lighted match in the receiver, without spending time, to close it up, we exhausted the receiver, and found the match then quickly ceas'd to smoke.

And gun-pow-
der fired in va-
cuo.

14. We took a pistol, and having firmly ty'd it to a stick, almost as long as the cavity of the receiver, we primed it with dry gun-powder; then cocking it, we fasten'd the trigger to one end of a string, whose other end

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was fasten'd to the key in the cover of our receiver. This done, we convey'd the whole apparatus into the vessel, which being clos'd up, and empty'd after the usual manner, we turn'd the key in the cover, and thereby shortning the string, pull'd the trigger, and observ'd, that the force of the spring of the lock, was not sensibly abated by the absence of the air; for, the cock falling with its usual violence, struck as many, and as conspicuous sparks of fire, as, for ought we could perceive, it did in the open air. Upon often repeating this experiment, we could not perceive, but that the sparks of fire moved upwards, downwards, and side-ways, as when out of the receiver.

We, likewise, substituted a piece of steel for the flint, when, the pistol being cock'd, and convey'd into the receiver, we pull'd the trigger, after the air was drawn out; and tho' the place were purposely darkned, there appear'd not, upon the collision of the two steels, the least spark of fire. We have, indeed, found, that, by the dextrous collision of two harden'd pieces of steel, many sparks may be struck out; but that was done with such a vehement percussion of their edges, as could not well be procured in our receiver.

But most of our attempts, to fire the gun-powder in the pan of the pistol, fail'd, because we were obliged to let it hang, almost perpendicularly, in the receiver; whereby the powder was shook out, before the sparks could reach it. Once, however, the experiment succeeded; and the kindled powder seem'd to make a more expanded flame, than it would have done in the open air, and mounted upwards: upon the extinction of the flame, the receiver appear'd darkned with smoke, which seem'd to move freely up and down, and, upon letting in the air, began to circulate much faster than before.

15. We convey'd into a small receiver, a piece of combustible, dry, black matter; and carefully closing the vessel, we brought it to a window, at which the sun shone in very freely; then, drawing out the air, we, with a burning-glass, threw the sun's rays upon the combustible matter, which began immediately to send out a smoke that darkned the receiver; but, notwithstanding all our care, the external air got in, and frustrated the experiment.

An attempt to kindle a combustible body, by the sun's rays in vacuo.

We, therefore, lodg'd this combustible matter in the cavity of our largest receiver, so that it was almost contiguous to the side next the sun: we then endeavour'd to kindle it, but found, that by reason of the thickness of the glass, the sun-beams, thrown in by the burning-glass, were, in their passage, so dislocated, and scatter'd, that we could not, possibly, unite enow of them, to make the matter yield a sensib smoke.

16. We convey'd into the receiver, a little pedestal of wood, in the midst of which was, perpendicularly erected, a slender iron, upon the sharp point whereof, an excited needle of steel, of about five inches long, was so placed, that, hanging in equilibrium, it could move freely every way. Then the air being pump'd out, we employ'd a load-stone, moderately vigorous, to the outside of the glass, and found that it attracted, or repell'd

An excited needle in vacuo affected by the magnet.

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The Torricelli-
an experiment in
vacuo.

repell'd the ends of the needle, without any remarkable difference from what the same load-stone would have done, had none of the air been drawn away from about the needle; which, when the load-stone was remov'd, rested, after some tremulous vibrations, in a position north and south.

17. A slender, and very exact cylinder of glass, near three feet in length; its bore, a quarter of an inch in diameter; being hermetically sealed, at one end, was, at the other, filled with quick-silver; care being taken, that as few bubbles as possible, should be left in the mercury. Then the tube, being stop'd with the finger, and inverted, was open'd into a long, slender, cylindrical box, half fill'd with quick-silver; when that in the tube subsiding, and a piece of paper being pasted level to its upper surface, the box and tube were, by strings, carefully let down into the receiver; and the cover, by means of this hole, slit along as much of the tube, as reach'd above the top of the receiver: the interval left betwixt the sides of the hole, and those of the tube, being exquisitely fill'd up with melted diachylon; and the round chink, betwixt the cover and the receiver, likewise, very carefully clos'd; upon which closure, there appear'd no change in the height of the mercurial cylinder: whence the air seems to bear upon the mercury, rather by virtue of its spring, than of its weight; since its weight could not be suppos'd to amount to above two or three ounces; which is inconsiderable, in comparison of such a cylinder of mercury as it would sustain. Now the sucker was drawn down, and immediately, upon the evacuation of a cylinder of air, out of the receiver, the quick-silver in the tube subsided; and notice being carefully taken of the place where it stop'd, we work'd the pump again, and mark'd how low the quick-silver fell at the second exsuction: but, continuing thus, we were soon hinder'd from accurately marking the stages in its descent, because it presently sunk below the top of the receiver: so that we could, from hence, only mark it by the eye. And continuing pumping, for about a quarter of an hour, we could not bring the quick-silver, in the tube, totally to subside. Then we let in some air; upon which, the mercury began to re-ascend in the tube, and continued mounting, till having return'd the key, it immediately rested at the height it had then attain'd. And so, by turning, and returning the key, we did, several times, impel it upwards, and check its ascent; till, at length, admitting as much of the external air, as would come in, the quick-silver was impell'd up, almost, to its first height; which it could not fully regain, because some little particles of air were lodg'd among those of the quick-silver, and rose in bubbles to the top of the tube.

It is remarkable, that having, two or three times, try'd this experiment, in a small vessel; upon the very first cylinder of air that was drawn out of the receiver, the mercury fell, in the tube, 18 inches and a half; and, at another time, 19 inches and a half.

We, likewise, made the experiment in a tube less than two feet in length; and, when there was so much air drawn out of the receiver, that the remaining part could not counter-balance the mercurial cylinder, it fell
above

above a span at the first stroke ; and the external air being let in, impell'd it up again, almost to the top of the tube : so little matters it, how heavy or light the cylinder of quick-silver be, provided its gravity overpower the pressure of as much external air, as bears upon the surface of that mercury into which it is to fall.

Lastly, we observ'd, that if more air were impell'd up, by the pump, into the receiver, after the quick-silver had regain'd its usual standard in the tube, it would ascend still higher ; and immediately, upon letting out that air, fall again to the height it rested at before.

But, in order to fill the *Torricellian* tube with exactness, the edges of the open end should be made even, and turned inwards, that so the orifice, not much exceeding a quarter of an inch in diameter, may be the more easily, and exactly stop'd by the finger ; between which, and the quick-silver, that there may be no air intercepted, it is requisite that the tube be perfectly full, that the finger, pressing upon the protuberant mercury, may rather throw some out, than not find enough to keep out the air exactly. It is, also, an useful way, not quite to fill the tube, but to leave, near the top, about a quarter of an inch empty : for, if you then stop the open end, and invert the tube, that quarter of an inch of air, will ascend in a great bubble to the top ; and, in its passage, lick up all the little bubbles, and unite them with itself, into one great one. So that, if by re-inverting the tube, you let that bubble return to the open end of it, you will have a much closer mercurial cylinder than before ; and need add but a very little quick-silver more, to fill up the tube exactly. And, lastly, as for such less, and invisible parcels of air, which cannot be thus gather'd up, you may endeavour, before you invert the tube, to free the quick-silver from them, by shaking the glass, and gently knocking on the outside of it, after every little parcel of quick-silver pour'd in ; and afterwards, forcing the bubbles to disclose themselves, and break, by applying a hot-iron near the top of the glass ; which will raise the bubbles so powerfully, as to make the mercury appear to boil. I remember, that by carefully filling a short tube, tho' not quite free from air, we have made the mercurial cylinder reach to thirty inches, and above an eighth ; which is mention'd, because we have found, by experience, that in short tubes, a little air is more prejudicial to the experiment, than in long ones.

18. We fill'd a glass tube, about three feet long, with mercury ; and having inverted it into a vessel of other quick-silver, that in the tube, fell down to its usual height ; leaving some little particles of air in the space it had deserted : for, by the application of hot bodies, to the upper part of the tube, the quick-silver would be a little depress'd. Lastly, having put both the tube, and the vessel whereon it rested, into a convenient wooden frame, we placed them together in a window of my chamber.

And during several weeks, that the tube continu'd there, I observ'd, that the quick-silver did, sometimes faintly imitate the liquor of a thermometer,

Odd phenomena
of the mercurial
barometer.

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meter; subsiding a little in warm, and rising a little in cold weather; which we ascrib'd to the greater, or lesser pressure of that little air, which remain'd at the top of the tube, expanded, or condens'd by the heat, or cold of the ambient air. But, the quick-silver often rose, and fell in the tube very considerably, after a manner, quite contrary to that of weather-glasses, where air is at the top; for sometimes, I observ'd it, in very cold weather, to sink much lower, than at other times, when the air was comparatively warmer. And sometimes, the quick-silver would, for several days together, rest almost at the same height; and at others, it would in the compass of the same day considerably vary its altitude; tho' there appear'd no change, either in the air abroad, or in the temper of that within my room, nor in any thing else, to which such a change could reasonably be imputed; especially considering, that the space wherein the mercury continued unsettled for five weeks, amounted to full two inches; descending in that time about $\frac{1}{2}$ of an inch from the place where it first settled, and ascending the other inch, and $\frac{1}{2}$: and when we took the tube out of the frame, after it had staid there part of *November*, and *December*, a large fire being then in the room, we found the mercurial cylinder to be above the upper surface of the stagnant mercury $29 \frac{1}{4}$ inches*.

Such

* That the quick-silver in the barometer should stand lower, when the air is thick and moist, than when it is dry, and clear, seems to overthrow the theory of the air's gravitation. Indeed, to discover the causes of all the minute variations in the air, is a very difficult task. The winds have a great share herein, with the vapours, exhalations, and expirations of the earth; perhaps also, the changes, which happen in the adjacent regions; the flux and reflux caused by the moon in the air, no less than in the sea, and many other particulars, are not unconcern'd. Now, the air is heavier, than the vapours it sustains; its particles being more gross, and arising from denser bodies, than the particles of vapours. But, winds may change this weight of the air, in any particular region; either by bringing, and keeping up more air over it, as may easily happen, when two contrary winds blow; or by sweeping it away, and affording room for the subjacent air to expand itself; as may be the case, when two opposite winds meet, or, when only one blows exceeding strong. Thus, 'tis fact, that violent gulls make the mercury in the barometer greatly to sink of a sudden. The cold nitrous particles of the air, or the air itself, being condensed by cold in the north, and

blown to another quarter, may, not only condense the atmosphere, but make it heavier. Moreover, heavy dry exhalations will increase the weight of the air, (as salts and metals dissolv'd in proper menstrua, increase the specific gravity of them;) and perhaps, at the same time, add to its elasticity. Again, the air, by these, or the like causes, being rendred heavier, is the more able to sustain the vapours; which therefore coming to be intimately mix'd therewith, and floating every where uniformly therein, render it fair and clear. But, when from contrary causes it becomes lighter, 'tis rendred unable to sustain the vapours, which always oppress it; so that being, as it were, precipitated together, they form clouds, and running into drops, fall, by their increased gravity, to the earth. Hence we see, what causes render the air heavier, and more able to sustain the quick-silver in the barometer, namely, such as make the air clear and dry: but the causes, which render the air light and unfit to sustain the mercury, produce rain. When therefore, the air is lightest, and the mercury in the barometer lowest, the clouds appear very low, and in very swift motion; and the air having clear'd itself of its clouds by rain, becomes very bright and trans-

Such an inequality in the rise, and fall of the mercury will, I fear, render it difficult to determine by the barometer, whether the moon be the cause of the tides, especially, till the reason of this odd phenomenon be certainly known; which seems principally to depend upon considerable alterations in the air, in point of rarity and density.

19. We took a tube of glass, about four feet in length, hermetically seal'd at one end, fill'd it with common water, and inverted the open end, beneath the surface of a vessel of water. Then this vessel, with the tube in it, being let down into the receiver, the pump was set on work; when, till the receiver was moderately exhausted, the tube continu'd quite full of water; it being requisite, that a great part of the air contain'd in the receiver should be drawn out, to bring the remaining to an equilibrium, with so short a cylinder of water. But, when once the water began to fall in the tube, each exsuction of air made it descend a little lower; tho' nothing near so much, nor so unequally, as the quick-silver did. The lowest, we were able to draw down the water, was, to about a foot above the surface of that in the vessel. And, when the water was drawn down thus low, we found, that by letting in the outward air, it might be immediately impell'd up again, to the higher parts of the tube.

A like experiment made with water.

Upon making this experiment in a small receiver, we observ'd, that at the first exsuction of the air, the water usually subsided several inches; and at the second, sometimes near two feet; whereupon letting in the external air, the water was impell'd up, with a very great velocity.

20. That the air hath a considerable elastic power, we have abundantly proved: but, whether water participates, in any measure, thereof, seems hitherto, to have been scarce consider'd.

Whether water be elastic?

Into a large glass bubble, with a long neck, we pour'd common water, till it reach'd about a span above the bubble; and a piece of paper being pasted thereon, we put it, unstopp'd, into the receiver; when, the pump

transparent, so as to afford an excellent prospect of remote objects. But, when it is heavy, and the quick-silver stands high in the barometer, the heavens appear fair, but somewhat thick, by reason of the vapours, every where equally dispersed therein, and is less fit to afford a good view of objects at a distance. And if any clouds are seen, they be very high, and move slow. When the air is at the heaviest, thick clouds sometimes cover the earth, consisting probably of such exhalations, as the air, at that time, is unable to sustain; and which, cannot float therein, when 'tis light. In our climate, the barometer stands highest, when the weather is coldest, and when the east, or north-east winds blow; because, at that time, two winds blow together, from op-

posite parts; for in the *Atlantic* ocean, at the degree of latitude answering to ours, the wind, almost continually blows west; and when the north-wind blows, an air condens'd by cold is brought to us. Farther, in the most northern regions, the height of the barometer varies more, than in the southern; the winds being there more strong, changeable, and contrary to one another, on a small tract of land; whereby, at one time, they heap up, and condense the air, and at another, sweep it away, and rarify it. Lastly, the barometer varies least between the tropics, because the wind is there almost always gentle, and blows the same way. See *Clark. Annotat. in Robault. & Philos. Trans. No. 181. 291.*

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was work'd, after the usual manner, and a considerable part of the air in the receiver drawn out, before we discern'd any expansion of the water; but continuing to pump, the water manifestly began to ascend in the stem of the glass, and several bubbles, from the lower parts of the vessel, made their way thro' the liquor to the top of it, and there broke into the receiver. After the water once appear'd to swell, at each time the air was let out from the receiver into the pump, the water in the neck of the glass, suddenly rose, about the breadth of a barley-corn, and so by degrees attain'd to a considerable height, above the mark. And at length, the external air, being suddenly re-admitted, the water immediately subsided, and deserted all the additional space, it had gain'd in the glass.

21. We convey'd into the receiver a new glass-vial, capable of holding about six or seven ounces of water; into which we had before-hand put only two or three spoonfuls of that fluid, and stopp'd it close with a fit cork. The receiver being emptied, there appear'd no change in the inclosed water; the air, imprison'd with it, not having the force to blow out the stopple. Wherefore, we again put in the vial, less firmly clos'd than before; but when the air was pumped out of the receiver, that within the vial quickly found little passages to get out at: for when the vial was put in the time before, the water remain'd all the while perfectly free from bubbles; but now the bottom of the glass appear'd all cover'd with them, which, upon the return of the excluded air, presently shrunk up.

Hence it seem'd deducible, that, whilst the vial continu'd well stopp'd, the included water sustain'd, from the air shut up with it, a pressure equal to that of the atmosphere; since, till the air could get out of the glass, there appear'd no bubbles in the water, notwithstanding the want of pressure in the ambient body.

But, further, we caus'd a convenient quantity of water to be hermetically seal'd up in a glass-egg, whose long neck was fasten'd to one end of a string, the other end whereof was ty'd to the cover of our receiver; then the egg being convey'd into the receiver, and that being evacuated, we, by turning the brass-stopple, so shorten'd the string, as to break the glass; whereby liberty being given to the air imprison'd in the egg, to pass into the receiver, its sudden recess made so many bubbles appear immediately, and ascend so swiftly in the water, that their motion look'd like that of a violent shower of rain; except that the bubbles did not, like the drops of rain, tend downwards, but upwards; as happens in the dissolution of seed-pearl, in some very acid menstruum, wherein, if a large quantity of the pearls be cast whole, they will, at first, be carry'd in swarms from the bottom to the top of the liquor. And, without sealing up the glass, this experiment may be try'd in a small receiver: for the air may here be drawn out so soon, that the bubbles, lurking in the water, will, immediately, display themselves, and ascend in throngs. So that, having made the experiment, in such a receiver, with red wine, instead of water, the wine appear'd all cover'd with a large vanishing white froth.

22. To discover whether the expansion of the water really proceeded from an elastic power in the parts of that fluid; we fill'd a glass-vial, with a pound and some ounces of water, and then put into it a glass-pipe, open at both ends, and several inches in length, so as to reach a little below the neck; then we carefully cemented it thereto, that no air might come into the vial, nor any water get out of it, but thro' the pipe; and the pipe, being warily fill'd about half way with water, and a mark being pasted over-against the upper surface thereof, the whole was, by strings, let down into the receiver: when, pumping out the air, the water in the pipe began to rise, while some little bubbles discover'd themselves on its sides; and, soon after, the water still swelling, there appear'd, at the bottom of the pipe, a bubble, about the bigness of a small pea; which, ascending thro' the tube to the top of the water, staid there a while, and then broke. But the pump being nimbly ply'd, the expansion of the water so increased, that, quickly getting up to the top of the pipe, some drops of it began to run down along the outside of it; which oblig'd us to forbear pumping a while, and let it subside, as it did, within less than two inches of the bottom of the pipe. Then the pump being again set on work, the bubbles began to ascend from the bottom of the pipe; of which we reckon'd about sixty large ones, that ascended one after another. And, at length, letting in the external air, the water, in the pipe, instantly fell down almost to the bottom of it.

When the greater part of the air had been pump'd out of the receiver, the bubbles ascended so very slowly in the pipe, that their progress was scarce discernible; their magnitude not permitting them sufficiently to expand themselves in the cavity of the glass, without pressing against the sides of it. And, what seems strange, these bubbles were commonly much larger than those which rose before them; some of them being equal in bulk to four or five peas.

And tho', in ordinary bubbles, the air, together with the thin film of water that invests it, commonly swells above the surface of the water, and constitutes hemispherical bodies; the little parcels of air, that came up after the receiver was tolerably emptied, did not make protuberant bubbles; but such, whose upper surface was either level with, or beneath that of the water: so that, the upper surface being usually somewhat convex, the less protuberant parts had a quantity of water above them.

We farther observ'd, that, in the bubbles which first appear'd, the ascending air made its way upwards, by dividing the water thro' which it pass'd; in those that rose at the latter end of the experiment, the ascending parcels of air, having now little more than the weight of the incumbent water to surmount, were able to expand themselves, so as to fill that part of the pipe which they pervaded, and, by pressing every way against the sides of it, to raise what water they found above them, without letting any considerable quantity glide down along the sides of the glass: so that, sometimes, we could see a bubble thrust on before it a whole cylinder of water, perhaps an inch high, and carry it up to the top

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of the pipe; tho', upon letting in the external air, these bubbles suddenly vanish.

Hence it appears, that the air, and other bodies under water, may be press'd upon as well by the atmosphere, as by the weight of the incumbent water. Hence, likewise, it cannot from the preceding experiment be safely concluded, that water uncompressed, has an elastic power; since the intumescence, produced in that experiment, may be ascribed to the numerous little bubbles produced in water, freed from the pressure of the atmosphere. And hence, lastly, it seems probable, that, in the interstices of water, there lie conceal'd many parcels either of air, or something analogous thereto; tho' so very small, that they have not been hitherto suspected to lurk there.

23. It may, indeed, be conjectur'd, that these bubbles proceed not so much from any air in the water, as from the more subtil parts of the water itself.

We, therefore, repeated our former experiment, in a three-foot tube, fill'd with water, and in a small receiver; and found, that, upon the subsiding of the fluid, so many bubbles, visibly broke into the upper part of the tube, that, having afterwards let in the external air, the water was not thereby impell'd to the top, within more than half an inch. Then we, again, drew the air out of the receiver, and found, that, by reason of the body which possess'd the top of the tube, we were able, not only to make the water fall to a level, with the surface of that in the vessel; but also a great way beneath it. Now, since this could not well be ascribed to the bare subsiding of the water by its own weight, the water seems to have been depressed by the air. And, indeed, the surface of the water, in the tube, was much more concave than usual. And, by the way, when the water, in the pipe, was sunk almost as low as the water without; we observ'd, that, by the bare application of the hand, moderately warm, to the deserted part of the tube, the remaining water would be, suddenly, considerably depress'd. And having, for a while, held a kindled coal to the outside of the tube; the air was, by the heat, so far expanded, that it quickly drove the water to the bottom of the tube, which rested several inches below the surface of the ambient water. Hence it appears, that the air, when expanded to between ninety, and a hundred times its natural dimensions, will, yet, readily admit of a much farther rarification, by heat.

But, to proceed; in case our bubbles were produced by air, lurking in the water; that air being got together at the top of the tube, I imagin'd, if the receiver were again exhausted, bubbles would not rise, as before: and, accordingly, the air being again pumped out, the water, in the tube, descended; but, for a great while, we scarce saw one bubble appear; only when the receiver had been very much exhausted, and the water fallen very low, we discover'd, near the bottom of the tube, some little ones, which seem'd to consist of such parcels of air, as had not, by reason

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son of their smallness, got up to the top of the water, with the more bulky and vigorous sort. And having, by letting in the air, forced up the water into the tube, we could not perceive that it ascended near the top, tho' the engine remain'd unemploy'd for two or three nights together. Having, also, try'd a like experiment with quick-silver, instead of water, in a tube about a foot and a half long; upon drawing down the quick-silver as low as possible, and letting in the external air, we found, that some lurking particles of air were got up to the top of the tube, and hinder'd the quick-silver from rising to that height again. And, tho' the mercury were, by this means, brought to appear as a very close cylinder; yet the air, in the receiver, being again evacuated, I could perceive several little bubbles fasten'd to the inside of the tube, near the bottom. And, having purposely watched one or two of the principal, I observ'd, that tho' they grew gradually bigger, as the surface of the mercurial cylinder fell nearer to them; so that, at length, they swell'd to a considerable bulk; yet, upon letting in the air, they did not break, but presently thrunk up, till they became invisible.

Hence, it seems highly probable, that, even in the closest, and most ponderous liquors, and, therefore, much rather in water, there may lurk undiscernible parcels of air, capable, upon the removal of the pressure of the atmosphere, and that of the liquor wherein it lurks, to produce conspicuous bubbles.

From these several particulars, it seems plain, that the bubbles we have been treating of, were produced by such a substance, as may be properly enough call'd air; tho' we do not, positively, determine, whether air be a primogenial body, that cannot be generated, or turn'd into water, or any other body. This seems an important question, and might greatly conduce to explain the nature of the air.

Many naturalists esteem the air to be ingenerable, and incorruptible; and plausible reasons may be drawn, to countenance this opinion, from the permanency required in the corporeal principles of other bodies. Schortus tells us, that, in the *Museum Kircherianum*, there is a glass, near half full of ordinary spring-water, which, having been hermetically seal'd up by the famous *Clavius*, is, to this day, preserv'd not only clear and pure, but without, in the least, turning into air, tho' it has stood for fifty years.

Whether air may
be generated, or
transmuted.

Nor doth it appear, in those glasses which are hermetically seal'd for chymical uses, that the included air, during its long imprisonment, notwithstanding the alteration it receives from various degrees of heat, discernibly alters its nature; whilst we plainly perceive, in digestions and distillations, that, tho' water may be rarify'd into vapours; yet it is not, really, changed into air, but only divided by heat, and diffused into very minute parts; which, meeting together, presently return to such water as they constituted before. And even spirit of wine, and other subtle and fugitive spirits, tho' they readily fly into the air, and mingle with it, do yet, in the glasses of chymists, easily resume the form of liquors. And so volatile

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volatile salts, tho' they will readily disperse themselves in the air, and play up and down the capacity of a receiver; yet, after a while, fasten themselves to the inside thereof, in the form of salts.

And the experiment made in our engine, with a piece of match, seems to shew, that even those light and subtile fumes, into which the fire itself shatters dry bodies, have no such spring as that of the air; since they were unable to hinder the expansion of the air, included in a bladder they surrounded. *Josephus Acosta*, indeed, tells us, that he saw, in the *West-Indies*, some grates of iron so rusted and consumed by the air, that the metal crumbled between the fingers, was like parch'd straw. *Varenus*, also, tells us, that, in the islands call'd *Azores*, the air is so sharp, as, in a short time, to fret not only iron-plates, but the very tiles upon the roofs of houses, and reduce them to dust. But it may be said, that these authors ascribe such effects, chiefly, to the winds; and that the corrosion of the iron may proceed not from the air itself, or any of its genuine parts; but from some saline corpuscles dispersed thro' it, and driven, by the winds, against the bodies it is presumed to fret.

But, to try whether water could be turn'd into air, we fill'd an æolipile therewith; and placing it upon kindled coals, when the heat forc'd out a vehement stream of aqueous vapours, we ty'd an empty bladder about the neck of it; and finding the æolipile, after a while, to blow up the bladder, we carefully ty'd it again, that the included substance might not get away. Then slipping it off from the æolipile, we convey'd it into our exhausted receiver, and found, that the included substance expanded to a much greater bulk than before. And, having again taken out the bladder, we suffer'd it to remain ty'd up till the next morning, when it appear'd little less tumid: but, upon repeating the experiment, I found it very difficult to make it so accurately, as to shew, that water may be rarify'd into true air.

On the other hand, we found, by experience, that water, rarify'd into vapour, may, for a while, resemble the elastic power of the air. For, if you fill a convenient æolipile with water, and lay it upon quick-coals, you may, after a while, observe so great a pressure of some of the parts, contain'd in it, upon others; that the water will, sometimes, be thrown up into the air, above three or four feet high. And, if you then take the æolipile, almost red-hot, from the fire, you may perceive, that the water will, for a considerable time, be spouted out in a violent stream. And, if there remains but little water in the æolipile, when 'tis thus taken from the fire; immersing the neck of it into cold water, you will find, that, after it begins to draw some of it in, there will be generated, from time to time, many large bubbles in that water wherein the neck was plunged. These bubbles seem manifestly to proceed from hence, that, for a while, the heat, in the æolipile, continues strong enough to rarify part of the water that is suck'd in, and expel it, in the form of vapours, thro' that incumbent on the pipe. If, also, when the æolipile is almost full of water, you hold a fire-brand in that stream of vapours which issues out of

of the narrow mouth thereof, it will be very strongly blown with a considerable noise. And it has been observed, that, by placing the brand almost at the mouth of the æolipile, the wind appear'd more vehement, than if it were held some inches from it.

The elastic power of this stream, indeed, seems manifestly owing to the heat that expands, and agitates the aqueous particles thereof; and such rapid winds seem to be but water broke into little parts, and put in motion; since, by holding a solid, smooth, and close body against it, the vapours condensing thereon, will presently cover that body with water.

But *Kircher* relates a remarkable experiment, which seems to shew, that water is convertible into air. He tells us, that he made an hydraulic organ, which was supplied with wind after the following manner. "There was built a little chamber A H, five feet high, and three broad, with two transverse partitions C D, and E F, perforated like a sieve; under these ran a pipe G, which carried the water that, by a stop-cock, was let out at H: the water, therefore, rushing in violently at G, excited a very great wind within; which bringing too much moisture along with it, the partitions were contrived to purge it therefrom, that it might be conveyed more pure thro' the pipe A: but to render the air still more pure, we made a spiral tube of lead Q R, and inserted it into the vessel S: by which means the air arriv'd at the organ, thro' the orifice Z, as dry as if it had come out of an oven".

Now, if the wind that blows the organ here, doth not, upon the cessation of its unusual agitation, gradually relapse into water, I should strongly suspect, that 'tis possible for water to be easily turn'd into air; for it can scarce seem probable, that so little air, as is commonly contain'd in water, should be able, in so small a quantity of water, as seems here employ'd, to make so violent a wind as our author speaks of. I, therefore, suspect that the wind, in this case, may be produced by small particles of the water it self, forcibly expell'd out of the chamber into the organ. And tho' no heat intervenes, perhaps, motion alone, if vehement, may suffice to break water into very minute parts, and make them ascend upwards, if they cannot, otherwise, more easily, continue their agitation. For, I remember, that betwixt *Lyons* and *Geneva*, where the *Rhone* is suddenly straitened by two rocks, exceedingly near each other, that rapid stream, dashing, with great impetuosity, against them, breaks part of its water into such minute corpuscles, and gives it such a motion, that a mist, as it were, may be observ'd at a considerable distance, arising from the place, and ascending high into the air. But, it seems odd, that aqueous vapours should, like a dry wind, pass thro' such a long winding pipe of lead, as that described by our author; since we see, in the heads of stills, and in the necks of æolipiles, such vapours are presently, even by a very little cold, condensed into water.

We took a clear glass bubble, capable of containing three ounces of water, with a long and wide cylindrical neck; this we fill'd with oil of vitriol, and fair water, of each almost a like quantity; and casting in six small.

PREPARATION. Small iron nails, we stop'd the mouth of the glass, which was now full of liquor, with a piece of diaphalma, and speedily inverting the bubble, we put the neck of it into a small wide-mouth'd glass, with more of the same liquor in it; and as soon as the neck had reach'd the bottom of the liquor, there appear'd, at the upper-part of the vial, a bubble, about the bigness of a pea, which seem'd rather to consist of new small bubbles, produc'd by the action of the dissolving liquor upon the iron, than any parcel of the external air, that might be suspected to have got in upon the inversion of the glass; especially since we allow'd time to those little particles of air, which were carried down with the nails, to fly up again: and, soon after, we perceiv'd the bubbles, produced by the action of the menstruum upon the metal, ascending in swarms to the former; and breaking into it, they soon exceedingly increas'd it, and, by degrees, depress'd the water, till, at length, the substance contain'd in these bubbles, possess'd the whole cavity of the vial, and most of its neck too; reaching much lower therein, than the surface of the ambient liquor, wherewith the open-mouth'd glass was, by this means, almost replenished. We suffer'd both the vial, and the open-mouth'd glass, to remain as they were, in a window, for three or four days and nights together; but often looking upon them, during that time, as well as at the expiration of it, the whole cavity of the glass bubble, and most of its neck, seem'd to be possess'd by air; since, by its spring, it was able, for so long, to hinder the expell'd liquor that surrounded it, from regaining its former place. And just before we took the vial out of the other glass, upon the application of a warm hand to the convex part of the bubble, the imprison'd substance readily dilated itself, like air, and broke thro' the liquor in several succeeding bubbles.

Having also, at another time, made the like experiment, with a small vial, and nails dissolv'd in *Aqua fortis*, we found it succeeded as the foregoing. And here we observ'd, that the steams newly generated, did not only possess almost the whole cavity of the glass, but several times, of themselves, broke away in large bubbles, thro' the ambient liquor into the open air: whence these experiments seem'd, manifestly, to prove, that, in general, air may be generated *de novo*.

And if, according to the mechanical hypothesis, the difference of bodies proceeds but from the various magnitudes, figures, motions, and textures of the small parts they consist of; there appears no reason why the minute parts of water, and other bodies, may not be so agitated, or connected, as to deserve the name of air.

24. We chose a glass-egg, half an inch in diameter at the top, and an inch at the bottom; and filling it with common water, to the height of about a foot and a half, so that the upper part remain'd empty, we enclosed it in the receiver; and, upon pumping, observed bubbles at the bottom and sides of the glass; and, increasing as the air was drawn away, they, from time to time, plentifully ascended to the top of the water, where they quickly broke: but the wideness of the glass allowing them free
pass-

passage thro' the water, they did not, as in the former experiments, seem to make it swell; and, upon the return of the external air, the water appear'd to have lost of its first extent, by the avolation of the air interspersed.

We put about two ounces of rain-water, carefully distill'd, into a round glass-bubble, with a very small neck, which was thereby fill'd half way to the top, and then convey'd it into the receiver; and, tho' we drew out more air than ordinary, there appear'd not the least intumescence of the water, nor any ascending bubbles. But suspecting that either the small quantity of the water, or the figure of the vessel, might affect the experiment, we took the former glass egg, and another, not much different from it, and fill'd the first, with distill'd rain-water, to the old mark, and, into the latter, put a long cylinder of solid glass, to straiten the cavity of the neck; and then pouring some distill'd water into that, also, till it reach'd near the top, they were both let down into the receiver: but here the air was so far exhausted, before there appear'd any bubble in either of the glasses, that the difference betwixt this, and common water, was very manifest. But, at length, when the air was almost quite drawn out, the bubbles began to disclose themselves, and to increase, as the pressure of the air, in the receiver, decreas'd. But, in the first egg, the bubbles were very small, and never able to swell the water above the mark; in the other, whose neck was straitned, great numbers of large ones, fasten'd themselves to the lower-end of the solid piece of glass, and gather'd to such a degree, between it and the sides of the neck, that the water swell'd a finger's breadth above the mark; tho', upon admitting the external air, it relaps'd to the former mark, or rather fell below it: upon which, all the bubbles presently disappear'd in the former vessel; whilst several remain'd fasten'd to the lower-part of the glass cylinder, and continued there for above an hour after, but contracted in their dimensions.

And having suffer'd these glasses to remain above twenty-four hours in the receiver, we, afterwards, repeated the experiment; but tho' the receiver was carefully exhausted, yet we scarce saw a bubble in either of the glasses; yet the water rose the breadth of a barley-corn in the neck of that glass wherein the solid cylinder had been placed; the liquor, in the other, not being sensibly swell'd. And, lastly, upon letting in the air, the water in the straitned neck, soon subsided to the mark, above which it had stretched.

25. We took a glass egg, with a long neck, of about $\frac{1}{4}$ inch in diameter, and pouring in fallad-oil till it reach'd above half-way to the top, we inclos'd it in the receiver, together with some common water in a similar vessel. The pump being set on work, there began to appear bubbles in the oil, much sooner than in the water; and afterwards they, also, ascended more plentifully in the former, than in the latter; and when the receiver was well exhausted, the bubbles rose almost as numerous as ever: so that none of the various liquors, we have try'd, seem'd to abound more with aerial particles, than this oil. And here 'twas remarkable, that, between the time

The air contain'd in oil.

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it was put into the receiver, and that before we could work the pump, it subsided about half an inch below the mark it at first reach'd to.

Oil of turpentine.

Common oil of turpentine, being put into a small glass bubble with a slender neck, so as to fill it about two inches from the top, presented us, upon evacuating the receiver, with numerous bubbles; most of which, rising from the bottom, expanded themselves exceedingly in their ascent, and made the liquor, in the neck, to swell so much by degrees, that at length, it several times ran over at the top: whereby we were hinder'd from discerning, upon letting in the air, how much the sinking of the oil, below the first mark, was due to the recess of the bubbles.

Oil of tartar.

Having fill'd a glass egg with a very strong solution of salt of tartar in fair water; tho' this, except quick-silver, is reckon'd the heaviest of liquors; we try'd, whether it would afford any bubbles; and putting it into the receiver, along with other liquors, we found that they yielded many bubbles, long before any appear'd in that: and upon prosecuting the experiment, it seem'd, of all the liquors whereof we made trial, this afforded the fewest, and smallest bubbles.

Spirit of vinegar, red wine, and milk.

Spirit of vinegar, examined after the same manner, exhibited a moderate quantity of bubbles. In red wine, we found nothing very remarkable: for tho' upon the exsuction of the air, the bubbles ascended in it, as it were in shoals, and shifted places, among themselves, in their ascent; yet the intumescence of the whole bulk of the liquor, was scarce sensible; the bubbles most commonly breaking very soon after their arrival at the top; where during their stay, they compos'd a kind of shallow froth, which, alone, appear'd higher, in the neck of the glass, than the wine, when it was first let down. Milk convey'd into our receiver, presented us with nothing considerable, except that the bubbles, not easily breaking at the top, and thrusting up one another, made the intumescence appear much greater, than that of common water.

Eggs.

We likewise convey'd hens eggs into the receiver, but after the exsuction of the air, took them out whole again.

Spirit of urine, and of wine.

We put some spirit of urine into a glass egg, fill'd another glass, to about two thirds of its neck, with rectified spirit of wine, and a third with common water, till it reach'd to the middle of the neck, and then pour'd to it of the same spirit of wine, till it reach'd about an inch higher. These glasses, having marks set on them, over against the tops of the contain'd liquors, were put into the receiver, and that beginning to be evacuated, bubbles began to appear in all three. The mixture of spirit of wine and water, disclosed numerous bubbles, especially towards the top, and the spirit of urine appear'd to swell near an inch and a half above the mark, and yielded plenty of bubbles, which made a kind of froth at the upper part of it; and above that, there appear'd eight or ten great bubbles, one higher than another, each of them constituting, as it were, a cylinder of about half an inch high, and as broad as the internal cavity of the neck; so that all the upper part of the neck seem'd to be divided into equal parts, by transverse

verse partitions, consisting of the coats of the bubbles, whose edges appear'd like so many rings, suspended one above another.

In the spirit of wine, there arose a great multitude of bubbles, all the while the experiment was in hand, which ascended with a great velocity, and being arriv'd at the top, made no stay there; yet, notwithstanding the great fluidity and volatility of the liquor, before they broke they lifted up the upper surface of it, and for a moment or two, form'd thereof, a thin film, which appear'd protuberant, above the rest of the superficies, like a small hemisphere: these also ascended in strait lines, whilst those produced at the lower part of the vessel, containing the mixture of the water and spirit of wine, ascended with a wavering motion, describing an indented line. Lastly, it was observable in the spirit of wine, as also in the oil of turpentine, lately mention'd, that not only the bubbles seem'd to rise from determinate places, at the bottom of the glasses; but that, in their ascent, they kept an almost equal distance from each other, and succeeded in a certain order, whence they seem'd part of small bracelets, consisting of equally small separate beads; the lower end of each bracelet being, as it were, fasten'd to a point, at the bottom of the glasses.

The air being sparingly let into the receiver, the great bubbles incumbent upon one another, in the glasses that contain'd the spirit of urine, were by regular degrees lessen'd, till at length, they wholly subsided. Notwithstanding the recess of so many bubbles as broke on the top of the spirit of urine, during all the time of the experiment, yet it scarcely appear'd, at all sunk below the mark. Nor did the mixture of spirit of wine and water considerably subside. But the spirit of wine, not only visibly expanded itself in the neck of the vessel, that contain'd it, whilst the bubbles broke at the top of it, almost as soon as they arrived there; but upon the re-admission of the external air, it retain'd its new expansion. And, tho' we let it alone, for near an hour together, yet when we took it out, it still swell'd between a quarter and half an inch above the mark. Repeating the experiment with fresh spirit of wine, it swell'd in the neck as formerly; and leaving it all night in the receiver, and allowing free access to the external air at the stop-cock, I found it, the next day, still expanded, as before; only it seem'd a little lower; which decrease, perhaps, proceeded from the avolation of some of the fugitive parts of the liquor. And for farther satisfaction, having taken out the glasses, and consider'd it in the open air at a window; I could not find, that there was any remaining bubble to occasion the continuance of this strange expansion.

26. We took two very small vials, of the size and shape express'd in Fig. 36. and into one of them, put so much of a certain ponderous mercurial mixture, that, the mouth being stop'd with a little soft wax, the glass would but just sink in water: this we let fall to the bottom of a wide-mouth'd crystal jar, fill'd with about half a pint of common water; and into the same vessel, we sunk the other glass, unstop'd, with as much water in it as was more than sufficient to make it subside. Both these sunk with their mouths downwards; the former being about three quarters full of air,

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and

*The gravity of
air expanded
under water.*

and the latter containing in it a bubble of air as big as half a pea ; then the wide-mouth'd glass was let down into the receiver, and the engine being work'd, the bubbles began to appear in the water, as in the former experiments ; but continuing long to ply the pump, that little glass, whose mouth was open'd, came to the top of the water ; being, as it were, buoy'd up by a great number of bubbles, that had fasten'd themselves to the sides of it ; and swimming thus, with the mouth downward, we could easily perceive, that the internal air above-mention'd, had much dilated itself, and thereby seem'd to have contributed to the emerging of the glass, which remain'd floating, notwithstanding the breaking, and vanishing of most of the contiguous bubbles. And persisting in pumping, we observ'd, that at each time the key was turn'd, the air, in the little glass, manifestly expanded itself, and thrust out the water ; generally retaining a very protuberant surface, where it was contiguous to the remaining water. And when, after several exsuctions of the air in the receiver, that in the vial so dilated itself, as to expel almost all the water, it turn'd up its mouth towards the surface of the water in the jar, and there deliver'd a large bubble, and then relaps'd into its former floating posture.

This experiment taught us, that it was a work of more time and labour, than we imagin'd, to exhaust our receiver as much as it may be exhausted ; for tho' before the smal viall emerged, we thought the receiver considerably emptied, because there seem'd to come but very little air at each exsuction, out of the cylinder ; yet, afterwards, the air included in the vial, manifestly dilated itself upon each stroke, so long, that for nine times it turn'd its mouth upwards, and discharg'd a bubble about the bigness of a pea. But that vial which had the weight in it, rose not at all : then leisurely letting in the air, that within the vial shrinking into a very narrow compass, the glass fell down to the bottom of the jar.

But being desirous to try once more, whether the little glass with the weight in it, might not also be rais'd ; after we had suffer'd the engine to remain clos'd, as it was, for five or six hours, the pump was again ply'd so vigorously, that not only about the upper-part of the jar, there appear'd a large number of small bubbles ; but afterwards, there came from the bottom of the jar, some as large as small peas, which, the pump being still kept going, follow'd one another, to the number of forty, coming from the stop'd vial ; whose mouth, it seems, had not been shut so closely, but the included air found a passage betwixt the wax and the glass. After this, the unstop'd glass began to float again ; the air shut up in it, being so dilated as to expel a large part of the water, but not so much as to break quite thro'. And, at length, the heavier of the two vials began to rise, but immediately subsided again : which seem'd owing to the air within it, whose bulk and spring being weakned by the recess of the forty bubbles, it was no longer able to break thro' the incumbent water ; but forming a bubble, at the mouth of the glass, buoy'd it up towards the top, and there getting away, left it to sink again ; till the pressure

sure of the air in the receiver, being farther taken off, the air, in the vial, was permitted to expand itself farther, and create another bubble, by which it was again, for a while, carry'd up. And tho', after having empty'd the receiver as far as we well could, we ceased from pumping; yet the vessel, continuing more stanch than usual, this ascent, and fall of the vial were repeated to the ninth time; the included air, by reason of the smallness of the vent at which it must pass out, being not able to get away, otherwise than by small degrees, and, consequently, in several such parcels as were able to constitute bubbles, each of them big enough to raise the vial, and keep it suspended, till the bubble flew off. Hence it may appear, that a body, lighter than an equal bulk of water, will float in that fluid, when the pressure of the atmosphere is, in very great measure, taken off from the liquor, and the body: tho' it were worth inquiring, what it is, that so plentifully concurs to fill the bubbles made, in our experiment, by the air so much expanded.

In this experiment, as in the former, the external air being let in, soon precipitated the floating vessel. And the water which, in the heavier vial, succeeded in the room of those forty, or more, great bubbles of air, which, at several times, got out of it; was of a very inconsiderable bulk.

27. It having been observ'd, that pendulums vibrate more slowly, and that their motion sooner ceases in a thicker, than in a thinner medium; A pendulum made to swing in vacuo. we thought proper to try if a pendulum would move faster, or vibrate longer, in our exhausted receiver, than out of it. We, therefore, took two round polished steel-pendulums, of equal bigness, each of them weighing twenty drams, bating so many grains. One of these we suspended in the cavity of the receiver, by a very slender string, about seven inches and a half in length, from the cover of the receiver whereto it was fasten'd. Then we made the pendulum swing, and, counting the returns of the other that hung in the open air, by a string of about the same length, we shorten'd and lengthen'd this, till it appear'd to keep the same pace with that in the receiver. Then, having carefully drawn away the air, we again made the pendulum in the receiver, vibrate; and, giving the other such a motion, as caus'd it to describe an arch, apparently equal to that of the included pendulum, we counted the recursions of both; and we reckon'd two and twenty vibrations of the included pendulum, whilst but twenty were observ'd of the other. And at another time, also, the former was found to have made twenty-one returns, whilst the other made but twenty. Yet this experiment seem'd to teach us little, except that the difference betwixt the motion of such a pendulum, in common air, and in a medium exceedingly rarify'd, is scarce sensible in vessels no bigger than our receiver; especially, since we could not suppose that to be altogether free from air. We observ'd, also, that, when the receiver was full of air, the included pendulum continu'd its recursions about fifteen minutes, before it left off swinging; and that, after the exsuction of the air, the vibration of the same pendulum appear'd not to last sensibly longer.

28. That

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*A watch and a
bell in the ex-
hausted receiver.*

28. That the air is the medium whereby sounds are convey'd to the ear, was a current opinion, till some pretended, that if a bell, with a steel clapper, be fasten'd to the inside of a tube, upon making the experiment *de vacuo*, with it, the bell remaining suspended in the deserted space, at the upper end of the tube; if a vigorous load-stone be apply'd on the outside of the glass, it will attract the clapper; which, upon the removal of the load-stone, falling back, will strike against the bell, and thereby produce a very audible sound: whence, several have concluded, not the air, but some more subtile body, to be the medium of sounds. But suspending a watch, freed from its case, in the cavity of our receiver, by a packthread; and then, closing up the vessel with melted plaister; we listen'd near the sides of it, and plainly heard the balance beat, and observ'd, that the noise seem'd to come directly in a streight line, from the watch to the ear. We found, also, a manifest difference in the noise, by holding our ears near the sides of the receiver, and near the cover of it; which seem'd to proceed from the difference between the glass, the cover, and the cement, thro' which the sound was propagated. But, upon working the pump, the sound grew gradually fainter; so that, when the receiver was emptied as much as usual, we could not, by applying our ears to the very sides of it, hear any noise from within; tho' we could easily perceive, that, by the motion of the hand which mark'd the seconds, and by that of the balance, the watch neither stood still, nor seem'd irregular. And, to satisfy ourselves farther, that it was the absence of the air about the watch, that hinder'd us from hearing it, we let in the external air at the stop-cock; and then, tho' we turn'd the key, and stopp'd the valve, yet we could plainly hear the noise made by the balance; tho' we held our ears, sometimes, at the distance of two feet from the outside of the receiver. And this experiment, being repeated, succeeded after the like manner: which seems to prove, that the air is, at least, the principal medium of sounds. And, by the way, it is very well worth noting, that, in a vessel so exactly closed as our receiver, so weak a pulsation as that of the balance of a watch, should propagate a motion to the ear, in a streight line, notwithstanding the interposition of glass, so thick as that of our receiver. We, afterwards, took a bell of about two inches in diameter at the bottom, which was supported, in the midst of the cavity of the receiver, by a bent stick, pressing with its two ends against the opposite parts of the inside of the vessel; which, being closed up, we observed the bell to sound more dead than in the open air. And yet, when we had empty'd the receiver, we could not discern any considerable change in the loudness of the sound: whereby it seem'd, that, tho' the air be the principal medium of sound; yet, either a more subtile matter may be, also, a medium of it; or else that an ambient body, that contains but few particles of air, is sufficient for that purpose. Whence, perhaps, in the above-mention'd experiment, made with the bell and the load-stone, there might, in the deserted part of the tube, remain air enough to produce a sound.

But

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But as, in making the experiment of firing gun-powder with a pistol in our evacuated receiver, the noise made by the flint, striking against the steel, was exceeding languid, in comparison of what it would have been in the open air: so, on several other occasions, it appear'd, that the sounds produced there, if they were not lost, seem'd to arrive at the ear very much weakned.

29. We have a liquor which, tho' most of its ingredients be metals, and all of them ponderous, is yet of such a nature, that, whilst the vial where-in it is kept, remains stopp'd, appears transparent, as, also, the upper part of the glass, to which the liquor reacheth not; but as soon as ever the stopple is taken out, and full access given to the external air; both the under part of the cork, and the liquor itself, presently send upwards, and diffuse a fume, as thick and white as if a quantity of alabaster-dust were thrown up into the air. And this smoking of the liquor lasts, till the vial be stopp'd again; and then the ascent of the fumes suddenly ceases.

*A fuming liquor
in the receiver.*

To a vial of this fuming liquor, we fasten'd a weight of lead; and, having ty'd to the stopple one end of a string, whilst the other was made fast to the cover of the receiver, the liquor was carefully closed up; and, the air being diligently pump'd out, we unstopp'd the vial. And tho, immediately upon drawing out the cork, there appear'd some white fumes, which seem'd to proceed from the air being imprison'd in the vial, and diffusing itself suddenly into the receiver; yet we afterwards observ'd, that the fumes did not mount, and disperse themselves, as they used to do in the open air; but, ascending to the lip of the vial, they stopp'd there, and ran down along the outside, and thence along an inclining piece of lead, on which the vial rested, like a little stream, that quitted not the vial, till it was come to the bottom of it, and there forsook it, like a stream of water of the same bigness. Then, letting in some of the external air, the stream run a-fresh, tho' not altogether so large: and, after the receiver was fill'd with air, I found, to my surprize, that, tho' the stream disappear'd, yet no white fumes arose, either from the cork, or out of the vial; no, not when the cover was removed from the receiver: tho', after a while, there ascended white fumes from the receiver. But, having immediately taken out the vial into the open air, it emitted white exhalations, as before; and having, presently after, unstopp'd it in an open window, we found both it, and the cork, immediately yielded a much more plentiful smoke; tho' it were now several years since this parcel of liquor was prepared.

30. Into one of our small receivers, we convey'd a piece of well-lighted match; and, letting it remain there, till it had fill'd the receiver with smoke, we took it out, and immediately closed the receiver again, that the smoke might not get away. Then staying, to let these fumes leisurely subside, we found, that, after some time, they settled themselves in the lower half of the receiver, in a darkish body; leaving the upper half transparent, and, as to sight, full only of clear air. And, inclining the vessel that contain'd this smoke, sometimes to one side, and sometimes to the other; we observ'd the fume to keep its surface almost horizontal, as water,

Smoke in vacuo.

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ter, or any other liquor, would have done in the like case. And if, by a quicker rocking of the engine, the smoke were more swiftly shaken, it would, like water, either vibrate from one side to the other of the glass; or else have its surface manifestly curl'd, like waves, and preserve itself, in an entire and distinct body, from the incumbent air; and, being permitted to rest a while, would soon recover its former smooth and level superficies. If, also, the key were turn'd, and the valve unstopp'd, so that there was a free passage open'd betwixt the external air, and the cavity of the receiver; then would some of this smoke fall down, as it were, in a stream, into the subjacent cylinder; and a proportionate quantity of the outward air, would, manifestly, ascend thro' it, into the incumbent air; after the same manner, as, when a vial, with a long neck, fill'd with red wine, being inverted into a glass of fair water, the water and wine, by degrees, mix, as it were, in little curl'd streams with each other; the one falling down, and the other ascending in its place. And if, when the superficies of our smoke lay smooth, and horizontal, a hot iron were held near the outside of the receiver; the adjacent part of the included fumes, being rarify'd by the heat, would readily ascend in a large pillar of smoke, to the very top of the receiver; yet, without seeming to lose its distinct surface, or to be confounded with the air, below which, upon the recess of the adventitious heat, it would again subside.

Since, then, there is so vast an inequality in the density and weight of liquors; we may consider the atmosphere as a peculiar kind of thin fluid, much lighter than spirit of wine. And as waves appear'd upon the surface of our agitated smoke; some such thing may, possibly, happen on the superficies of the atmosphere: as may be conjectur'd from those strange inequalities that often appear, especially when the air abounds with exhalations and vapours, upon the limb of the sun in its rising and setting. And if this phenomenon be owing to the refraction, which the sun's rays suffer in our air; 'tis easy to suppose the surface of the atmosphere to be often, as we said, exceedingly curl'd, or wav'd. And, certainly, it is surprizing to see how, thro' a good telescope, there will not only appear inequalities in the edge of the sun, which often seems to be indented; but those inequalities vanish in one place, and presently appear in another, and seem perfectly to move, like waves succeeding and destroying one another: only their motion frequently seems to be quickest; as if, in that vast sea, they were carry'd on by a current, or a tide. And this, also, appears to the eye, when a large, and well defined image of the sun, is, by the telescope, cast upon white paper.

31. It hath been thought strange, that, the perfectly polish'd surfaces of two flat pieces of marble being apply'd to each other, they should stick so fast together, that the lower may be rais'd, by taking hold of the upper. But, as this seems owing to the unequal pressure of the air upon the undermost stone, the lower superficies of that being freely expos'd thereto, and press'd upon by it, whilst the upper surface is defended therefrom; which, consequently, pressing the lower stone against the upper, hinders

it

The cohesion of
polish'd marbles
in vacuo.

it from falling; we therefore conjectur'd, that two marbles, being exactly ground to one another, and together suspended in our evacuated receiver, the lower stone would fall from the upper: but we could not procure marbles to be ground so true, as to sustain one another in the air for above a minute or two, which is a much shorter time, than is required to empty our receiver. We did, indeed, try to make our marbles stick close together, by moistning their surfaces with rectified spirit of wine; but to little purpose; for having convey'd into the receiver two black square marbles, the one with its side two inches and a third, and somewhat more than half an inch in thickness, the other of equal surface, but not above half so thick, fastned together by means of that spirit; and having suspended the thicker by a string from the cover, we found not, that the exsuction of the air would separate them, tho' a weight of four ounces were fasten'd to the lower, to facilitate its falling.

I would gladly have the experiment try'd with marbles, so well polish'd, as to need no liquor to make them cohere, and in a vessel, out of which, the air may be more perfectly drawn, than it was out of ours. But, tho' we will not determine, whether the spirit of wine contributed to the strong cohesion of these stones, otherwise than by keeping the subtlest parts of the air from getting in between them; yet it seem'd, that the reason, why the lower marble fell not, was, probably, because of the pressure of the air remaining in the receiver; which, as we formerly noted, being able to sustain a cylinder of water, of above a foot in height, may be supposed capable of keeping so broad a marble from descending. And, tho' this may seem a strange proof of the strength of the spring of the air, even when rarified; yet it will scarce appear incredible to him, who hath observ'd, how exceeding strong a cohesion may be made, betwixt broad bodies, only by immediate contact. A notable instance of this, is given us by the learned *Zucchi*, who tells us, that "a young fellow, bragging of his strength, some body set him to pull at the ring in the middle of a brass-plate, that lay upon a polish'd marble, whereto, it was exactly ground: this he thought a trivial matter; but after his utmost endeavour, found it impossible to separate them by direct pulling; which made him imagine they were fastned together, by means of some vehement strong glew, till he saw the plate, afterwards lifted by another, who, first split it along the marble".

33. Our receiver being exquisitely clos'd, and the air, in a good measure, drawn out, we remov'd it from the pump, and to the lower branch of the stop-cock, speedily apply'd a tapering valve of brass, made fit to go with its narrower end into the cavity of the branch, and to fill the orifice of that cavity with its broader part. And, that the air might not get in at the little intervals, between the convex surface of the stopple, and the internal edge of the branch, they were stop'd with diachylon. And, to the door of the valve, there was, at a button of brass, fasten'd a broad scale, wherein weights were to be put. This done, the key of the stop-cock was turn'd, and the external air beating like a forcible stream upon the valve to get in there,

An erect pressure exercised by the atmosphere. Fig. 37.

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there, it suddenly shut the valve, and kept it so close, that we had time to cast in several weights, one after another, into the scale, till at length, the weight overpowering the pressure of the atmosphere, drew down the valve by the strings that ty'd the scale to it, and gave liberty to the outward air to rush into the receiver. Tho', another time, when the valve had but little weight hanging to it, being, by accident, drawn down beneath its former place, it was, by the impetuous current of the external air, suddenly impell'd up into it again, and kept there. But, in the former experiment, tho' the receiver were not well exhausted; tho' it leak'd, whilst the rest of the experiment was in hand; and tho' the valve, whereon the cylinder of the atmosphere could press, were not above an inch and a half in diameter; yet the whole weight, supported by the air, amounted to about ten pounds, of sixteen ounces each: so that, had the experiment been made with favourable circumstances, the air endeavouring to press in, at the orifice of the stop-cock, would very probably have kept a much greater weight from falling out of it.

The pressure of
the atmosphere
computed.

33. But our pump, alone, may afford us a nobler instance of the force of the air; so that, by means of this part of our engine, we may conjecture at the strength of the atmosphere, computed as a weight. For, first, the sucker, brought to move easily up and down the cylinder, being impell'd to the top of it, and the receiver taken off from the pump, that the upper orifice of the cylinder remaining open, the air may freely succeed the sucker, and, therefore, readily yield to its motion downwards; and there being fasten'd to one of the iron teeth of the sucker, such a weight, as may just suffice to draw it to the bottom of the cylinder; we may hence find the weight necessary to draw down the sucker: and when the atmosphere makes the ordinary resistance against its descent, the sucker being again forc'd to the top of the cylinder, whose upper orifice must now be exactly clos'd; we may easily, by hanging a scale to the above-mention'd iron, that makes part of the sucker, cast in known weights, till the sucker be drawn down; then, to these weights in the scale, that of the scale itself being added, the sum will give us the weight of a column of air, equal in diameter to the sucker, or to the cavity of the cylinder, and, in length, to the height of the atmosphere.

According to this method, we attempted to measure the pressure of the atmosphere, but found it more difficult, than we expected, to perform it accurately; for tho', by the help of the handle, the sucker mov'd up and down with great facility; yet, when it came to be mov'd by a dead weight, we found, that the little inequalities, and, perhaps, the unequal pressure of the leather against the cavity of the cylinder, now and then stop'd the descent or ascent of the sucker; tho' a very little external help, would easily surmount that impediment. We found then, that a weight of twenty-eight pounds, being fasten'd to one of the teeth of the sucker, drew it down close, when the upper orifice of the cylinder was left open; but, by the help of oil, and water, and the frequent working of the sucker with the handle, its motion in the cylinder had been before purposely facilitated.

cilitated. Then the upper orifice of the cylinder was very carefully stop'd; the valve being likewise shut, with its stopple well oiled, after the sucker had been again impell'd up to the top of the cylinder. To the former weight we now added a hundred and twelve pounds, which forcing down the sucker, though but leisurely, we took off the 28 pound weight, and hung on, instead of it, fourteen pound; but found that, with the rest, unable to carry down the sucker. And to satisfy ourselves it was the resistance of the ambient air, that hinder'd the descent of so great a weight; after we had try'd, that upon unstopping the valve, and thereby opening an access to the external air, the sucker would be immediately drawn down, having forcibly depress'd the sucker, to the bottom of the cylinder, and then fasten'd weights to the iron, the pressure of the external air, finding little resistance, in the cavity of the cylinder, presently began to impel the sucker, with the weights that clogg'd it, towards the upper part of the cylinder, till some such accidental impediment, as we formerly mention'd, check'd its course; and when that was remov'd, it would continue its ascent to the top. And tho', possibly, there might remain some particles of air in the cylinder, after the sucker was drawn down; yet the pressure of a cylinder of the atmosphere, somewhat less than three inches in diameter, uncompress'd, not only sustain'd, but drove up a weight of a hundred and odd pounds: for, besides the weight of the whole sucker itself, which amounts to some pounds, the weights annex'd to it, made up a hundred and five pounds; yet all this falls short of the weight just said to be suspended, by the resistance of the air, in the cavity of the cylinder. This experiment was made in the winter, the weather neither frosty nor rainy, about the change of the moon; and at a place whose latitude is about 51 degrees and a half: for, perhaps, the force, or pressure of the air, may vary, according to the seasons of the year, the temperature of the weather, the elevation of the pole, or the phases of the moon; any of them seeming able to alter either the height, or consistence of the atmosphere. And therefore, it would not be amiss, if this experiment were try'd carefully, at several times and places, with variety of circumstances. It might, also, be try'd with cylinders of several diameters, exquisitely fitted with suckers; that we might know what proportion several pillars of the atmosphere, bear to the weight they are able to sustain, or lift up; and consequently, whether the increase, or decrease of the resistance of the ambient air, can be reduced to any regular proportion, to the diameter of the suckers. These, and other experiments, which may be made with this cylinder, might, most of them, be more exactly try'd by the *Torricellian* tube; if glass could be blown, and drawn perfectly cylindrical.

Here we may observe, that as many other phenomena of our engine, ^{The nature of suction.} so especially the two last experiments, seem to shew the nature, or cause of suction. It's true, indeed, in sucking, we commonly use some manifest endeavour, by a peculiar motion of our mouths, chests, &c. yet it appears not how the upper-part of the emptied cylinder, that remains at rest all the while, or any part of it, endeavours to draw the depress'd sucker,

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sucker, and the annex'd weights to it; tho' such as behold the ascent of the sucker, without considering the cause of it, readily conclude it to be rais'd by some secret thing, that powerfully sucks or attracts it. Whence it seems not absolutely necessary to suction, that there be in the body, which is said to suck, an endeavour, or motion in order thereto; but rather that suction may be reduced to trusion, and its effects ascribed to a pressure of the neighbouring air, upon the bodies contiguous to that which is said to attract them. To object here, that some particles of air, remaining in the emptied cylinder, attracted this weight, to obviate a vacuum, is to no purpose; unless it can clearly be made out, by what grappling instruments the external air could take hold of the sucker; how so little of it obtain'd the force to raise so great a weight; and why, upon letting a little more air into one of our evacuated vessels, the attraction is much weakned. For that still there remain'd in the exhausted cylinder many little empty spaces, may appear by the great violence wherewith the air rusheth in, if it be permitted to enter. In the next place, these experiments may teach us, what to judge of the vulgar axiom, That nature utterly abhors a vacuum; so that no human power is able to make one in the universe. For, if by a vacuum we understand a place perfectly free from all corporeal substance, it may be plausibly maintain'd, that there is no such thing in the world. But the generality of the plenists take not the word in so strict a sense. For when they alledge, that by sucking water thro' a long pipe, the liquor, contrary to its nature, ascends into the mouth, only to fill up that space, made by the dilatation of the breast and lungs, which would, otherwise, in part, be empty; and when they tell us, that the reason why in a gardener's watering-pot, conically shaped, and filled with water, none falls thro' the numerous holes at the bottom, whilst the orifice at the top, is clos'd; must be, that if, in case the water should descend, the air being unable to succeed it, there would be left a vacuum at the upper part of the vessel, they seem to mean by a vacuum, any space here below, that is not fill'd with a visible body, or, at least, with air, tho' it be not quite destitute of all bodies whatsoever.

And vacuity.

Taking then, a vacuum in this vulgar and obvious sense, the common opinion about it seems liable to several exceptions, whereof some of the chief are suggested by our engine.

It seems unintelligible, how hatred, or aversion, which is a passion of the soul, can either for a vacuum, or any other object, be supposed in water, or any inanimate body, which cannot be presum'd to know when a vacuum would ensue, if they did not attempt to prevent it; nor to act contrary to what is most conducive to their own particular preservation, for the good of the universe. The meaning, therefore, of this metaphorical expression seems to be, that by the wise author of nature, the universe, and the parts of it, are so contriv'd, that it is as hard to make a vacuum in it, as if they studiously conspired to prevent it.

But our experiments teach, that this supposed aversion of nature to a vacuum, is merely accidental, or consequent upon the weight, fluidity, or fluxi-

fluxility of the bodies here below ; and, perhaps, principally of the spring^P of the air, whose constant endeavour to expand every way, makes it either rush, or compel the interposed bodies, into all spaces where it finds no greater resistance than it can surmount ; and shew, that the power, exercised by nature, to avoid, or replenish a vacuum, is limited, and may be determined even to pounds and ounces *.

And the experiment we are now upon, affords us a notable proof of the unheeded strength of the pressure sustain'd by the free air, which we presume to be uncompress'd: for hence we see, that even in our climate, and without any other compression than what is natural, or ordinary, it bears so strongly upon contiguous bodies, that a cylinder of it, not exceeding three inches in diameter, is able to raise, and carry up a weight, amounting to between sixteen and seventeen hundred ounces. In more northern countries, the air may be much thicker, and able to support a greater weight ; since the *Hollanders*, who were forced to winter in *Nova Zembla*, found the air there so condens'd, that they could not make their clock go, by a very great addition to the weights that used to move it.

34. We took a dry bladder, strongly ty'd at the neck, and about half filled with air, and fastening it to one part of a very exact balance, we put a metalline counterpoise into the opposite scale ; and so the two weights being brought to an equilibrium, the balance was convey'd into the receiver, and suspended from the cover of it : when we observ'd, that presently after laying on the cover, the bladder appear'd to preponderate ; whereupon the scales being taken out, and reduced very near to an equilibrium, yet so, that a little advantage remain'd on that side to which the metalline weight belong'd ; they were again let down into the receiver, which was presently closed. Soon after this, before the pump was work'd, the bladder seem'd again a little to preponderate ; and the air in the glass beginning to be drawn out, the bladder expanded itself, and greatly raised the opposite weight, by drawing down the scale to which it was fasten'd, especially when the air had swell'd it to its full extent. This done, we, very leisurely let in the external air, and observ'd that, upon the flagging of the bladder, the scale whereto it was fasten'd, not only, by degrees, return'd to an equilibrium with the other ; but, at length, was a little outweigh'd by it ; tho' the bladder, after a while, began again to preponderate, and, by degrees, to sink lower for several hours : wherefore, leaving the vessel closed up all night, we, next morning, found the bladder fallen

Bodies of different specific gravities, lose their equilibrium in vacuum.

* "All the parts of space," says Sir Isaac Newton, "are not equally full ; for if they were, the specific gravity of the fluid, which would fill the region of the air, could not, by reason of the exceeding great density of its matter, give way to the specific gravity of quick-silver, gold, or any body how dense soever ; whence neither gold, nor any other body, could descend in the air. For no bodies can descend in a fluid, unless they be

"specifically heavier than it. But, if a quantity of matter may, by rarification, be diminish'd in a given space, why may it not diminish in infinitum ? If all the solid particles of bodies, are of the same density, that is, have their *vires inertia* as their magnitudes, and cannot be rarified, without leaving pores, there must be a vacuum". Newton. Princip. p. 368.

yet

Barometrical. yet lower: as if the very substance of it, had imbibed some of the moisture wherewith the air then abounded; as the strings of musical instruments, are known to swell so much in rainy weather, as to break. This conjecture is the more to be regarded, because having a little warm'd the bladder, we found it lighter than the opposite weight. And, without removing the scales, or the cover of the receiver, we again caus'd the air to be drawn out; the weather continuing very moist; but found not any manifest alteration in the balance.

But to make the experiment with a body, less apt to be alter'd by the temperature of the air, than a bladder, we brought the scales again to an equilibrium with two weights, the one lead, and the other cork. And, having exhausted the receiver, observ'd, that both upon the exsuction, and after the return of the air, the cork manifestly preponderated: and much more, a while after the air had been let in again, than whilst it was kept out. Wherefore, for the cork, we substituted a piece of charcoal, as less likely to imbibe any moisture from the air; but the event proved much the same; so that this experiment seems very liable to casualties.

*The ascent of
liquors in si-
phons, and fil-
tres, whence.*

35. The true cause of the ascent of liquors, in siphons and filtres, remaining unknown; we were desirous to try whether the pressure of the air might reasonably be supposed to have any considerable share in it. But, because we could not so far evacuate our receiver, but the remaining air would impel the water to a greater height than is usual in filtrations; instead of a list of cotton, or the like filtre, we made use of a siphon of glass, consisting of three pieces, two strait, and the third crooked, to join them together; whose junctures were carefully closed, that no air might find entrance at them: one of the legs of this siphon was somewhat longer than the other, and pervious at the bottom of it, only by a hole almost as slender as a hair, that the water might drop very gently out of it. The shorter leg of the siphon was quite open at the end, and of the same diameter with the rest of the pipe; that is, about a fourth of an inch. The whole siphon was design'd to be about a foot and a half long, that the remaining air, when the vessel was exhausted, might not impel the water to the top of it: then the siphon, being inverted, was fill'd with water, and the shorter leg let down, two or three inches, into a glass-vessel; whilst the upper part remain'd fasten'd to the inside of the cover of the receiver.

Fig. 38.

And, till a considerable quantity of the air had been evacuated, the water dropp'd freely out at the lower end of the lower leg of the siphon; as if the experiment had been made in the free air: but, afterwards, the bubbles began to appear in the water; and, ascending to the top of the siphon, run into one, which was gradually augmented by the rising of other bubbles, that, from time to time, broke into it, but much more by its own dilatation, which increased, proportionably, as the receiver was evacuated so that, at length, the water, in the shorter leg, was reduced, by the extraction of the ambient air, and the expansion of the great bubble, at the upper part of the siphon, to the height only of a foot; whence, the
course

course of the water, in the siphon, was interrupted, and that which remain'd in the longer leg of it, continued suspended there, without dropping any longer. But, upon turning the stop-cock, the external air got into the siphon, by the little hole at which the water formerly dropp'd out: and, traversing all the incumbent cylinder of water, in the form of bubbles, join'd itself with that air which before possessed the top of the siphon.

To prevent the inconveniences arising from these bubbles, two glass-pipes, like the former, were so placed, as to terminate together in the midst of the belly of a glass-vial, into whose neck they were cemented; and then both the vial, and the pipes, being filled with water, the siphon was placed with its shorter leg in the glass of water, as before; and the experiment being prosecuted after the same manner, much more air was now drawn out before the bubbles caused any disturbance; because there was room enough in the vial for them to stretch, without depressing the water below the ends of the pipes; and during this time, the water continued to drop out of the lower leg of the siphon. But, at length, the receiver being very much emptied, the water ceased to run thro' the siphon; the upper ends of the pipes beginning to appear above the remaining water in the vial, the dilated air wherein, seem'd likewise to press down the water in the pipes, and fill the upper part of them.

Hence, the experiment being interrupted, we let in the air again, which, according to its various proportions of pressure, to that of the air in the vial, and the pipes, exhibited a pleasing variety of phenomena. And upon the whole, there seem'd little cause to doubt, if the bubbles had not disturb'd the experiment, that the course of water, thro' siphons, would have appear'd to depend upon the pressure of the air.

An eminent mathematician lately told me, some *French* gentlemen had observ'd, that, if one end of a slender open pipe of glass, be dipp'd in water, the liquor will ascend to some height in the pipe, tho' held perpendicular to the plain of the water; and, soon after, brought me two or three small pipes of glass, which gave me the opportunity of trying it: tho' I had often before, in the long and slender tubes of some weather-glasses, made after a peculiar manner, taken notice of the like ascent of liquors; but, presuming it to be casual, I made little reflection upon it. But, after this trial, supposing that tho' the water, in these pipes, rose not above a quarter of an inch; yet, if the tubes were slender enough, it might ascend to a much greater height; I caus'd several of them to be dextrously drawn at the flame of a lamp, in one of which, that was almost incredibly slender, we found, the water ascended five inches, tho' the pipe were held erect: but, if it were inclined, the water would fill a greater part thereof. We also found, that, when the inside of the pipe was wetted before-hand, the water would rise much better than otherwise. And some of these slender pipes, being bent, like siphons, we immers'd the shorter leg of one in a glass of fair water; and found, that the water, rising to the top of the siphon, of itself, ran down the longer leg, and continued run-

Their ascent in capillary tubes.

PNEUMATIC. running, like an ordinary siphon. The cause of this ascent of the water, appears very difficult to discover*. We try'd, indeed, by conveying a very slender pipe, and a small vessel of water, into our engine, whether the exsuction of the ambient air would assist us herein; but, tho' we employ'd red wine, instead of water, yet we could scarce certainly perceive, thorough so much glass as was interposed betwixt our eyes and the liquor, what happen'd in a pipe so slender, that the redness of the wine was scarce visible in it. But, as far as we could discern, there happen'd no great alteration to the liquor; which seem'd the less strange, because the spring of that air, which might depress the water in the pipe, was equally debilitated with that which remain'd to press upon the surface of the water in the little glass. Wherefore, in favour of that conjecture, which ascribes this phenomenon to the greater pressure upon the water by the air, without the pipe, than by that within, it was shewn, that, in case the little glass-vessel of water were so closed, that the air might, by the mouth, be suck'd out of it, the water would immediately subside in the small pipe. Hence, we might infer, that it ascended before, by the pressure of the incumbent air; only it may be objected, that this, perhaps, would not happen, were the upper end of the pipe in a vacuum; as also, that, 'tis very probable, the water may subside, not because the pressure of the internal air is taken off by suction, but because the spring of the external air impels the water in its way to the cavity, deserted by the other air; and would as well impel the same water upwards, as make it subside, were it not for the accidental posture of the glasses. 'Twere here, likewise, proper to inquire, why the surface of water, in pipes, should be concave; and, on the contrary, that of quick-silver, convex; and why, if the end of a slender pipe be dipp'd in the latter, the surface of that fluid will be lower within the pipe, than without.

A parcel of air weigh'd. 36. We caused a glass-bubble to be blown at the flame of a lamp, about the size of a small hen-egg, and of an oval form; only, at one end, there

* This phenomenon, the suspension of water in capillary tubes, is, with great sagacity, accounted for by Dr. *Farin*; who proves it owing to the attraction of the upper periphery, or section, of the concave superficies of the tube; that is, a small surface, or annulus, whose base is that periphery, and height the distance, whereto the attractive power of the glass extends. For the gravity of the water that enters the orifice of the tube, upon its immersion, being immediately taken off, by the attraction of the annulus, wherewith its upper surface is in contact, the water must necessarily rise higher, by reason of the pressure of the stagnant fluid, and the attraction of the periphery immediately above that whereto the up-

per surface of the water is already contiguous. The consideration of this phenomenon, and the experiments made with relation to it, both in water and quick-silver; those made with the latter, proving exactly the reverse of the former; led the Doctor to clear the whole matter, by shewing, that the particles of water attract each other; that the particles of quick-silver attract each other; that water is attracted by glass; that quick-silver is attracted by glass; that the particles of water are more strongly attracted by glass, than by one another; and, lastly, that the particles of quick-silver are more strongly attracted by each other, than by glass. See all these proved in the *Philos. Transf.* N^o 355. p. 739.

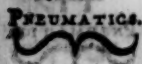
was

was drawn out an exceeding slender pipe, that the bubble might be seal'd up, with as little rarification, of the air included in the great cavity, as possible. This glass, being seal'd, was fasten'd to one of the scales of an exact balance; and, being counterpois'd with a weight of lead, was convey'd into the receiver, and clos'd up in it. The beam appearing to continue horizontal, the pump was set on work; and, after two or three exsuctions, the balance inclined to that side on which the bubble hung; which, as the air was farther drawn out, preponderated more manifestly: at length, the air being gradually let in again, the scales, by degrees, return'd to an equilibrium. Then we took them out, and casting into that scale, to which the lead belong'd, three fourths of a grain, we again placed them in the receiver; which, being clos'd and exhausted as before, as the air was drawn out, so the glass-bubble came nearer to an equilibrium with the other weight, till the beam stood horizontal: which, by another trial, we could not bring it to do, when one fourth of a grain more was added to the scale whereto the lead belong'd. Tho', without doubt, if we could have perfectly evacuated the receiver, the air included in the bubble, would have weigh'd above a grain; tho' it were somewhat rarify'd by the flame wherewith the bubble was seal'd. And, upon the return of the excluded air, the lead, and the weight cast into the same scale, did again very much preponderate.

We, likewise, convey'd into the receiver, the same bubble, open'd at the end of the slender pipe above-mention'd; but, having drawn out the air as usual, we found not, as before, the bubble to out-weigh the opposite lead: so that by the help of our engine, we can weigh the air, as we weigh other bodies, in its natural or ordinary consistence, without condensing it. Nay, having convey'd a lamb's bladder, half full of air, into the receiver, we observ'd, that tho' upon working the pump the imprison'd air expanded, till it seem'd ready to burst the bladder; yet this rarified air, manifestly depressed the scale whereto it was annexed.

And, having once caus'd the pump to be obstinately ply'd, in repeating the former experiment, the imprison'd air broke the containing glass-bubble, and threw the greatest part of it against the side of the receiver, whereby 'twas shatter'd into a multitude of pieces. Hence we may discern, of how close a texture glass is, since so very thin a film of it, as this bubble was, prov'd so impervious to the air, that it could not get away thro' the pores, but was forced to break the glass in pieces, to free itself; and this, notwithstanding the time, and advantage it had, to force thro' the pores. This I mention, that our experiments may receive no prejudice from one I happen'd to make long since; which might be drawn to countenance their opinion, who would persuade us that glass is pervious to air, properly so call'd: for, in distilling a certain substance, greatly abounding with subtle spirits, and a volatile salt, in a strong earthen vessel, of an unusual shape, to which was luted a large receiver of green glass; the fire was, by accident, so excessively increas'd, that we found the spirituous and saline corpuscles, thrown over so hot, and in such plenty, into the re-

Whether glass be
pervious to air.



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ceiver, as to render it all opaque, and likely to fly in pieces. We ventur'd, however, to approach it, and observ'd, on the outside thereof, at a great distance from the juncture, there was settled a round, whitish spot, or two, which, at first, we thought might be some stain upon the glass; but after finding it, in several qualities, like the oil and salt of the concrete distill'd, we suspected, that the most subtile, and fugitive parts of the impetuous steams, had penetrated the substance of the glass, and, by the cold of the ambient air, were condensed on the surface of it. And, indeed, upon examining the whole matter, a number of us unanimously concluded, that the subtile parts of the distill'd matter, being violently agitated by the excessive heat, had pass'd through the pores of the glass made wide by the same heat. But this having never happen'd, more than once, in any of the distillations we have either made, or seen, it is much more reasonable to suppose, that the perviousness of our receiver, to a body much more subtile than air, proceeded from the looser texture of that particular parcel of metal, the receiver was made of; for all glass is not equally compact, and solid; and from the prodigious heat, which, together with the vehement agitation of the subtile spirits, open'd the pores of the glass; than to imagine, that such a substance as air, should be able to permeate the body of glass, contrary to the testimony of a thousand chymical and mechanical experiments; and, of many made in our engine.

The penetrating power of air, compared with that of water.

And, the following experiment seems to teach, that tho' air, when sufficiently compress'd, may, perhaps, get entrance into smaller cavities, than water; yet, unless the air be forc'd in, it will not pass them, whilst they may admit of water. I took a glass siphon, the lower end of whose longest leg was drawn so slender, that the orifice, at which the water was to fall out, would hardly admit a very small pin. This siphon being inverted, we so order'd it, that a little bubble of air was intercepted in the slenderest part, betwixt the little orifice, just mention'd, and the incumbent water; whence the air, being not to be forced thro' so narrow a passage, by so light a cylinder of water, as rested upon it, hinder'd the farther efflux of the water, as long as we let it stay in that narrow place: but when, by blowing a little at the wider end of the siphon, that small parcel of air was forc'd out, with some water; the remaining water that before continued suspended, began freely to drop down again, as before. And a glass pipe, either in the form of a siphon, or otherwise, half an inch in diameter, but at one end so slender, as to terminate in an orifice almost as small as a horse-hair, be fill'd with water, it will drop down freely thorough the slender extremity. But if the pipe be inverted, the air will not easily get in at the small hole, thro' which the water pass'd. For, in the sharp end of the pipe, some inches of water will remain suspended; which, probably, would not happen, if the air could get in to succeed it; since, if the orifice were a little wider, the water would immediately subside. And tho' when the pipe is many inches long, a great part of the water will run down at the wider orifice; yet that seems

to

to happen for some other reason, than because the air succeeds it at the upper and narrow one; since all the slender part of the pipe, and, perhaps, some inches more, will continue full of water.

And, tho' we have formerly shewn, that the aerial corpuscles cannot pass thro' the pores of a lamb's bladder; * yet, particles of water will; as may easily be try'd, by very closely tying a little alkaline salt in a fine bladder, and dipping its lower end in water: for, if it be held there for a competent time, there will strain thro' the pores of the bladder, water enough to dissolve the salt into a liquor.

But, to return to our bubble; we endeavour'd to measure its capacity by filling it with water, to find how much water answer'd, in weight, to $\frac{1}{4}$ of a grain of air; but all the diligence we used to preserve so brittle a vessel, could not prevent its breaking, before we had gain'd our point.

But, there occurs a problem, upon occasion of the slow breaking of the glass bubble in our evacuated receiver. For, it might seem strange, since the air, as we have seen, expands itself by its own internal spring, twice as much as *Mersennus* was able to rarify it by a red-hot æolipile; that yet, the spring of the air was scarce able to break a very thin glass bubble; and utterly unable to break one somewhat thicker, within whose cavity it was imprison'd; whereas, air pen'd up, and agitated, is able to perform effects so much more considerable, that the learned jesuit *Cabeus* tells us, he saw a vast strong marble pillar quite broken off in the middle, by the heat proceeding from wood, which happen'd to be burnt just by it; which so rarified some air or spirituous matter shut up in the cavities of the marble, that it burst thro' the solid body of the stone by the force of expansion. But, probably, the reason why the included air did not break the seal'd bubbles, in our exhausted receiver, was, that being somewhat rarified by the flame employ'd to seal the glass, its spring upon the recess of the heat grew weaker than before. Yet, this will not, alone, serve the turn, because, much smaller glass bubbles, exactly clos'd, will by the included air be made to fly in pieces.

We took an æolipile of copper, weighing six ounces, five drams, and forty-eight grains; and being made hot, we remov'd it from the fire, and immediately stop'd it with hard wax, that no air might get in at its orifice. Then the æolipile, being suffer'd leisurely to cool, 'twas again weigh'd, together with the wax, and found to be six ounces, six drams, and thirty-nine grains. Lastly, the wax being perforated, without taking any of it out of the scale, the external air was suffer'd to rush in; and then the æo-

The proportion of the weight of air to that of water.

* *M. Homberg* is of opinion, that water enters such narrow pores of animal substances, as will not admit the air, only because it moistens and dissolves the glutinous matter of the fine fibres of the membranes, and also renders them more pliable and distractile; which are things,

that the air, for want of a wetting property, cannot do. As a proof of this doctrine, he fill'd a bladder with air, and compress'd it with a stone, and found no air to come out; but, placing the bladder, thus compress'd, in water, that air easily escaped. *Hist. de l'Acad. A. 1700. p. 17.*

M m m 2

lipile

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lipile, and wax, being again weigh'd, amounted to six ounces, six drams, and fifty grains. So that the æolipile, freed as far as our fire could free it, from its air, weigh'd less than when replenish'd with air, full eleven grains; that is, the air containable within the cavity of the æolipile, amounted to eleven grains, and somewhat more. And, by the way, if there be no mistake in the observation of *Mersennus*, it may seem strange that it should so much differ from two or three of ours; in none of which we could rarify the air in an æolipile, though made red-hot, almost all over, and immediately plung'd into cold water, to half that degree which he mentions, viz. seventy times its natural extent; unless the æolipile, he employ'd, was able to sustain a more vehement heat than ours*.

This way of weighing the air, by the help of an æolipile, seems somewhat more exact, than that which *Mersennus* used, because we weigh'd not the æolipile till it was cold; whereas he weigh'd it red-hot, whereby it is subject to lose of its weight in cooling: for, copper heated red-hot, throws off, in the cooling, little thin scales in such plenty, that, having purposely watch'd a copper æolipile, during its refrigeration, we have seen the place round about it, almost cover'd with them every way. Perhaps, too, the æolipile, in cooling, may not receive some little increase of weight, either from the vapid, or saline steams that float in the air. We employ'd, to weigh our æolipile, both when fill'd with air, and when replenish'd with water, a pair of scales that would turn with the fourth part of a grain. As to the proportion of weight betwixt air and water, some learned men have attempted to settle it, by ways so inaccurate, that they seem to have been much mistaken. *Ricciolus* having purposely endeavour'd to discover this proportion, by means of a thin bladder, estimates the weight of the air, to that of the water, as about 1 to 10,000; and, indeed, having once weigh'd a large bladder, full of air, and found it to contain 14 grains; the same bladder, afterwards fill'd with water, contain'd near 14 pounds; whence the proportion of air to water, seem'd, almost, as a grain to a pound, that is, as 1 to above 7600. On the other hand, *Galileo* makes the air to water, as 1 to 400. But our way of weighing the air by an æolipile, seems, by much, the more exact. And, according to our observations, the water it contain'd, amounting to 21 ounces and a half; and as much air as was requisite to fill it, weighing eleven grains; the proportion in gravity of air, to water of the same bulk, will be as 1 to 938. And tho' we could not fill the æolipile with water, very exactly; yet, as we neither could perfectly drive the air out of it

* It may be pretended, that 'tis not the air, but some vapour, or exhalation, contain'd in it, that here weighs upon the balance. To obviate this objection, *M. Muschenbroeck* contriv'd the following experiment. 'Tis a known thing in chymistry, that dry alkaline salts attract, and absorb the moisture of the air, and thereby run, *per deliquium*, as 'tis called. That philosopher, therefore, having exhausted a proper vessel of its air, fitted another vessel, wherein was lodg'd a large quantity of very dry salt of

tartar, reduced to fine powder, and made hot, to the neck of the former; so that the external air must pass slowly thro' this salt, before it could possibly get into the exhausted vessel; whereby the air that entered, was strained, and perfectly freed from any moisture that might have been lodged therein. The vessel being thus fill'd with pure air, and put into the scale, was found to weigh as much as when fill'd with unpurged air. *De Mater. subtil.* p. 7.

Physico-mechanical Experiments.

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by heat, we think the proportion may hold good: however, in a round sum we may say, water is near 1000 times heavier than air. And accordingly, having, at another time, put some water in the æolipile, before we set it on the fire, that the vapours of the rarified liquor might the better drive out the air, we found, upon trial carefully made, that when the æolipile was refrigerated, and the included vapours, by the cold, turn'd again into water; the air being let in, increas'd the weight of the æolipile, eleven grains, as before; tho' there were already in it, twelve drams, and 32 grains of water, which remain'd of that we had put into it, to drive out the air. *Mersennus*, indeed, tells us, that, by his account, air is in weight to water, as 1 to 1356; and adds, that we may, without any danger, suppose, the gravity of water to that of air of a like bulk, as 1300 to 1; and, consequently, that a quantity of air, to a quantity of water, equi-ponderant thereto, is as 1300 to 1. But why we should relinquish our own carefully repeated trials, I see not; yet I am unwilling to reject those of so accurate and useful a writer; and therefore suspect, that the difference in our observations, proceeds from the different consistence of the air at *London*, and at *Paris*: for, our air being more cold and moist than theirs, may be supposed, also, to be a fourth, or a fifth part heavier. Perhaps it may be of moment, too, that our observations were made in the midst of winter, whilst his might be made in some warmer part of the year.

It might be expected, that we should, from these and other observations, decide the controversy about the height of the atmosphere; but, tho' it seems easy to shew that many famous writers have been mistaken in assigning this height; yet, 'tis very difficult, precisely to define its extent.

The height of the atmosphere computed.

Now, we have, already made it appear, that at least about *London*, the proportion of specific gravity betwixt water and air, is, as 1000 to 1. And, to determine the difference in weight betwixt water and quick-silver; we took a glass-pipe, in the form of an inverted siphon, and pouring into it a quantity of quick-silver, we held it so, that the superficies of the liquor, both in the longer and shorter leg, lay in the horizontal line *E F*; then pouring water into the longer leg of the siphon, till that was almost fill'd, we observ'd the surface of the quick-silver in that leg to be, by the weight of the water, depress'd from *E*, to *B*, and in the shorter leg, to be as much impell'd upwards from *F*, to *C*. And having, before-hand, made marks, as well at the point *B*, as at the opposite point *D*, we measur'd both the distance *DC*, to have the height of the cylinder of quick-silver, which was rais'd above the point *D*, by the weight of the water, and the distance *BA*, which gave us the height of the cylinder of water. So that the distance *DC*, being $2\frac{1}{2}$ inches, and the height of the water $30\frac{1}{2}$ inches, the proportion appear'd to be as 121 to 1665, or as 1 to $\frac{1665}{121}$.

Fig. 40.

We also measured the proportion betwixt quick-silver and water by the help of a balance, which would turn with the hundredth part of a grain. But, because an over-sight is usually committed in weighing quick-silver, and water; especially, if the orifice of the containing vessel be wide; since the

13 $\frac{92}{121}$

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the surface of water in vessels, will be concave, but that of quick-silver considerably convex; to avoid this inconvenience, we made use of a glass-bubble, blown very thin, that it might not be too heavy for the balance, and terminating in a very slender neck, wherein the concavity or convexity of a liquor could not be considerable. This glass weighing $23\frac{1}{2}$ grains, we almost fill'd it with quick-silver, and fastning a mark over against the middle of the protuberant superficies, we found that the quick-silver alone, weigh'd $299\frac{7}{8}$ grains; then the quick-silver being pour'd out, and the same glass fill'd to the same height with common water, we found it to weigh $21\frac{1}{2}$ grains: whereby, it appear'd, that the weight of water to quick-silver, is, as 1 to $13\frac{1}{2}$; tho' the illustrious *Verulam*, merely for want of exact instruments, makes the proportion betwixt them greater than of 1 to 17. And, by the way, since quick-silver, and well rectified spirit of wine are accounted, one the heaviest, and the other the lightest of fluids; with the same glass, and scales, we found, the difference betwixt them, to be, as 1 and $16\frac{2}{3}$; whence, the difference betwixt highly rectified spirit of wine, and common water, is, as betwixt 1 and $1\frac{1}{3}$. But to avoid fractions, let us suppose quick-silver is fourteen times as heavy as water. We have then given us, the proportion of air to water, and of water to quick-silver; from whence, it is easy to find the proportion betwixt air and quick-silver; if we suppose the atmosphere to be uniformly of such a consistence as here below. For, since our engine hath sufficiently manifested, that 'tis the equilibrium with the external air, that, in the *Torricellian* experiment, sustains the quick-silver; and, since by our accurate experiment, formerly mention'd, a cylinder of mercury, able to balance a cylinder of the whole atmosphere, amounted to about thirty inches; and, since, consequently, we may assume the proportion of quick-silver to air, to be as 14000 to 1; it will follow, that a cylinder of air, able to maintain an equilibrium with a mercurial cylinder of two feet and a half in height, must amount to 35000 *English* feet, and consequently to seven miles.

But we cannot safely conclude, that the air is every where of the same consistence we found it, near the surface of the earth; not only because, as *Seneca* says, "the air is more gross, the nearer it comes to the earth, as the feces fall to the bottom in water"; but because the springy texture of the aerial corpuscles makes them capable of a very great compressure, which the weight of the incumbent part of the atmosphere exerts upon the undermost, near the surface of the earth. And as we have seen, that air, much rarify'd without heat, may easily admit a farther rarification with it; and that, even without being expanded thereby, it is capable of being rarify'd to above a hundred and fifty times the extent it usually possesseth here below; perhaps the atmosphere may rise to the height of some hundred miles: nay, exhalations may ascend much higher, if there was no mistake in that strange observation made at *Toulouse*, in a clear night in *August*, by the diligent *Emanuel Magnan*; who, as *Ricciolus* tells us, "saw, from eleven a-clock at night, till twelve, while the moon was under the horizon,"

"horizon, a little lucid cloud, near the meridian, and almost in the zenith, which could be illumin'd by nothing but the sun; and, therefore, must have been higher than the whole shadow of the earth. And," says *Ricciolus*, "the like phenomenon was observ'd by the great mathematician *Riccius*."

Various observations, made at the feet, tops, and interjacent parts of high mountains, might, perhaps, assist us to make an estimate, in what proportion the higher air is thicker than the lower; and to guess at the different consistence, as to laxity and compactness, of the air, at several distances from us. And, if the difficulties about the refractions of the celestial luminaries were satisfactorily determined; that might, also, conduce to assign proper limits to the atmosphere. But, at present, we dare not pronounce any thing, peremptorily, concerning the height of it.

37. We have often observ'd, that, when the sucker of our pump was drawn down, immediately upon turning the key, there appear'd a kind of light in the receiver, almost like a faint flash of lightning, in the day-time; and almost as suddenly did it appear, and vanish. When we first took notice of this phenomenon, the day was clear, the hour about ten in the morning, and the only window in the room faced the north; and we found that, by interposing any opaque body between the receiver, and the window, tho' the rest of the room were sufficiently enlighten'd, yet the flashes did not appear as before. As soon as night was come, we made the room very dark, and plying the pump, as in the morning, could not find, upon turning the key, the least glimmering of light. Whence we inferr'd, that the flash, appearing in the receiver, did not proceed from any new light, generated there; but from some reflections of the light of the sun, or other luminous bodies, placed without: tho', whence the reflection should happen, was hard to say.

Odd phenomena
of light produced
in the receiver of
the air-pump.

Wherefore, the next morning we went about to repeat the experiment; but tho' we could, as well as formerly, exhaust the receiver; tho' the place wherein we made the trial, was the very same; and tho' other circumstances corresponded; yet we could not discover the least appearance of light all that day, no more than on several others: nor can we, to this very time, be sure, a day before-hand, that these flashes will appear in our great receiver. Nay, having once found the engine disposed to exhibit this phenomenon, we sent notice of it to *Dr. Wallis*, who was then very near at hand, and made haste to satisfy his curiosity; yet, by that time he arrived, the appearance was ceased: and having long, in vain, endeavour'd to exhibit it again, we were, after all, unexpectedly presented with a few flashes.

And this contingency, whereto our experiment is liable, being such, that, in all constitutions of the weather, times of the day, &c. it will sometimes answer, and sometimes disappoint our expectations; we are much discouraged from framing an hypothesis to solve it; tho' it might be attempted from considering the following phenomena. (1.) The appearance may as well be exhibited by candle-light, as by day-light, and in what-

whatever position the candle be held to the receiver, provided the rays of light be not hinder'd from falling upon the vessel. (2.) The flash appears immediately upon turning the key, to let the air out of the receiver into the empty'd cylinder; so that, I remember not, that the flash appear'd, when at any time, in our great receiver, the stop-cock was open'd, before the cylinder was exhausted. (3.) When, instead of the great receiver, we made use of a small glass, not containing above a pound and a half of water; the phenomenon might be exhibited, tho' the stop-cock remain'd open, provided the sucker was drawn nimbly down. (4.) When we began to empty the vessel, the appearances of light were much more conspicuous, than towards the latter end, when little air, at a time, could pass out of the receiver. (5.) When the sucker had lately been well oil'd, and, instead of the great receiver, the smaller vessel, above-mention'd, was emptied; upon opening the stop-cock, as the air descended out of the glass into the emptied cylinder, there ascended out of the cylinder into the vessel, a certain steam, which seem'd to consist of very little bubbles, or other minute corpuscles, thrown up from the oil, rarify'd by the attrition it suffer'd in the cylinder. For, at the same time that these steams ascended into the glass, some of the same kind manifestly issued out, like a little pillar of smoke, at the orifice of the valve, when that was occasionally open'd. And these steams, frequently presenting themselves to our view, we found, by exposing the glass to a clear light, that they play'd up and down in it; and, by their whitishness, in some measure resembled the appearance of light. (6.) For, when the flash was great, the receiver, at the very instant, lost of its transparency, by appearing full of some kind of whitish substance; and, for a short time after, the sides of the glass continu'd opake, and seem'd to be darken'd, as if some whitish steam adhered to the inside of it.

But he who would fairly account for the phenomenon, whereof these are not all the circumstances, must shew from whence the apparent whiteness proceeds; and why that whiteness sometimes appears, and sometimes not. Now, had our phenomenon been constant, and uniform, we should suspect it to have been produced after the following manner; for tho' what we saw in our receiver, seem'd to be a kind of light, yet it was, indeed, but a whiteness, which render'd the inside of the glass opake.

Now our common air abounds with particles, able to reflect the rays of light, as appears from that vulgar observation, the motes in the air, when the sun-beams shooting into any shady place, discover them, tho' otherwise, the eye cannot distinguish them from the air. And, I particularly remember, that being at some distance from London, at a time when numerous bon-fires happen'd to be made there; tho' we could not see the fires themselves, yet we could plainly perceive the air all enlightned near the city: which argued, that the rays, shot upwards from the fires, met, in the air, with corpuscles opake enough to reflect them to our eyes.

White may be produc'd, when the continuity of a transparent body happens to be interrupted by a great number of surfaces, which, like so many little looking-glasses, confus'dly represent a multitude of small and seemingly contiguous images of the lucid body. For, water, or the whites of eggs, beaten to a froth, lose their transparency, and appear white. And, having, out of one of our small receivers, carefully drawn out the air, and left a very little hole, by which the water was to get in, we observ'd that the neck, being held under water, and the little hole open'd, the water that rush'd in, was so broken, and acquired such a multitude of new surfaces, that the receiver seem'd to be full rather of milk, than water. And farther, by heating a lump of crystal, and quenching it in fair water, it will be discontinued by such a multitude of cracks, which create new surfaces within it, that tho' it will not fall asunder, yet it loses its transparency, and appears white.

Hence we might imagine, that upon the rushing of the air out of the receiver, into the empty'd cylinder, the air in the receiver, being suddenly, and vehemently expanded the texture of it was as suddenly alter'd; and the parts made so to shift places, and, perhaps, some of them, to change postures, as during their new and vehement motion, and their varied situation, to disturb the usual continuity, and, thereby, the transparency of the air; which ceasing to be a transparent body, must easily degenerate into white.

Several things there are which make this conjecture seem the more probable; as, first, the whiteness always appear'd greater, whilst there was much air in the receiver, than when the air was in great part drawn out. Secondly, having exhausted the receiver, and applied to the hole in the stop-cock, a large bubble of clear glass; so that we could, at pleasure, let the air pass out, at the small glass, into the great one, and easily fill the small one with air again; we observ'd, that upon opening the communication betwixt the two glasses, the air, in the smaller, finding so much room in the greater, to receive it, flew out with such force, that the small vial seem'd to be full of milk: and this experiment we repeated several times. And, thirdly, having provided a small receiver, with its upper orifice so narrow, that I could stop with my thumb, I observ'd, that when, upon the exsuction of the air, the capacity of the glass appear'd white; if, by a sudden removal of my thumb, I let in the outward air, that whiteness would immediately vanish. It may, indeed, be objected, that when water turns from transparent to white, the air intervenes, which converts it into bubbles. To this I reply, there are two very volatile liquors, which being gently put together, are as clear as rock-water, and yet will instantly, without the help of air to turn them into bubbles, so alter the disposition of their insensible parts, as to become a white consistent body. And this happens not as in the precipitation of benjamin, and some other resinous substances; which being dissolv'd in spirit of wine, may, by the affusion of fair water, be turn'd into a milky substance: for this whiteness belongs not to the whole liquor, but to the corpuscles

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cles of the dissolved gum, which, after a while subsiding, leave the liquor transparent, themselves only remaining white. But, in our case, 'tis from the varied texture of the whole transparent fluid; and not from any particular part, that this whiteness results: for the body is white throughout, and will long continue so; and yet may, in process of time, without any addition, be totally reduced into a transparent body, as before.

Another conjecture, we grounded upon this observation: having convey'd some smoke into our receiver, placed against a window, we observ'd, that, upon the exsuction of the air, the corpuscles floating in it, manifestly enough made the receiver seem more opaque, at the very instant the air rushed out. For, considering that the whiteness, whose cause we enquire after, did but sometimes appear, it seem'd not impossible, that, at such times, the air in the receiver, might abound with particles capable of reflecting the light, in the manner requisite to exhibit a white colour, by being put into a certain unusual motion; as the new motion of their former fumes, made the inside of the receiver appear darker than before; and as our smoking liquor, formerly mention'd, whose parts, tho' they seem'd transparent, whilst they compos'd a fluid; yet when the same corpuscles, upon unstopping the glass, were put into a new motion, and dispos'd after a new manner, they render'd that part of the air opaque, wherein they mov'd, and exhibited a greater whiteness than sometimes appears in our receiver.

But as to the reason why our phenomenon appears not constantly, I remember not that we ever made the experiment in a small vessel, without finding the expected whiteness. But it remains to be explain'd, why in our great receiver, the phenomenon should sometimes be seen, and often not. All I have to say on this head is, that the air about us, and much more that within the receiver, may be much alter'd by such cases, as few are aware of. The learned *Josephus Acosta* tells us, that "in *America* there are winds which naturally trouble the water of the sea, making it green and black, and others as clear as crystal." And, tho' we convey'd into the receiver, the scales and the pendulums, formerly mention'd, clear and bright; yet, after the vessel had been emptied, and the air let in again, the lustre of both appear'd tarnish'd by a beginning rust. And, lastly, having, with pure spirit of wine, drawn a transparent tincture out of a certain conerete, commonly reckon'd among minerals, we put it into a crystal-vial, carefully stop'd it, and lock'd it up in a press; and this liquor, being a chymical rarity, and of a pleasing golden colour, we had often occasion to view it; and took notice that once it seem'd to be very thick: whereupon, we imagin'd it possible, that some of the mineral corpuscles were then precipitating. But finding, after some days, that tho' no precipitation had been made, and that the liquor, retaining its former vivid colour, was grown clear again, as before; we lock'd it up again in the same press, and resolv'd to observe whether the like changes would again appear in our tincture; and, in case they should, whether they might be ascribed to the alterations of the weather. But tho' during the greatest part of a winter,

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winter, and a spring, we observ'd the liquor would often grow turbid; and, after a while, clear again; yet we could not find, that it depended upon any manifest changes in the air; which would be often dark and cloudy, when the tincture was clear and transparent; as, in clear weather, the liquor would, sometimes, appear troubled, and more opake.

38. Into a glass vial, open at the top, we put a mixture of snow, and common salt; and, in the midst of this mixture, set a cylindrical glass, closely stopp'd at the lower-end, and open at the upper, where we fill'd it with common water; then let them all down into the receiver; and the pump being set on work, the snow began to melt faster than we expected. However, by that time the receiver had been considerably exhausted, which it was in less than a quarter of an hour, we perceiv'd the water, near the bottom of the glass cylinder, to freeze; and the ice, by a little longer stay, seem'd to increase, and to rise somewhat higher than the surrounding surface of the liquor whereinto, almost all the snow and salt were dissolved. The glass being taken out, it appear'd that the ice was as thick as the inside of the vessel it fill'd; tho', into that, I could put my thumb. The upper surface of the ice was very concave, and, held against the light, appear'd not destitute of bubbles; tho' they were fewer than if the water had been frozen in the open air. The like experiment we made, also, in one of our small receivers, with like success.

But, whence proceeds that strange force, we may sometimes observe in frozen water, to break the bodies that imprison it, tho' hard and solid? A stone-cutter, lately complain'd to me, that, sometimes, thro' the negligence of his servants, the rain being suffer'd to soak into marble, the violent frosts coming on, would burst the stones. And, another tradesman complain'd, that, even implements made of bell-metal, being carelessly expos'd to the wet, have been broken and spoil'd by the water, which, having enter'd at the little cavities of the metal, was there, afterwards, froze, and expanded into ice. And *Cabeus* tells us, that he saw a huge vessel of exceeding hard marble split asunder, by congeal'd water. I know it will be said, to solve this problem, that congelation doth not reduce water into less space, than it before possess'd, but, rather makes it take up more. But, tho' we grant, that water swells in freezing; yet how cold, which, in weather-glasses, manifestly condenseth air, should expand either the water or the intercepted air, so forcibly as to perform what we have here related, remains to be discover'd.

39. We took an oval glass, clear, and pretty strong, with a short neck at the obtuser end, thro' which we thrust, almost to the bottom, a pipe of glass, and closely cemented it to the neck: the upper part of the pipe was drawn, in some parts, more slender than a crow's quill, that the changes of the air in the glass-egg, might be the more conspicuous; then we convey'd into the glass, five or six spoonfuls of water, part of which, by blowing air into the egg, was rais'd into the slender part of the pipe; so that the water was interpos'd between the external air, and that included in the egg. This weather-glass, was so placed, and clos'd

Pneumatic.

Water made to freeze in vacuo.

A water-thermo- meter in vacuo.

Fig. 41.

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up in the cavity of a small receiver, that only the slender part of the pipe, to the height of four or five inches, passing thro' a hole in the cover, remain'd expos'd to the open air.

In evacuating the receiver, the water, in the pipe, descended about a quarter of an inch; and this upon two or three repeated trials; which seem'd to argue, that there was no heat produced in the receiver, upon the exsuction of the air: for even a little heat would, probably, have been discover'd by that weather-glass; since, by the bare application of my hand to the outside of the receiver, the warmth, after some time, having been propagated thro' both the glasses, and the interval betwixt them, to the imprison'd air, so rarify'd it, that, by pressing upon the subjacent water, it impell'd that in the pipe much higher than it had fallen downwards, upon the exsuction of the air.

Yet we do not hence conclude, that in the cavity of the receiver the cold was greater after the extraction of the air, than before.

If it be demanded, what then could cause the water to subside; we answer, that, probably, it was the stretching of the glass-egg, which, upon the exsuction of the ambient air, was unable to resist, as formerly, the pressure of the included air, and of the atmosphere, which, by the intervention of the water, press'd upon its concave surface. This seems probable, as well from the experiment about breaking a glass, by the force of the atmosphere, as because, when by drawing the air out of the receiver, the water, in the pipe, was subsided, upon the re-admission of the external air, to press against the convex surface of the egg, the water was presently re-impell'd to its former height: for, if a glass-egg be blown exceeding thin, and afterwards broken, you may, by degrees, considerably bend some narrow parts of it; and upon the removal of what kept it bent, it will readily recover its former state. From our experiment, then, it appears either that there succeeds no body in the room of the air drawn out of the receiver; or, that every substance is not subtil enough, readily to pass the pores of glass, tho' always sufficiently agitated to produce heat, wherever it is found in plenty. So that if we admit no vacuum, this experiment requires us to allow a great disparity, either as to bulk, or agitation, or both, betwixt some parts of the ætherial substance, and those which, here below, produce heat and fire.

We try'd, also, what operation the extraction of the air would have upon camphire; which consists of such volatile parts, that they will exhale without any greater agitation, than that of the open air. But we found not, that even this loose body, was sensibly alter'd thereby.

Insects in vacuo. 40. We convey'd a large flesh-fly into a small receiver; and, at another time, shut into a great receiver, a humming-bee, that appear'd strong and lively; we also procur'd a white butter-fly, and inclos'd it in a small receiver; where, though at first, he flutter'd about, yet, presently, upon the exsuction of the air, he fell down, as in a swoon; retaining no other motion, than some little trembling of the wings. The fly, after some exsuctions of the air, drop'd down from the side of the glass, whereon she

was

was walking: but, that the experiment of the bee might be more instructive, we convey'd in with her a bundle of flowers, which remain'd suspended by a string, near the upper-part of the receiver; and having provoked the bee, we excited her to fly up and down the vessel, till, at length, she lighted upon the flowers, when we presently began to draw out the air, and observ'd, that tho', for some time, she seem'd to take no notice of it, yet, within a while after, she fell down from the flowers, without making any use of her wings.

41. To satisfy ourselves, in some measure, why respiration is so necessary to the animals, that nature hath furnish'd with lungs, we took a lark, one of whose wings had been broken by a shot; but, notwithstanding this hurt, the bird was very lively; and put her into the receiver, wherein she, several times, sprung up to a considerable height. The vessel being carefully clos'd, the pump was diligently ply'd, and the bird, for a while, appear'd lively enough; but, upon a greater exsuction of the air, she began manifestly to droop, and appear sick; and, very soon after, was taken with as violent, and irregular convulsions, as are observ'd in poultry, when their heads are wrung off, and died; (tho' when these convulsions appear'd, we let in the air,) with her breast upward, her head downward, and her neck awry; and this within ten minutes, part of which time had been employ'd in cementing the cover to the receiver. Soon after we put a lively hen-sparrow, which was not at all hurt, into the receiver; and prosecuting the experiment, as with the former, she appear'd to be dead within seven minutes; one of which was employ'd in cementing on the cover: but, upon suddenly turning the key, the fresh air, flowing in, began slowly to revive her; so that, after some pantings, she open'd her eyes, and regain'd her feet, and, in about a quarter of an hour after, attempted to escape at the top of the glass, which had been unstop'd to let in the air upon her: but the receiver being clos'd the second time, she died, violently convuls'd, within five minutes from the first stroke of the pump.

Birds and mice
in the exhausted
receiver.

Then we put in a mouse, newly caught, and, whilst he was leaping up very high in the receiver, we fasten'd the cover to it; expecting, that an animal, used to live with very little fresh air, would endure the want of it better than the birds; but tho', for a while after the pump was set on work, he continu'd leaping up, as before; yet 'twas not long e'er he began to appear sick, giddy, and to stagger; after which, he fell down as dead, but without such violent convulsions as the birds had: when, hastily letting in some fresh air upon him, he recover'd his senses, and his feet, but seem'd to continue weak and sick; at length, growing able to skip, as formerly, the pump was ply'd again, for eight minutes; about the middle of which space, a very little air, by mischance, got in at the stop-cock; and, about two minutes after that, the mouse, several times, leap'd up lively; tho', in two minutes more, he fell down quite dead; yet with convulsions far milder than those wherewith the birds expired. This alacrity, so little before his death, and his not dying sooner than

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than at the end of the eighth minute, seem'd owing to the air that pass'd into the receiver: for, the first time, the convulsions seiz'd him, in six minutes after the pump began to be work'd. These experiments seem'd the more strange, because, during a great part of those few minutes, the engine could but considerably rarify the air, and that too by degrees; and, at the end thereof, there remain'd in the receiver, a large quantity: for, as we formerly said, we could not draw down water in a tube, with-in much less than a foot of the bottom. And, by the exsuction of the air, and interspersed vacuities, there was left in the receiver, a space some hundreds of times exceeding the magnitude of the animal, to receive the fuliginous steams, from which, expiration discharges the lungs, and which, in the other cases, may be suspected, for want of room to stifle those animals that are closely pent up in too narrow receptacles.

Having caused these three creatures to be open'd, I could discover little of what we sought for, and might, possibly, have found in larger animals: for tho' the lungs of the birds appear'd very red, and, as it were, inflamed; yet that colour is usual in the lungs of such winged animals: but in almost all the destructive experiments, made in our engine, the animals appear'd to die with violent convulsive motions. From whence, whether physicians can deduce any thing towards the discovery of the nature of convulsive distempers, I leave to them to consider.

And, to obviate objections, and remove scruples, about the fuliginous steams of pent up animals, which are supposed to kill them; we shut up another mouse, as close as possible, in the receiver, where it liv'd about three quarters of an hour; and might, probably, have done so, much longer, had not a person of quality desired to see whether the mouse could be kill'd by the exsuction of the ambient air. Upon this, we open'd, for a while, an intercourse betwixt the air in the receiver, and that without, whereby the mouse might be refreshed, tho' without uncementing the cover at the top; to avoid the objection that, perhaps, the vessel was more closely stopp'd for the exsuction of the air than before.

The event was, that, after the mouse had liv'd ten minutes, the pump being a little out of order, he died with convulsive motions; wherein he made two or three bounds into the air, before he fell down dead.

I, also, caused a mouse, that was very hungry, to be shut up all night into a well-closed receiver, with a bed of paper for him to rest on; and caus'd the engine to be placed by the fire-side, to keep him from being destroy'd by the immoderate cold of a frosty night; and, the next morning, I found he had devour'd a large part of the cheese that had been put up with him. And, having thus kept him alive full twelve hours, we, by sucking out part of the air, brought him to droop, and to appear swell'd; but, by letting it in again, we soon reduced him to his former liveliness.

It may be here expected, I should attempt to clear the nature of respiration; but I pretend to go no farther in it, than our engine leads me.

'Tis alledged by those who would have the lungs rather passive than active, in respiration, that as the lungs, being destitute of muscles and fibres,

the nature of
respiration con-
sider'd.

fibres, are unfit to dilate themselves; so, without the motion of the thorax, they would not be fill'd with air: since, as Dr. *Highmore* hath well observ'd, if a live dog have a great wound made in his chest, the lobes of the lungs, on that side of the mediastinum, will collapse, and lie still; whilst the thorax, and the lobes on the other side of the mediastinum, continue their former motion. And if, at once, the muscles of the chest be on both sides dissected; upon the ingress of the air, the whole lungs, tho' untouch'd, will remain without motion, at least as to any expansion, or contraction of their substance.

And *Bartholine* affirms, that if the diaphragm be wounded, the lungs will fall together, and respiration cease; which appears to be true, provided the wound be large. And, indeed, the diaphragm seems the principal instrument of ordinary respiration; tho' the intercostal muscles, and, perhaps, some others, may be allow'd eminently to concur in extraordinary cases. But it is not yet decided, what conveys air into the lungs; for 'tis demanded, what should bring the air into the lungs, if they do not attract it? To this question, some of the best modern philosophers answer, that, by the dilatation of the chest, the contiguous air is thrust away; and that, pressing upon the next air to it, and so onwards, the propulsion is continu'd, till the air be drawn into the lungs, and so dilates them. It is, again, objected by *Bartholine*, that, according to this doctrine, a man could not fetch his breath from a great vessel, with a slender neck, full of air; because, when his mouth covers the orifice of the neck, the dilatation of his thorax could not propel the air of the vessel into his lungs; being separated by the inclosing vessel, from the ambient air: and yet, it will be said, experience witnesseth, that out of such a vessel a man may suck air. But this difficulty our engine can easily solve; since many of the preceding experiments shew, that, in this case, there needs no propulsion of the air, by the swelling thorax, or abdomen, into the lungs: since, upon the bare dilatation of the thorax, the spring of that internal air, which possesses as much of the cavity of the chest, as the lungs fill not, being much weaken'd, the external and contiguous air, must necessarily press thro' the open wind-pipe into the lungs, as finding there the least resistance.

And hence, by the way, we are assist'd to judge of that famous controversy, among naturalists and physicians, ever since the time of *Galen*; some maintaining, that the chest, with the contain'd lungs, resembles a pair of bellows, which are, therefore, fill'd, because dilated: and others pleading, that the comparison should be made with a bladder, which is, therefore, dilated, because it is fill'd. For, as to the thorax, it seems evidently, like a pair of bellows, to be partly fill'd with air, because it was dilated; but as for the lungs, which want fibres to distend them, they may fitly be compared to a bladder; since they are dilated, by being fill'd with that air which rusheth into them, upon the dilatation of the chest, in the cavity whereof, it finds less resistance to its spring, than elsewhere. And this calls to mind that strange observation of *Nicholaus Fontanus*, a physician at *Amsterdam*, who declares, that, in a boy of the same city, four years old, there

there was found, instead of lungs, a certain membranous bladder, which, being fill'd with air, and furnish'd with little veins, had its origin from the wind-pipe. This being supposed true, I leave it to be consider'd, how well it will agree with most of the opinions, as to respiration.

And thus may the grand objection of *Bartholine*, and others, be answer'd; but I leave anatomists to consider what is to be said to some observations, that seem to contradict those anatomical experiments above-mention'd: such was, particularly, that in *Sennertus*, of a melancholy student, who, having stabb'd himself, and pierc'd the diaphragm in the tendinous part, lived seven months after the wound was made; but, dying at length, it appear'd so great, being, perhaps, dilated by his straining to vomit, that the whole stomach was found to have got by it, into the left side of the thorax. And such, also, was the accident which happen'd to a nobleman whom I have seen, and who is yet alive; in whose chest, there hath, for these many years, remain'd a hole so great, that the motion of his heart may be perceiv'd thro' it. An ingenious conjecture hath been made, at the cause of the sudden death of animals in the exhausted receiver; which supposes it to be, not the want of air that destroys them, but the pressure of that in the cavity of the chest; as if the spring thereof, being no longer balanced by the ambient air, thereby becomes so strong, as to keep the thorax forcibly distended, hinder its wonted contraction, and so compress the lungs and their vessels, as to obstruct the circulation of the blood. But *Wallaus* relates, that he often observ'd, in the dissection of live bodies, the membrane which invests the lungs, had pores in it, as big as the larger sort of peas: which agrees with the observations of surgeons and physicians, that matter, collected in the thorax, hath penetrated into the lungs, and been discharged by coughing. And most of the animals, kill'd in our engine, were birds; whose lungs, *Dr. Harvey* has observ'd, very manifestly to open, at their extremities, into the abdomen: and, by such perforations, we may well suppose the passage free, betwixt the external air, and that in the abdomen. Besides, to shew that the animals, which expired in our glasses, need not be supposed to have been kill'd by the want of air; we foresee another argument, which ought not to be conceal'd. The possibility of a vacuum is, frequently, deny'd; and the spaces void of air, and other grosser bodies are, all of them, supposed exactly replenished with a certain ethereal matter, so thin and subtile, that it can freely penetrate the pores of the most compact, and close bodies, even glass itself. Hence it may be said, that the animals, included in our receiver, died not so much for want of air, as because the air pumped out, was necessarily succeeded by an ethereal substance; which, consisting of parts vehemently agitated, and so very small, as, without resistance, to pass in and out, thro' the pores of glass; a considerable quantity of this restless matter, meeting together in the receiver, may be quickly able, by the excessive heat of it, to destroy a little animal, or, at least, make the air too hot to be fit for respiration.

But

But we have already answer'd this objection, by the late experiments; which shew no heat to be generated in our exhausted receiver.

It might, also, seem probable, that, upon the sudden removal of the wonted pressure of the ambient air, the warm blood of our animals was so vehemently expanded, as to disturb the circulation, and so disorder the whole œconomy of the body; did such animals, alone, as are of a hot constitution, lose their lives in the exhausted engine. But as to the use of air, in respiration, 'tis known to serve in the production and modulation of the voice; the expulsion of excrements, by coughing; the conveying in of odours, by inspiration, &c. which are rather convenient for the well-being of an animal, than necessary to life. *Hippocrates* says, of the air in animals endow'd with lungs, that "tis the cause both of life, and diseases; that 'tis so necessary, a man cannot live part of a day without it; and that respiration, alone, is the action which can never be suspended." But, as to the reason why the inspiration, and expiration of air, are so very necessary to life, both naturalists, and physicians, differ so widely, that it will be very difficult, either to reconcile their opinions, or determine their controversies.

Many suppose the chief use of respiration, is to cool and temper that heat in the heart and blood, which would, otherwise, be immoderate. They, also, suppose, that the air is necessary, by its coldness, to condense the blood that passeth out of the right ventricle of the heart into the lungs; whereby it may gain such a consistence, as is requisite to make it fit fuel for the vital flame in the left ventricle of the heart. And, indeed, fish, and other cold creatures, whose hearts have but one cavity, are unprovided of lungs. But, tho', possibly, the air inspired, may, sometimes, be of use in refrigerating the heart; yet, it may be objected, that several cold creatures, as, particularly frogs, stand in need of respiration; which seems unnecessary for refrigeration to them, who are destitute of any sensible heat, and live in the cold water; that even decrepid old men, whose natural heat is very languid, and almost extinguished, have, yet, a necessity of frequent respiration; that a temperate air, is fittest for the generality of breathing creatures; and as an air too hot, so also, an air too cold, may be inconvenient for them; that in some diseases, the natural heat is so weaken'd, that were the use of respiration to cool, it would be more hurtful, than beneficial, &c. These, and other objections, might be oppos'd, and press'd, against the recited opinion; but, we shall only add, that it appears not, by our foregoing experiments, in the exhausted receiver, where animals die so suddenly, for want of respiration, that the ambient body is sensibly hotter, than the common air.

Others will have the very substance of the air to get, by the vessels of the lungs, to the left ventricle of the heart, not only to temper its heat, but to provide for the generation of spirits. And, these alledge the authority of the ancients, among whom, *Hippocrates* seems, manifestly, to favour their opinion; and both *Aristotle*, and *Galen*, sometimes appear inclinable to it. But, it seems very difficult to shew, how the air is convey'd into

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the left ventricle of the heart; especially, since the systole and diastole of the heart, and lungs, are very far from synchronal: besides, the spirits appearing to be, but the most subtil, and unctuous particles of the blood, seem of a very different nature, from that of dry incombustible corpuscles of air.

Another opinion of respiration makes the genuine use of it to be the ventilation of the blood, in its passage thro' the lungs; whereby, it is disburthen'd of those excrementitious steams, proceeding, for the most part, from the superfluous serosities of the blood, and chyle. But, this hypothesis may be explain'd two ways. For the necessity of air in respiration, may be suppos'd to proceed from hence; that, as a flame cannot long subsist in a narrow, and close place, because, the fuliginous steams, it continually throws out, cannot be long receiv'd into the ambient body, which, after a while, growing too full of them, to admit any more, stifles it; so the vital fire in the heart requires an ambient body of a yielding nature, to receive into it the superfluous serosities, and other recrements of the blood; the seasonable expulsion whereof, is requisite to depurate the mass, and make it fit, both to circulate, and to maintain the vital heat residing in the heart. The other way, is, by supposing, that the air doth, not only as a receptacle, admit into its interstices the excrementitious vapours of the blood, when they are expell'd thro' the wind-pipe; but, also conveys them out of the lungs; because, the inspired air, reaching to all the ends of the *Aspera Arteria*, there associates itself with the exhalations of the circulating blood: and, when 'tis exploded, carries them away with itself, as winds speedily dry up the surfaces of wet bodies.

Now, to the first of these two ways, our engine affords us this objection; that upon the exsuction of the air, the animals die a great deal sooner, than if it were left in the vessel; tho', by that exsuction, the ambient space is left much more free to receive the steams, that are either breathed out of the lungs of the animal, or discharg'd by insensible transpiration.

But, if the hypothesis be taken in the other sense, it seems agreeable to that grand observation, which the phenomena of our engine, and the relations of travellers suggest, that there is a certain consistence of air, requisite to respiration; so that, if it be too thick, and already over-charg'd with vapours, it will be unfit to unite with, and carry off those of the blood; as water will dissolve, and associate, but a certain proportion of saline corpuscles; and, if it be too thin, the number or size of the aerial particles is too small to receive, and carry off the excrements of the blood in due quantity.

Now, that air too much thicken'd with steams, is unfit for respiration, appears by what happens in the lead-mines of *Devonshire*, and, perhaps, of some other countries; for, I am credibly inform'd, that damps often rise here, which so thicken the air, as suddenly to stifle the workmen. And, that this proceeds, not from any arsenical, or poisonous exhalation contain'd in the damp; but, from too great a condensation of the air; seems

seems probable, because it often leisurely extinguishes the flames of their candles, or lamps; and also, because in those cellars, where large quantities of new wine are set to work, men have been suffocated by the steams exhaling from the must, and too much thickening the air: for this reason, in some hot countries, those who have occasion to go into such cellars, carry with them a quantity of well-kindled coals, which they hold near their faces, whereby, the fumes being dissipated, and the air rarified, the ambient body is reduced to a consistence fit for respiration.

And, by way of confirmation hereof, we may add, that in a small receiver, we carefully clos'd a bird, which, tho' for a quarter of an hour, he seem'd not much prejudiced, by the closeness of his prison, he, afterwards, began to pant vehemently, keep his bill open, and appear very sick; and, at length, after some long, and violent strainings, he cast up a little matter out of his stomach: and this he did several times, till growing so sick, that he stagger'd, and gasp'd, and was ready to expire. Now, we perceiv'd, that within three quarters of an hour, from the time he was put in, he had so thicken'd, and tainted the air, with the steams of his body, that it was become altogether unfit for the use of respiration; which is no wonder, since, according to *Sanctorius*, that part of our aliment, which goes off by insensible perspiration, exceeds, in weight, all the visible, and grosser excrements, both solid, and liquid.

That air too much dilated, is unfit for respiration, the sudden death of animals kill'd in our exhausted receiver, sufficiently manifests. And, it may well be doubted, whether if a man were rais'd to the very top of the atmosphere, he would be able to live there many minutes. *Josephus Acosta* tells us, that when he himself pass'd the high mountains of *Peru*, to which, he says, the *Alps* seem'd but as ordinary houses, compared with high towers; he, and his companions were surpriz'd with extreme pangs of straining, and vomiting blood, and with so violent a distemper, that he concludes, he should undoubtedly have died, but, that this lasted not above three, or four hours, before they came into a more natural temperature of air. Our author adds, that he is, therefore, perswaded, "the element of the air is there so subtle, and delicate, as to be inconsistent with the respiration of man, which requires a more gross, and temperate air."

But, perhaps, the air doth something more, than barely help to carry off what is thrown out of the blood, in its passage thro' the lungs, from the right ventricle of the heart to the left. For in phlegmatic constitutions, and diseases, the blood will circulate tolerably well, notwithstanding its being excessively serous; and in asthmatical cases, tho' the lungs be greatly stuff'd with viscid phlegm, yet the patient may live for some years: whence it is scarce probable, that either the detention of the superfluous serum of the blood, for a few moments in the lungs, should be able to kill a perfectly sound and lively animal; for we commonly found, upon repeated trials, in a small receiver, that, within half a minute, a bird would be surpriz'd by mortal convulsions, and, within a minute more, would die, beyond a possibility of recovery from the air, tho' never so hastily let in. And,

what shews it was not the closeness of the vessel, but the sudden exsuction of the air, that killed those creatures so soon; we once inclos'd a bird in a small receiver, where, for a while, he eat very chearfully some seeds that we convey'd in with him; and not only liv'd ten minutes, but had, probably, surviv'd much longer, tho' he had not been rescu'd. Another bird being, within half a minute, cast into violent convulsions, upon the exsuction of the air; we hastily turn'd the stop-cock, to let it in again, whereby the gasping animal was presently recover'd. And, at another time, we, at night, shut up a bird in one of our small receivers, and observ'd, that, for a while, he was so insensible of the alteration of the air, that he fell asleep, with his head under his wing; and tho' he afterwards awaked sick, yet he continued upon his legs, for above forty minutes; and then seeming ready to expire, we took him out, and soon found him lively. Upon the whole, there appears reason to suspect, that there is some use of the air, which we do not yet thoroughly understand, that makes it so necessary to the life of animals.

Paracelsus, indeed, tells us, that "as the stomach concocts the aliment, and makes part of it useful to the body, rejecting the other; so the lungs consume part of the air, and reject the rest." Whence, according to him, we may suppose a little vital quintessence in the air, which serves to refresh and restore our vital spirits; for which purpose, the grosser, and far greater part of the air, being unserviceable, it is not strange that an animal should incessantly require fresh air. This opinion, indeed, is not absurd; but it requires to be explain'd and prov'd: besides, some objections may be made to it, from what has been already argued against the transmutation of air, into vital spirits. Nor is it probable, that the bare want of the generation of the usual quantity of vital spirits, for less than one minute, should be able to kill a lively animal, without the help of any external violence. And, upon this supposition, *Cornelius Drebell*, is affirm'd, by many credible persons, to have contrived a vessel to be row'd under water: for *Drebell* conceiv'd, that it is not the whole body of the air, but a certain spirituous part of it, that fits it for respiration; which being spent, the remaining grosser body of the air, is unable to cherish the vital flame residing in the heart. So that, besides the mechanical contrivance of his boat, he had a chymical liquor, which, by unstopping the vessel wherein it was contain'd, the fumes of it would speedily restore to the air, fould by respiration, such a proportion of vital parts, as would make it again fit for that office; and having made it my business to learn this strange liquor, his relations constantly affirm'd, that *Drebell* would never disclose it, but to one person, who himself told me what it was. I have, therefore, been sometimes, inclined to suppose, the air necessary to ventilate and cherish the vital flame, which some imagine to be continually burning in the heart: for that, in our engine, the flame of a lamp will vanish almost as soon after the exsuction of the air, as the life of an animal. We have made a hard body, in the form of a clove, but twice as long, and proportionably thick, of such a composition, that if it be kindled at the upper end, it will

most

most certainly burn away to the bottom, much better than a match: this we often convey'd, kindled at the upper end, into a small receiver; but still found, that tho' presently, upon the exsuction of the air, it would leave smoking, and seem quite gone out; and again begin to smoke, as soon as the air was let in upon it; yet, if the air were kept out but four or five minutes, the fire would be totally, and irrecoverably extinguish'd. And, conveying a small lamp into a large receiver, with highly rectified spirit of wine, we could not, upon several trials, make the flame last two minutes, after the air was began to be drawn out. This latter opinion, however, has its difficulties: for tho', in the hearts of many animals, the blood be a warm liquor, and, in some, even hot; yet it is hard to conceive either how the air can get thither; or how, in case it could, it should increase the heat: since, however the air may increase the heat of a coal, by blowing off the ashes, and making the active corpuscles penetrate farther into the kindled body, and shatter it the more; yet hot liquors have their heat allay'd, by air blown on them. And, since some naturalists think the heat residing in the heart, to be a true flame, but temperate as the flame of spirit of wine; which will long burn upon fine linen, or paper, without consuming them; I wish they had been more curious to make different trials with that liquor. For the flame of highly rectified spirit of wine, will not only consume paper, and linen; but I have used it in lamps, to distil liquors out of tall cucurbits, and found that it gave, at least, as great a heat, as oil: nay, I have readily melted crude gold, with the bare flame of this spirit.

Dr. *Harvey* demands, "why a fœtus, even out of the womb, if involv'd in the secundines, may live, for a considerable time, without respiration; yet, if after having once began to breathe, its respiration be stop'd, it presently dies?" We pretend not to solve this problem, but made the following experiment with a view to it. We caus'd a bitch to be strangled, that was almost ready to whelp; and presently opening her, found four puppies; one of which we freed from the coats that involv'd him, and from the liquor wherein he swam, and observed, that he quickly open'd his mouth very wide, mov'd his tongue, and exercis'd respiration. Then we open'd both his abdomen, and chest, and cut the diaphragm asunder; notwithstanding which, he seem'd often to endeavour at respiration, and remarkably mov'd the intercostal muscles, part of the diaphragm, the mouth and tongue. But being desirous to try whether the other young ones, that had not yet breath'd at all, would long survive this; we took them out, and having open'd them, found none of them so much alive as to have any perceptible motion in their hearts; whereas the heart of that which had once enjoy'd the benefit of respiration, continued its motion so long, that we observ'd the auricle to contract, after five or six hours; and it continued about two hours longer.

It is much doubted, whether fish breathe under water. That such as are not of the whale kind, have no respiration, as 'tis exercised by beasts, and birds, may be argued from their having no cavity in their hearts, and from their

their want of lungs, whence they are observ'd to be mute; unless we say, that their gills answer to lungs. But that air is necessary even to the lives of fish; and that therefore, 'tis probable, they have some obscure kind of respiration, seems manifest from observations, and experiments. Several authors tell us, that fish soon die in ponds, and glasses quite fill'd with water, if the one be so frozen over, and the other so closely stop'd, that they cannot enjoy the benefit of the air. And our engine hath taught us, that many little parcels of interspersed air, lurk in water; and this, perhaps, fish may make some use of.

Removing a large eel, out of a vessel of water, into our great receiver, we caus'd the air to be evacuated, and observ'd, that after some motion in the glass, she seem'd somewhat discomposed, and, at length, turn'd up her belly, and afterwards lay altogether moveless, as if quite dead; but upon taking her out of the receiver, she shew'd herself as much alive as before.

But, indeed, a large grey house-snail, being clos'd up in one of our small receivers, neither fell down from the side of the glass, upon drawing out the air; nor was so much as depriv'd of progressive motion thereby: tho', except this, we never put any living creature into our exhausted receiver, but what gave signs of death.

Hippocrates, and some learned physicians of late, suppose, that a fœtus respire in the womb; but it seems very difficult to conceive how air should traverse the body of the mother, and the teguments of the child: and since nature hath, in new-born infants, contriv'd peculiar temporary vessels, that the blood may circulate thro' other passages, than it does in the same individuals, when they come to have the free use of their lungs, 'tis improbable that the fœtus in the womb should properly respire: but, then, since our experiments have manifested, that almost all kinds of liquors, as well as water, abound with interspersed corpuscles of air, it seems not altogether absurd, that when the fœtus is grown big, it may exercise some obscure respiration; especially since children have been heard to cry in the mother's womb. And I know a young lady, whose friends, when she once went with child, complain'd to me, that she was several times much frighted with such cries; which, till I disabused her, she, and her friends, look'd upon as portentous. And 'tis no very unfrequent thing, to hear the chick pip in the egg, before the shell is broken. This, however, I only bring as a probable argument, till I can discover whether the motion of a rarified substance, tho' no true air, may not, at the top of the larynx, produce a sound; since the blade of a knife, held in several postures, in the stream of the vapours that issues out of an æolipile, will afford various and very audible sounds. I have, also, had thoughts of trying to make a large receiver, with little glass windows, capable of holding a man, who may observe several things as to respiration, &c. and, in case of fainting, may, by giving a sign, be immediately relieved with fresh air. And it seems not impossible, that some men, by use, may bring themselves to support the want of air a pretty while; since we see that several will live much longer than others under water. Those who dive for pearls in
the

the *West-Indies*, are reported to be able to stay a whole hour under water: and *Cardan* tells us of one *Colanus*, a diver in *Sicily*, who was able to continue there three or four times as long. We have, also, often seen in *England*, a corpulent man, who descends to the bottom of the *Thames*, and thence brings large fish, alive in his hands, out of deep holes; as *Acosta* tells us, he saw in *Peru*, the like manner of fishing practised by the *Indians*.

However, there are but few men, who, even by use, can support, for many minutes, the want of air: a famous diver, of my acquaintance, tells me, that at the depth of 50 or 60 feet under water, he cannot continue above two minutes, without resorting to the air which he carries down with him in an engine. He, also, told me, that by the help of sponges dip'd in oil, and held in his mouth, he could much longer support the want of respiration, under water, than without them: the true cause of which, would, perhaps, if discover'd, hint the nature of respiration in fish. But the necessity of air to the greatest part of animals, unaccustomed to the want of it, may be best judg'd of by the following experiment.

We convey'd a bee, a flesh-fly, and a palmer-worm, into one of our small receivers, and, upon exhausting thereof, observ'd, that the bee and the fly fell down, and lay with their bellies upwards, and that the worm seem'd to be suddenly struck dead; all of them lying without motion, or any other discernible sign of life, in less than one minute; notwithstanding the smallness of the animals, in proportion to the receiver, which, too, was not free from leaks: but we had no sooner re-admitted the air, than all the three insects gave signs of life, and, by degrees, recover'd. When we had again drawn out the air, their motions presently ceased, and they fell down, seemingly dead, as before; continuing moveless, as long as, by pumping, the vessel was kept exhausted. Herein appears the wise conduct, and goodness of the creator, who, by giving the air a spring, hath made it very difficult to exclude a thing so necessary to animals. And here we may suspect, that if insects have no lungs, nor any part answering thereto, the ambient air affects, and relieves them, at the pores of their skin; for, as *Hippocrates* well said, "a living body is every where perispirable." Thus the moister parts of the air readily insinuate themselves into, and recede from the pores of the beards of wild oats, and of other wild plants, which almost continually wreath and untwist themselves, according to the lightest variations in the temperature of the air.

We, particularly, took notice in this experiment, that, when, at any time, upon the re-admission of the air, the bee began to recover, the first sign of life she gave, was a vehement panting, which appear'd near the tail; the like we have observ'd in bees drown'd in water, when they first come to be revived, by a convenient heat; as if the air were, in one case, as proper to set the spirits, and alimential juice in motion, as heat, in the other.

This experiment, also, seems to manifest, that, even living creatures, man always excepted, are a kind of very curious machines. For, here we see animals lively, and perfectly sound, immediately deprived of motion, and

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and all discernible signs of life, and reduced to a condition that differs from death, only, in being not absolutely irrecoverable: and this is perform'd without the least external violence, more than is offer'd to a wind-mill, when, the wind ceasing to blow on the sails, all the several parts remain moveless, and useless, till a new breeze puts them again into motion.

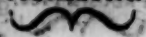
'Tis known, that bees, and some other insects, will walk, and fly, for a great while after their heads are off, and sometimes one half of the body will, for several hours, walk up and down, when it is sever'd from the other; yet, upon the exsuction of the air in this experiment, not only the progressive motion of the whole body, but the very motions of the limbs immediately cease; as if the air were more necessary to these animals, than their own heads.

But, in these insects, that fluid body, in which life chiefly resides, seems nothing near so dissipable, as in perfect animals. For, the birds convey'd into our small receiver, were, within two minutes, brought past recovery; but, we were unable to kill our insects, by the exsuction of the air: for, tho' as long as the pump was kept working, they continued immoveable, yet, when that rested, the air, which press'd in, at the unperceiv'd leaks, slowly restored them to the free exercise, and functions of life. Without denying, then, that the air may be, sometimes, very useful, by condensing, and cooling the blood, that passeth thro' the lungs; I am of opinion, that the depuration of that animal fluid, is one of the ordinary, and principal uses of respiration.

Whether the
action of men-
struum depends
upon the pressure
of the air.

42. Having entertain'd a suspicion, that the action of corrosive liquors in dissolving bodies, may be considerably varied by the gravitation, or pressure of the incumbent air, and the removal of it; I examined my conjecture by the following experiment.

I cast ten whole pieces or sprigs of red coral, into as much spirit of vinegar as reach'd an inch above them; then putting these, together with the menstruum, into a long-neck'd vial, whereof they scarce fill'd a third part, we convey'd that vial into one of our small receivers, and having fasten'd on the cover, we let the liquor remain unmov'd a while. But finding, there only arose, as before, a number of small bubbles, that caused no sensible froth upon the surface of the vinegar; we made two or three exsuctions of the air, upon which there rose, from the coral, such a multitude of bubbles, as made the whole body of the menstruum appear white; and soon after, yielded a froth, equal in magnitude to the rest of the liquor; the menstruum plainly appearing to boil: tho', if we desisted but one minute from pumping, the decrease of the froth, and ebullition, upon the getting in of a little air, at some leak or other, seem'd to argue, that the removal of the pressure of the external air, gave occasion to this effervescence. But, for farther satisfaction, we let in the external air at the stop-cock, when, immediately, the froth vanished; and so many of the bubbles, within the body of the liquor, disappear'd, that it lost its whiteness, and became transparent again; the menstruum, also, working as languidly upon the coral, as before they were put into the receiver: but,
when



when we had again drawn out the air, first the whiteness re-appear'd; and then the ebullition was renew'd; which, at length, grew so great, that, for three or four times successively, when the air was let out of the receiver into the emptied cylinder, the froth overflow'd the glass, and ran down the sides of it: and yet, upon re-admitting the excluded air, it grew, immediately, calm and transparent; as if its operation upon the coral, had been facilitated by the exsuction of the incumbent air; which, on its recess, left it easier for the more active parts of the liquor to shew themselves, than whilst the pressure of the air continued. It may, indeed, be suspected, that those vast and numerous bubbles proceeded not from the action of the menstruum upon the coral, but from the sudden emersion of those many little parcels of air, which are dispersed in liquors; but, having had this suspicion before we made the experiment, we convey'd our distill'd vinegar, alone, into the receiver, and kept it a while there, to free it from its bubbles, before ever we put the coral into it. It may be suspected, likewise, that the agitation of the liquor, consequent upon shaking the glass, by pumping, might occasion the ebullition; but, upon trial, there appear'd no considerable change in the liquor, or its operation, tho' the containing vessel was shaken, if no air were drawn out. The experiment was again made in a small receiver, upon coral grossly powder'd, with a success very like the former; only the coral, being now reduced to smaller parts, so many little lumps of it would, upon the ebullition of the liquor, be carry'd, and buoy'd up, by the emerging bubbles, as sometimes to darken the vial; tho' they would fall again, upon letting in the air. We must not omit, that, when the spirit of vinegar was boiling upon the coral, we took out the vial, but could not find that the liquor was sensibly hot.

43. We caused water to be long boil'd, that it might be freed from its air; then, almost filling a four-ounce glass-vial with it, we convey'd that, whilst the water was yet hot, into a small receiver; and having luted on the cover, the air was drawn out: upon the two first exsuctions, there scarce appear'd any change in the liquor; nor was there any great alteration made by the third; but at the fourth, and afterwards, the water appear'd to boil in the vial, as if it had stood over a very strong fire; for the bubbles were much greater than are usually found, upon the ebullition of large quantities of water. And this effervescence was so great, that, the liquor, boiling over the top of the neck, much of it ran down into the receiver, and, sometimes, continued to boil there. In prosecuting the experiment, we observ'd, that, sometimes, after the first ebullition, we were obliged to make several exsuctions, before the liquor could be brought to boil again: but, at other times, as often as the air was suffer'd to pass from the receiver into the pump, the effervescence would begin afresh; tho' the pump were ply'd for a pretty while together: which seem'd to argue, that the boiling of the water proceeded from hence, that, upon withdrawing the pressure of the incumbent air, either the fiery corpuscles, or rather the vapours agitated by the heat in the water, were permitted greatly

The ebullition of warm liquors in vacuo.

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greatly to expand themselves in the evacuated receiver; and, in their tumultuous dilatation, lifting up the higher part of the water, and, turning it into bubbles, made it appear to boil; for the effervescence was confined to the upper part of the water; the lower remaining quiet, unless the liquor were but shallow. And tho', sometimes, as we said, the ebullition began again, after it had ceased a pretty while; whence it seem'd that some concurrent cause did a little modify the operation of heat; yet, when the water, in the vial, could, by pumping, be brought to boil no more, the same water, being, in the very same vial, convey'd back to the receiver, was quickly brought to boil afresh, with vehemence, and for a considerable time; whilst a new parcel, taken out of the same boil'd water with the former, and put in cold, could not, by pumping, be brought to shew the least effervescence. And hot sallet-oil shew'd no effervescence in our receiver; but the chymical oil of turpentine was presently made to boil up, till it reached four or five times its former height in the vial; and continued boiling, till it was almost but luke-warm. Wine, also, being convey'd in hot, did, at the very first exsuction, begin to boil so vehemently, that, in a short time, while the pump was kept moving, four parts of five boil'd over the vial, tho' it had a long neck. And even of the water itself, near one half would, sometimes, boil over into the receiver, before it became luke-warm. It was, also, remarkable, that once, when the air had been drawn out, the liquor did, upon a single exsuction, boil so long, with prodigiously vast bubbles, that the effervescence lasted almost a quarter of a minute. Hence it appears, that the air, by its stronger, or weaker pressure, may very much modify several operations of that vehement and tumultuous agitation of the small parts of bodies, wherein the nature of heat seems to consist: so that if a heated body were convey'd above the atmosphere, 'tis probable, that the heat would have a different operation, as to the power of dissipating the parts of it, from what it hath here below.

S E C T. II.

The air-pump
fanter im-
proved.

HAVING now presented my great engine to the royal society, I was obliged to procure another; wherein, tho' the construction, in general, be the same in both, there were some alterations, and improvements made.

Fig. 42 & 43.

The figures represent this engine, as ready for work; and, because the sucker is to be always under water, and the perforation P Q, that is continu'd perpendicularly quite thro' it, and serves, together with the stick R S, for a valve, is to be stopp'd at the bottom of the cylinder, as at N O, when 'tis full of water; 'twas requisite to make the stick R P, two or three feet long. But the chief thing is, that, in the second figure, the pipe A B, whose end B, bends upwards, lies in a groove, purposely made, in the flat wooden board C D E F, on which the receivers are to rest. This square board, I caused to be overlaid with very good cement, on which was applied a strong plate of iron, of the bigness and shape of the board, leaving only a small

small hole, for the erect part of the pipe to come out at; which I added, not only to keep the board from warping, but because the pressure of the atmosphere on the side of it, when there is none, or very little, on the other, will enable many aerial particles to strain thro' the wood, tho' thick, and moisten'd with oil. To this iron-plate, we can fit a lip, turning up about it, to prevent the water, that, on some occasions, comes from the receiver, from falling on the floor. And, by the way, tho' the stop-cock GHIK, that belongs to the pipe, may be inserted at I, into the cylinder LMNO, by the help of folder; yet we chose to have the branch I, of the stop-cock, made like a screw, which, being once firmly fitted to the barrel, is not apt to be broken off, and may be more easily mended, if any thing happen to be out of order; which the engine is most liable to, in, or about the pipe.

The square, and hollow wooden part of this engine discernible in the first figure, is so made, not only to contain the cylinder, but as much water as will always keep it quite covered, by which means, the sucker lying, and playing always under water, is continually kept plump, and turgid; and the water being ready to fill up any little interval, that may happen, between the sucker, and the inside of the barrel, farther conduces to keep out the air. But, if great care be not taken in turning the stop-cock, the water will be impell'd into the receiver, and prejudice several experiments, when the included bodies may be spoil'd, or impair'd by that liquor.

The flat plate, lately mentioned, has this great conveniency in many experiments, that the receiver needs no stop-cock of its own; for such a vessel being made of an entire piece of glass, and laid upon the plate, well cover'd with cement, can better keep out the air, than if there were a stop-cock, at which the air too frequently gets in.

A good cement, wherewith to fasten the receivers to the iron-plate, is a thing of great moment in making the following experiments, and we employ different compositions for different purposes; but, in general, only a mixture of bees-wax and turpentine, made with equal parts for the winter, and three parts of the former to two of the latter, for the summer.

1. We took a vial with a small neck, and having fill'd about a fourth part of it with quick-silver, we so erected, and fasten'd a long and slender pipe of glass, open at both ends, in the neck thereof, with hard sealing wax, that the lower end reach'd almost to the bottom of the quick-silver, and the upper more than a yard above the vial; then, having blown in a little air, we convey'd the whole into a long slender receiver: upon evacuating whereof, we found, that the spring of the air included in the vial, impell'd the quick-silver into the erected pipe, to the height of 27 inches; and suffering the external air to return into the receiver, the quick-silver subsided in the tube, sometimes almost, and sometimes quite as low as the stagnant mercury in the vial.

*Mercury rais'd
by the spring of
a little included
air.
Fig. 44.*

This experiment we made several times; and having once blown in so much, air, that what was in the cavity of the vial rais'd and kept the quick-silver

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three inches high in the pipe, we found, by emptying the receiver, that the quick-silver rose 30 inches, or more, above that in the vial.

Sometimes it may happen, that the mercury, when taken very soon out of the receiver, will not appear to have subsided to its first station; which is not to be wonder'd at; since, in a receiver, containing but little air, the heat of the cement, and of the iron employ'd to melt it quite round the glass, may impart a little warmth to the air in the vial, which will afterwards return to its former temper.

'Tis very remarkable, if the receiver be properly stopp'd and slender enough, that upon the turning of the stop-cock to let out the air at the first exsuction, the mercury will be impell'd up by the spring of that in the vial, so as to rise several inches above the height it will afterwards rest at, and make several vibrations up and down before it comes to settle; just as the mercury does in the *Torriceilian* experiment: and such motions of the mercury will be made for four or five subsequent exsuctions; but they grow gradually less, as the spring of the included air is weaken'd.

At the first exsuction, when the spring of the included air was yet strong, we found the mercury would be rais'd above half, if not $\frac{2}{3}$ of the whole height, whereto 'twill, at length, ascend: but the subsequent strokes add a less proportion of height to the mercurial cylinder, successively; because the more mercury is impell'd into the tube, the greater weight presses upon the included air; and because the air hereby gains the more room, in the vial, to expand itself: whence the spring must be, proportionably, weaken'd.

Lastly, in making of these trials, I observed the mercury in a good barometer, and found its greatest height twenty-nine inches, and $\frac{1}{2}$; and soon after we had finished, but twenty-nine.

To estimate the quantity of air, that had rais'd the quick-silver to twenty-seven inches; we counterpois'd the vial, employ'd about this experiment, whilst it was empty; afterwards fill'd it with water, and found the liquor to weigh five ounces, two drams, and twenty grains; then having pour'd out the water, till it was sunk to a mark, we made on the outside of the glass, we weigh'd the remaining water, equal in bulk to the quick-silver, and found it one ounce, two drams, fourteen grains: so that the air, which had rais'd up the mercury, possess'd, before its expansion, the space but of four ounces, and a few odd grains in the vial. The bore of the pipe used in this experiment, was about $\frac{1}{4}$ inch in diameter.

Much included
air raises mer-
cury, but to the u-
sual standard of
the barometer.

2. Into a strong glass bottle, capable of holding a quart, we put a convenient quantity of quick-silver, and erected in it a very long slender pipe of glass, open at both ends, and reaching with the lower beneath the surface of the stagnant mercury; and having well cemented this pipe in the neck of the bottle, we convey'd the whole into a receiver, much larger than the former; and then the engine being work'd, the quick-silver was presently rais'd to a greater height than before; and when it stood still, we, by the help of some marks made before-hand on the pipe, and a very long and well-divided ruler, carefully measured the height of the mercurial

curial cylinder, which we found to be 29 inches and about $\frac{7}{8}$; but deducting half an inch, which was rais'd, before we employ'd the pump, by some air, that had been blown into the bottle, to try whether it were stanch, there remain'd 29 inches and near $\frac{1}{4}$ for the height of the mercury rais'd by the spring of the air shut up in the bottle; and then, consulting the barometer, which stood in another part of the house, I found the weight of the atmosphere sustain'd a mercurial cylinder, of about twenty-nine inches, and a half.

We caus'd the pump to be well ply'd, to try whether the quick-silver would not rise higher; but were confirm'd, that the spring of the air was insufficient for that purpose.

3. Taking the glass-bottle, used in the former experiment, and erecting in it, after the manner above-described, a cylindrical pipe of glass, much larger than the other; we prosecuted the experiment, as with the slender tube before-mention'd, and found, that, by the spring of the air in the bottle, the quick-silver was rais'd twenty-eight inches, and one eighth; that is, above an inch short of the mercurial cylinder in the barometer, at the same time; a difference no greater than I expected; considering the weight of the atmosphere, remains the same, when the mercury is at its full height, in a seal'd tube, whether great or small; whilst the spring of our included air must needs be weaken'd, the larger the tube, and the higher the mercury is impell'd in it. Whence, 'tis considerable, that the spring of so little air should raise the mercury within an inch as high in a wide, as in a slender tube; for the diameter of the bore of the former, was double to that of the latter: and the greater mercurial cylinder may be suppos'd to have weigh'd near four times as much as the less; allowance being made for an inch difference in their heights. But, in case these had been equal, then the solidity of the cylinders would have been as their bases; that is, as the squares of their diameters, or as 1 to 4.

The spring of included air raises mercury nearly to an equal height, in unequal tubes.

We thought it worth trying, whether, when the included air had rais'd this great cylinder of mercury to the utmost height it could, by the spring it then had, heat would not force it still higher. And, having caus'd a hot iron, and a shovel of kindled coals, to be held near the opposite parts of the receiver; we perceived, after a while, that the mercury ascended one eighth of an inch, or more, above the greatest height it had reached before; and, causing the pump to be ply'd again, to withdraw the air I suspected to have stole in; the mercury was quickly rais'd five eighths of an inch, by virtue of the additional force which the included air acquired by the heat.

4. We took a glass-bottle, furnished with a convenient quantity of water, and fitted it with a slender glass-pipe, about three feet long, open at both ends; which was so placed, that the lower orifice reach'd far beneath the surface of the water, and the pipe itself pass'd, perpendicularly, upwards, thro' the neck; which, by the pipe, and hard cement, was so firmly clos'd, that no water, or air, could get out of the bottle, or external air get into it; but by passing thro' the pipe. This instrument

A fountain made by the spring of un-compressed air. Fig. 45.

we

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we convey'd into a large receiver, shaped like a pear; of which a great part of the obtuse end, and a small portion of the sharp one, were cut off by sections parallel to the horizon. And, because this receiver was not long enough to receive the whole pipe, there was cemented on, to the upper part of it, a smaller, of such a length and bigness, that the higher end of the pipe might reach to the middle of its cavity; and that the motions of the springing water might have a convenient scope, and be the better observed.

This double receiver, being cemented on to the engine, a little of the air was, by one stroke of the pump, drawn from it; by which, the pressure of the remaining air being weaken'd, that included in the bottle, having not its spring, likewise, weaken'd, expanded itself; and, consequently, impell'd up the water, in the same bottle, thro' the pipe, so as to make it strike briskly, at first, against that part of the top of the small receiver, which was just over the orifice of the tube. But, after the water was, for a while, thus forced up, in a perpendicular line, it would be impell'd up less strongly, and less directly, till the air, in the bottle, being as much expanded, as that in the receiver, it quite ceased to ascend, unless by pumping a little more air out of the receiver, we renew'd it again. The other figure is designed to represent the difference that would happen, if, instead of making this experiment with water, it were made with quick-silver.

Fig. 46.

In making this experiment, 'tis convenient that the upper part of the pipe be very slender; whence the water, having but a very small orifice to issue out at, may be spent but slowly, and thereby make the experiment last so much the longer: or, instead of making the upper part of the pipe slender; a top, consisting of three, or more, very slender pipes, with a small hole at the end of each, may be cemented on to it; that one of these, pointing directly upwards, and the others to the right hand, and to the left, the water may spin out several ways at once; by which kind of branched pipes, we have, sometimes, imitated a *Jet d'eau*, and the artificial fountains of gardens, and grotto's.

Hence we infer, that, had we not wanted convenient vessels, we might, by the pressure of the air, included in the bottle, have raised water fourteen times as high as we did quick-silver in the former experiment; since, upon weakening the pressure of the air, but a little, in the double receiver, that within the bottle was able to impel the water, forcibly, and for a considerable time, to the top of a pipe a yard long, and higher.

Hence, too, it appears, that, in those hydraulo-pneumatical engines, where water is placed between two parcels of air, the water may be put in motion, as well by the mere dilatation of one of the parcels, as by giving a new force by heat, or compression, to the other. And, whether this mechanical principle of motion may not prove useful in engines, we leave to be consider'd.

But if, when some of the air had been pumped out of the receiver, we removed that double vessel from the bottle, the external air would, by its weight,

weight, suddenly depresses the water in the pipe, till, having driven it to the very bottom, it ascended in numerous bubbles thro' the water, and joined it self with the air incumbent on that liquor. 'Twas here observable, that all the external air, which got into the bottle, did not come in suddenly; but, after the first irruption, we could perceive, from time to time, new portions of air, leisurely insinuate themselves thro' the pipe into the bottle, and emerge thro' the stagnant water in bubbles, that succeeded one another very slowly; as if the spring of the included air, having been once deprived of its natural constitution, by its late expansion, could be but gradually reduced to it, by the weight of the atmosphere, which was still the same; or rather, as if between the spring of the included, and the pressure of the external air, balancing each other, there happen'd some such thing as is observable in scales, of which one is too much depress'd; whilst the motion becomes slower, as the weights are nearer to an equilibrium.

But, our principal design, in this experiment, was to observe, whether the lines made by the water, in its efflux, would retain the same figure, notwithstanding the rarification of the air, in the upper part of the receiver; and, for this purpose, it is best to make the observation towards the latter-end of the experiment; because, then, the receiver being most exhausted, the difference, made by the change of the density of the medium in which the streams of water move, is likely to be best discern'd. And this convenience we had, by our way of making the experiment, that we could observe the lines, described by the flowing water, as the projection thereof grew fainter. But, for want of a large upper receiver, we could not be satisfied in the nature of the curve; tho' both *Dr. Wallis*, and myself, found it to be, sometimes, part of a parabola.

5. We provided a brass ring of a considerable thickness, in height three inches; and the diameter of its cavity, as well at the upper as the lower orifice, was something more than three inches. To this ring we successively fasten'd, with cement, several round pieces of window-glass, and thereby made the ring a kind of receiver, whose open orifice we carefully cemented on to the engine; and found, that usually, at the first exsuction, the glass plate would be broken inwards, with such violence, as to be shatter'd into a great multitude of small fragments; and the irruption of the external air, driving in the glass constantly, made a loud report, like that of a pistol.

6. If, instead of the brass ring, above-mention'd, both orifices whereof are equal in breadth, you employ a taller hollow piece of brass, or latton, shaped like a truncate cone; and the two orifices be made very unequal; as if the larger be as wide as that of our brass ring, and the straiter were less than an inch in diameter; and this piece of metal be made use of, as that in the preceding experiment the flat glass will be easily broken when cemented to the wider orifice: but, if the narrower orifice be turn'd upward, the glass thereon, if it be of a due strength, tho' no thicker than the former, notwithstanding the air is withdrawn from beneath it, will remain

Flat glasses broken by the weight of the atmosphere.

*Without the assistance of a *vacui*.*

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remain entire: which sufficiently argues, that nature's abhorrence of a vacuum, is not the cause why glasses are usually broken in such experiments, since, whether the wider, or narrower orifice be uppermost, and cover'd, the capacity of the exhausted vessel, will be equal; and therefore nature ought to break the glass, in one case, as well as the other.

This phenomenon, therefore, is more properly explain'd, by saying, that when the wider orifice lies uppermost, the glass that covers it, must serve for the basis of a large column of the atmosphere, which, by its great weight, may easily force thro' the glass; whereas, when the smaller orifice is uppermost, there rests upon its cover, so slender a pillar of air, as cannot, by its weight, surmount the natural cohesion of the parts of the glass.

Blown bladders
burst by the
spring of the air
included in
them.

7. We seldom fail'd of bursting blown bladders in our exhausted receiver, by tying their necks very closely, and keeping them, for a pretty while, in the glass, whilst the air was exhausting, and then taking them out again; that the fibres being stretch'd, and relax'd, and the capacity diminish'd by a new ligature, tho' the air were the same, and the membrane being not so able to yield, as before; upon the second exhaustion of the receiver, they would break far more easily, than otherwise; and sometimes be oddly lacerated.

A considerable
weight lifted by
the bare spring
of a little air,
included in a
bladder.

8. We took a middle-sized bladder, and having press'd out the air, till there remain'd but about a fourth or fifth part, we caus'd the neck to be very strongly ty'd again; and, about the opposite part of the bladder, within an inch of the bottom, we so strongly tied another string, that it would not be slip'd off, by a considerable weight hung at it. Then fastening the neck to the turn-key, we convey'd the bladder, and the weight hanging at it, into a large receiver; when, by plying the pump, the air, within the bladder, being freed from the pressure of the air without it, manifestly swell'd by its own spring, and thereby greatly shortned the bladder that contain'd it, and lifted up the weight, which exceeded 15 pounds.

After this, we took a large bladder, and having let out so much air, that it was left lank, we fasten'd the two ends of it to the upper part of the receiver, and hung a weight from the middle of the bladder; then exhausting the receiver, as before, tho' the bladder, and this new weight, which stretch'd it, reach'd so low, that, for a while, we could scarce see whether it hung in the air or no; yet, at length, we perceiv'd the bladder to swell, and concluded it had lifted up its clog about an inch; as was confirm'd by the return of the air into the receiver; upon which, the bladder became more wrinkled than before; and the weight, amounting to about 28 pounds, descended.

Perhaps this experiment may conduce to explain muscular motion*.

* Something has, from this hint, been offer'd, with a very specious and plausible shew of reason, to account for muscular motion; but when thoroughly consider'd, it fails in solving the phenomenon. And the last best writer on this subject, the

learned Dr. Pemberton, after shewing the insufficiency of all other methods, accounts for it, from that subtle medium whereby the great Sir *Is. Newton* solves various other phenomena of nature.

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9. A large glass bubble, hermetically seal'd, being put into the receiver, and the air drawn out somewhat more than usual; tho' I had, several times, observed, that such bubbles would not break immediately, upon evacuating the receiver; yet this continued so long entire, after we had left off pumping, that presuming it had been blown too strong, I began to despair of success in the experiment; when, about four minutes after the pump had been let alone, the bubble surpriz'd us with breaking so violently, by the spring of the included air, that the fragments of it were dash'd every way against the sides of the receiver, and broke to powder.

Pneumatica
Glass bubbles
broke by the
spring of their
own air.

10. We took the brass-ring, lately mention'd, whereto were fitted some plates of window-glass, as covers; and, having carefully fasten'd one of them, with cement, to the upper orifice of the ring; and cementing the lower orifice to the engine, so that the vessel, composed of metal and glass, serv'd for a small receiver, we whelm'd another over it that was large and strong; which was also fasten'd to the engine, with cement, after the usual manner. By this contrivance, when the pump was set on work, the small included receiver must have its air withdrawn, while that, in the larger, could not get out, but by breaking through the glass; so that the internal air of the small receiver, being evacuated, the glass plate, that made part of it, must lie exposed to the pressure of the ambient air, shut up in the other receiver, without having the former assistance of the air, now withdrawn, to resist the pressure; wherefore, at the first or second exsuction of the air, included in the small receiver, the glass plate was, by the pressure of the incumbent air, contain'd in the larger one, broken into a hundred pieces, which were beaten inwards into the cavity of the ring.

*The external
force of the spring
of uncompress'd
air upon solid
bodies.*

But to shew that there needed not the spring of so great a quantity of included air, to break such glasses, we took another roundish one, which, tho' wide enough at the orifice, to cover the brass ring, and the new glass plate, that we had cemented on it, was yet so low, that it held but a sixth part of what the large receiver, formerly employ'd, would contain; and having whelm'd this vessel, which was shaped like a tumbler, over the little receiver, and well fasten'd it to the engine with cement, we found, that tho' the external receiver had a great part of its cavity fill'd by that included; yet when this internal one was evacuated, by an exsuction or two, the spring of the little air that remain'd, broke the plate into a multitude of fragments.

And because the glass plates, hitherto mention'd, seem'd not so thick, but that the pressure of the included air might give greater instances of its force; instead of the small metalline receivers, before employ'd, we took a strong, square bottle of glass, able to contain a pint, inverted it, and applied it to the engine, as a receiver; over which we whelm'd, and cemented the large one, formerly mention'd; and setting the pump on work, to empty the square bottle, the figure of the vessel allow'd the pressure of the air, included in the external receiver, to crush it into a great number of pieces.

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We, also, took another glass, of the shape, and about the bigness of the former; and having applied it to the engine, as before, and cover'd it with a receiver, that was a little higher than itself; upon exhausting the air, this was, likewise, broken into many fragments, some of them very thick: tho', probably, the cracks that reach'd thereto, were begun in much weaker parts of the glass.

The bottoms, and the necks of both these square bottles, were entire; by which it seem'd probable, that the vessels had been broken, by the pressure of the air against the sides, which were not only thinner than the other parts, but expos'd a larger superficies to the lateral pressure of the air, than to the perpendicular. We observ'd, in one of these experiments, that the vessel did not break presently, upon the last exsuction of the included air, but a considerable time after.

To confirm that it is the spring of the air, in the external receivers, that breaks the glasses, and to prevent some scruples, we apply'd a plate of glass, like those formerly mention'd, to the brass ring; but, in the cementing of it on, we plac'd, in the thickness of the cement, a small pipe of glass, about an inch long, whose cavity was not so big as that of a straw, and which, being left open at both ends, might serve for a little channel, for the air to pass thro', from the external receiver, to the internal; over this we whelm'd a small receiver, and then, tho' we work'd the pump much longer than would have been necessary, if the little pipe had not been made use of, we found the internal receiver continue entire; because the air, whose spring should have broken it, having liberty to pass thro' the pipe, and, consequently, to expand itself, into the place deserted by the air pump'd out, thereby weaken'd its spring too much for that purpose.

But, either the pipe must be made bigger, than that lately mention'd, or the exsuction of the air must not be sudden, by the pump; otherwise the plate of glass may be broken, notwithstanding the pipe: because the air contain'd in the external receiver, having a force much greater than is necessary to break such a plate, it may well happen, as I have sometimes found it, that if the air be hastily drawn out of the internal receiver, that which should succeed in its room, cannot get fast enough out of the external receiver, thro' so small a pipe; whilst the air remaining in the same, will yet retain a spring strong enough to break the glass. Thus, sometimes, when at the flame of a lamp, glass-bubbles are blown with slender stems; if they be suddenly remov'd out of the flame, they either break, if cool'd too fast; or are compress'd inwards, if they long retain the softness given them by fusion. For the air in the bubble, being exceedingly rarified, and expanded, whilst the glass is kept in the flame, and coming to cool hastily, when remov'd from thence, loses, upon refrigeration, the spring which the heat had given it; and so, if the external air cannot press in fast enough, thro' the too slender pipe, a sufficient quantity of air will not get in to resist the pressure of the atmosphere; and therefore, if this pressure find the bubble yet soft, it will press it a little inwards, and either

either flatten it, or make a dent, though the orifice of the pipe be left open.

11. We took a brass pipe, bent like a siphon, and fitted at the bigger end with a stop-cock, &c. and to the slender end of this, we fasten'd, with cement, the upper end of a cylindrical glass pipe, about fifty inches long, open at both ends, and having the lower plung'd into a vessel of stagnant quick-silver, whose upper superficies reach'd considerably higher than the immers'd orifice of the glass tube: then, causing the pump to be work'd, the air was, by degrees, drawn out of the siphon, and, consequently, out of the glass tube that open'd into it; and the stagnant mercury, proportionably impell'd up into the glass tube, till it had attain'd to its due height, which exceeded not thirty inches. And, then, tho' there remain'd in the upper part of the pipe, above twenty inches unfill'd, with quick-silver, we could not, by further pumping, raise it higher.

Mercury rises no higher by suction, than the weight of the atmosphere impels it.

Fig. 47.

Hence it appears, that the fancied power of nature, to prevent a vacuum, has its bounds; and those depending upon the specific gravity of the liquor, to be rais'd by suction. For, substituting, instead of the stagnant mercury, a basin of water; and, instead of the many strokes, in vain employ'd, to raise the quick-silver above the height just mention'd, making scarce one exsuction, which only, in part, emptied the siphon; yet the water, upon opening the stop-cock, was not only impell'd to the very top of the glass tube, but continu'd running, for a considerable time, thro' the siphon, and thence fell upon the plate of the engine: so that it appear'd strange to those, who knew not the reason of it, that the water should run very briskly, of its own accord, out of the leg of a siphon; which, perhaps, was not above a quarter so long as the other. I must not here omit, that tho' sometimes, in the *Torricellian* experiment, I have observ'd the mercury to stand at thirty inches, and, now and then, above it; yet the height of the mercury in our glass tube, appear'd not to reach full twenty-nine inches, and a quarter. But, consulting the barometer, I found the quick-silver at twenty-nine inches, and one eighth; which, probably, would have been the very height of that, rais'd by the engine, had it been freed from bubbles.

Hence we may conclude, that suction will elevate liquors in pumps, no higher than the weight of the atmosphere is able to raise them; since the closeness requisite in the pump of our engine, makes it very unlikely, that a more accurate suction can be effected by an ordinary pump.

Tho' the exhausting siphon, used in this experiment, may be easily conceiv'd by an attentive inspection of the figure; yet, because I frequently employ it in pneumatical experiments, 'tis proper to intimate, once for all, that though the bended pipe itself, may be, on some occasions, more conveniently made of glass, for the sake of transparency; yet, for the most part, we chose to employ pipes of brass, because the others are so very subject to break; that 'tis convenient to make the longer leg of the siphon, a little larger at the bottom, than the rest of the pipe usually needs

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to be, that it may the more commodiously admit the shank of a stop-cork, which is to be very carefully inserted, with cement; by seasonably turning and returning whereof, the passage between the engine and the vessel to be exhausted, is to be open'd and shut; and, lastly, that tho' we sometimes immediately apply the brass siphon to the engine by cementing the external shank of the stop-cork to the orifice of the little pipe, thro' which, the exsuction of the air is made; yet the bended pipe alone, is so apt to be loosen'd by the motion of the engine and the turning of the stop-cork, that, for the most part, we use a siphon consisting of a brass-pipe, a stop-cork, and a glass eight or ten inches high, and of some such shape as is exprell'd in the figure; for, by this means, tho' the exhaustion is longer in making, yet it is more securely and uninterruptedly carried on; because of the stability, which the breadth of the lower orifice of the glass gives to the whole instrument. Besides, not only the siphon is thus much lengthen'd, but we may commodiously place a gage in the glass part of this compounded siphon, to shew, from time to time, how far the air is drawn out of the vessel to be exhausted.

Liquors ascend
to different
heights by suction,
according to
their specific
gravities.

12. I caus'd to be made and inserted to the shorter leg of the above-mention'd siphon a short pipe, which branch'd itself equally to the right hand and to the left; so that I might exhaust two glass tubes, at the same time, and prevent any suspicion, that the engine was not equally applied to both. This additional brass pipe, being carefully cemented into the siphon, to each of its two branches were well fasten'd, with the same cement, a cylindrical glass of about forty two inches in length; the lower orifice of one of these glasses being immers'd in a vessel of stagnant mercury, and that of the other in a vessel of water; when care was taken, that as the tubes were chosen near of a size, so the surfaces of the two different liquors should be near of a height. This being done, we began to pump warily and slowly, till the water in one of the pipes was elevated about forty-two inches; and then measuring the height of the quick-silver in the other pipe above the surface of the stagnant mercury, we found it to be almost three inches, so that the water was about fourteen times as high as the quick-silver. And, to prosecute the experiment further, we very warily let in a little air to the exhausting siphon, and saw the two fluids proportionably descend; till turning the stop-cork, when the water was about fourteen inches high, we thereby kept them from sinking any lower, before we had measur'd the height of the quick-silver, which we found to be about one inch.

But, we observ'd, that the quick-silver, for the most part, seem'd to be a very little higher, than the proportion of one to fourteen required; and accordingly, I had long before, by particular trials, found, that, tho' fourteen and one be the nearest of small integer numbers, that express the proportion between the specific gravities of mercury and water; yet the former is not quite so heavy as this proportion supposes.

This experiment evidently shews that the fluids rose by the weight of the air, and leaves no pretence of a *Fuga vacui*. It may also be made useful to estimate the different gravities of liquors: for which purpose,

pose, I caus'd the afore-mention'd glass pipes, to have their ends plung'd, the one in fresh water, and the other in some impregnated with a large proportion of sea-salt; and found, that when the fresh water was rais'd to about forty-two inches, the saline solution had not fully reach'd to forty.

But, to make the disparity more evident, I prepar'd an unusual brine, by suffering sea-salt to dissolve in the moist air: and, having apply'd this liquor, and fresh water to the two pipes, and proceeded after the former manner; we found, that when the pure water was elevated to near forty-two inches, the liquor of sea-salt wanted about seven inches and one fourth of that height; and when the water was made to subside to the middle of its pipe, the saline liquor in the other pipe was between three and four inches lower than that. I also took fair water, and a liquor made of the salt of pot-ashes suffer'd to run *per deliquium*, and proceeding as before, found, that when the common water was about forty-two inches high, the solution wanted of thirty inches; and when the water was made to subside to the middle of its tube, the other liquor was between six and seven inches lower.

13. We took a strong glass bottle, that would contain above a pint, and having in the bottom of it lodg'd a convenient quantity of mercury, we pour'd on it a greater quantity of water; and providing two slender glass pipes, open at both ends, we so plac'd and fasten'd them close by cement, that the shorter of the pipes had its lower orifice immers'd beneath the surface of the quick-silver, and the longer reach'd not quite so low as that surface, and so was immers'd but in the water. This done, we convey'd the bottle into a proper receiver, and having begun to pump out the air; we took notice to what heights the quick-silver and water were impell'd up in their respective tubes, on which, we had before made marks; and found, that when the quick-silver was impell'd up to two inches, the water was rais'd to about twenty-eight; and when the quick-silver stood at about one inch, the water stood at about fourteen.

The heights where to water and mercury may be rais'd by the spring of the air.

14. We convey'd into a fitly shaped receiver two glass pipes very unequal in length; but each of them seal'd at one end: the shorter tube was fill'd with mercury, and inverted into a small glass jar, wherein a sufficient quantity of that fluid had been before lodg'd, the longer pipe was fill'd with common water; and inverted into a larger glass, which likewise contain'd a fit proportion of the same liquor. Then the receiver being closely cemented to the engine, the air was pump'd out for a pretty while before the mercury began to subside; but when it was so far withdrawn, that its pressure could no longer keep up a mercurial cylinder of that height, the quick-silver began to sink; the water in the other tube, tho' three times as long, still retaining its full height. But when the quick-silver was fallen to between three and four inches above the surface of that in the vessel, the water also began to subside; but sooner than according to the laws of Statics it ought to have done; because many aerial particles emerging from

And the heights where to they will subside upon withdrawing it.

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the body of the water to the upper part of the glass, by their spring concurr'd with the gravity of the water to depress this liquor. And so when the quick-silver was three inches above the stagnant mercury, the water in the pipe was fallen several inches beneath forty-two; and several beneath twenty-eight, when the mercury had subsided an inch lower. But after the pump had been ply'd, to free the water from the latent aerial bubbles, we let in the external air; and having thereby impell'd both the fluids up again into their pipes, and remov'd the receiver; we took them both out, to free them from the air, and fill'd each of them with a little of their respective stagnant liquors; then inverting them again into their proper vessels, we repeated the experiment, and found it to require more pumping than before, to make the liquors begin to subside: so that when the mercury was fallen to three inches, or two, or one, the water subsided so near to the heights of forty-two, twenty-eight, or fourteen inches, that we suppos'd the little differences which appear'd between the several heights of the quick-silver, and fourteen times as great heights of the water, proceeded from some aerial corpuscles yet remaining in the water, and, by their spring, when once they had emerg'd, promoting the depression of it.

The greatest height to which water can be rais'd by attraction, or sucking pumps.
Fig. 48.

15. Having procur'd several tin pipes above an inch in bore, very carefully solder'd together, to make one whole tube, about thirty-two feet long; and cas'd it over first with cement and then with plaister of *Paris*; we very carefully cemented a strong pipe of glass, between two and three feet in length to the upper part of it; and to the upper end of this pipe, by means of cement and a short elbow of tin, we very closely fasten'd another pipe of the same metal, consisting of two pieces making a right angle; whereof the upper part was parallel to the horizon, and the other, which lay parallel to the glass pipe, reach'd down to the engine that was placed on the flat roof of a house thirty feet high from the ground, and was to be cemented to the lower end of this descending part of the pipe, whose horizontal leg rested upon a piece of wood nail'd to the rails on the top of the building: the tube, also, was kept from shaking by a board fasten'd to the same rails, with a deep notch for it to be inserted in.

This apparatus being made, and the whole tube, with a pole to sustain it, erected along the wall, fasten'd there, and the descending pipe carefully cemented on to the engine; there was placed under the bottom of the long tube a convenient vessel, whereinto so much water was pour'd, as reach'd far above the orifice of the pipe; and providing, that the vessel might still be kept competently full, we, at length, rais'd the water to the middle of the glass pipe; but not without numerous bubbles, made by the air conceal'd in the pores of the water, which, for a time, kept a kind of foam upon the surface of it. And finding the engine, and tube as staunch as could be expected; I thought fit to try what was the utmost height, to which water could be elevated by suction: and therefore, tho' the pump seem'd to have been sufficiently ply'd already; yet, for further satisfaction, when the water was within a few inches of the top of the glass, I caus'd twenty exsuctions more to be suddenly made. And, having taken notice where

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where the surface rested, we measured the height of the cylinder of water, and found it thirty-three feet, and about six inches; the barometer then standing at twenty-nine inches, and between two and three eighths of an inch. Now, supposing the specific gravity of water, to that of quick-silver, as 1 to 14; the height of the water ought to have been thirty-four feet, and about two inches; that is, about eight inches more than we found it. But, then, I formerly noted, that the proportion betwixt mercury and water, is not altogether so great; and, therefore, in so tall a cylinder as ours was, the difference must be considerable. If, therefore, instead of making an inch of quick-silver, equivalent to fourteen inches of water, we abate a quarter of an inch; which is but a fifty-sixth part of the height of the water; this abatement, being repeated twenty-nine times and one quarter, will amount to seven inches, and above a quarter; which, added to the former height of the water, thirty-three feet, six inches, will make thirty-four feet, and above an inch: so that the difference between the height of the mercury, sustain'd by the weight of the atmosphere in the barometer, and that of the water, rais'd, and sustain'd, by the pressure of the same in the long tube, did not appear to differ more than an inch or two, from the proportion they ought to have, according to their specific gravity: nor could we, by obstinately plying the pump, raise the water higher.

This experiment, being soon repeated, in my absence, by Dr. Wallis, Dr. Wren, and Dr. Millington; they, presently after, assured me, that the greatest height, whereto they could raise the water, was thirty-three feet and a half: and, as it happen'd, within less than an hour before, I had observed the barometer to stand somewhat below twenty-nine inches, and three eighths; when, now, consulting the same instrument again, the mercury appear'd to be risen a little higher. Hence appears the impossibility of making water pass over the highest mountains, by the help of inflected pipes, and suction. For, if the water be to rise above thirty-five, or thirty-six feet, a sucking-pump will not, ordinarily, here in England, suffice for that purpose.

16. To try whether the air contributes to the elasticity of bodies, we took a piece of whale-bone, of a convenient length, and, having fasten'd one end of it into a thick heavy trencher, to be placed on the plate of the engine; to the other end we tied a weight, whereby the whale-bone was moderately bent, which reached down to a flat body, placed under it, so that if the spring were but a little weaken'd, the weight must either rest upon, or touch the horizontal plane; or if, on the other side, the spring should grow sensibly stronger, it might be easily perceived, by the distance of the weight, which was so near the plane, that a little increase of it must be visible. These things we convey'd into the receiver, and took care to shake the engine as little as possible, that the weight might not hit against the body which lay under it; or, we be hinder'd from discerning, whether it were depressed by the bare extraction of the air. And, when the air had been well pumped out, I watched attentively, whether any notable change, in the distance of the weight from the plane, would happen upon

An elastic body bent in the exhausted receiver.

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its being let in again; for the weight was then at rest: and the returning air, flowing in much faster than it could before be drawn out, this seem'd the likeliest time to discover, whether the absence of the air had, sensibly, alter'd the spring of the whale-bone. But, tho' the experiment were made more than once, I could only satisfy myself, that the depression, or elevation, of the weight, owing to the mere change of the spring, was not very considerable; for I do not think myself sure, that I perceived any at all: tho', sometimes, when the receiver was well exhausted, the weight seem'd to be a little depress'd; yet this, I thought, might well be ascribed to the absence of the air, not consider'd as a body that had any thing to do directly with the spring, but as a body that had some gravity; whereby it made the medium, wherein the experiment was try'd, contribute to support the weight that bent the spring; which weight, when the air was absent, must have its gravity increased, by as much weight, as a quantity of the exhausted air, equal to it in bulk, amounts to.

To make gages
for estimating
how far the re-
ceiver is ex-
hausted.

17. The air, being invisible, it is not always easy to know, whether it be sufficiently pumped out of the receiver, to be exhausted; we, therefore, thought it very convenient to have some instrument within the receiver, that might serve for a gage, or standard, whereby to judge when it was sufficiently evacuated. The first attempt, made to this purpose, was by means of a bladder, very strongly tied at the neck; after having had only so much air left in its folds, as might fully distend it, when the receiver was very well exhausted. And this way, in some cases, is useful; but, in others, a bladder takes up too much of the receiver, and hinders the objects from being observ'd on all sides.

Another sort of gage we made with quick-silver, pour'd into a very short pipe, which was, afterwards, inverted into a little glass of stagnant quick-silver, as in the *Torricellian* experiment. For this pipe, being but a very few inches long, the mercury in it would not begin to descend, till a very great proportion of air was pumped out of the receiver; because, till then, the spring of the remaining air would be strong enough to sustain so short a cylinder of mercury. And this kind of gage is no bad one. But, because it cannot easily be suspended, and the mercury in it is apt to shake, by the motion of the engine, another was substituted in its place, consisting of a kind of siphon, to the shorter leg whereof belong'd a large glass-bubble.

But none of these gages having the conveniences, that some of our experiments require; I devised another, after the following manner.

Fig. 47.

Take a cylindrical pipe of glass, six, eight, ten, or more inches in length, and not so thick as a goose-quill; and, by the flame of a lamp, melt it, but not too near the middle, and make it into a siphon; the legs whereof are to be parallel, and as near to each other, as possible. In one of these legs, usually the longer, leave at the top, either half an inch, or a whole inch, more or less, according to the length of the gage, or the design of the experimenter, of air in its natural state; and fill the rest of the longer leg,

leg, and as great a part of the shorter as shall be thought proper, with quick-silver. This done, there may be marks placed on the outside of the longer, or seal'd leg, whereby to measure the expansion of the air included therein.

This instrument, being convey'd into a receiver, and the air very diligently pumped out, notice must be taken, to what part of the gage the mercury is depressed, that we may know, when the mercury shall, afterwards, be driven so far, that the receiver, wherein the gage is placed, is well exhausted. And if it be desired to know, more accurately, what stations of the mercury, in the gage, are answerable to the degrees of the rarification of the air in the receiver; this may be gain'd, by letting in water, as often as is necessary, into a receiver, whose entire capacity is first measured; and in which there might be marks made, to shew when the water to be let in, shall have fill'd a fourth, a half, &c. of the cavity. For if, when the quick-silver in the gage, is depressed to a certain mark, you let in water, which appears to fill a fourth part of the receiver; you may conclude, that about one fourth of the air was pumped out; or that a fourth of the spring of the whole included air was lost. And if the water either falls considerably short of, or exceeds the quantity expected; you may, the next time, let in the water, either after the mercury has a little pass'd the former mark, or a little before it is arrived at it. And when once you have, this way, obtain'd one long, and accurate gage, you may divide others by the help of this, placed with them in a small receiver: when, the mercury in the former, being depressed to any determinate division, obtain'd by observation; you may, thence, conclude, how much the air, in the receiver, is rarify'd; and, consequently, by taking notice of the place where the mercury rests in the other gages, determine what degree of exhaustion, in a receiver, is denoted by that station of the mercury.

That leg of the gage which includes the air, may be seal'd up, either before the pipe is bent into a siphon; or, which is much better, by first drawing out that end of it you design shall be seal'd, to a short, and very slender thread: then, having made the tube into a siphon, pour into the leg, which is to remain open, as much quick-silver as you judge convenient, which will rise to an equal height in the other leg; and, by gently inclining the siphon, you may pour the superfluous mercury out of it, if there be any; and when there is an inch, or the proper space, unfill'd with mercury, next the end that is to be closed; and the rest of that leg, and as much of the other as is necessary, fill'd with quick-silver; you may, by keeping the siphon in the same posture, and warily applying the slender apex, above-mention'd, to the upper part of the flame of a lamp, blown horizontal, conveniently seal it up.

But there are some experiments, wherein it is not necessary that the receiver should be fully exhausted; but, rather, that the degrees of the air's rarification should be well measured. And, in many cases, we may use gages, shaped like those hitherto described, made as long as the receiver will admit, and furnish'd, instead of quick-silver, either with tinged spirit

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of wine; or else the tincture of red rose-leaves, drawn with common water, and heighten'd with a little spirit of vitriol. For the lightness of these liquors, in comparison of quick-silver, will allow the expansions of the air, included in the gage, to be very manifest; tho', perhaps, a quarter of the air be not pumped out of the receiver.

We may, also, in such cases, and where the receiver is sufficiently large, and not to be quite exhausted, make use of a mercurial gage, differing from the former in this, that the shorter leg need not be above an inch, or half an inch long, before it widens into a bubble, about half an inch, or an inch in diameter; and having, at the upper part, a very short and slender open pipe, whereat the air may get in and out: and here we need not include so much air as, otherwise, would be requisite, at the top of the longer leg; because the mercury, in the shorter, cannot, by reason of the breadth of the bubble, into which the expansion of the air drives it, be considerably raised; whereby the degrees of the included air's rarification become very visible.

An easy way to make the pressure of the air sensible to the touch.

18. I caused a hollow strong piece of brass to be made, two or three inches high, opening, at both ends, in orifices circular and parallel, but not equal; which, being cemented, as a small receiver, to the engine; whoever doubted the pressure of the air to be considerable, needed only lay the palm of his hand upon the upper orifice, and press it close thereto: for, upon withdrawing, by a single stroke, the greatest part of the pressure of the internal air, that, before, counter-balanced the external; the hand, being left alone, to support the weight of the atmosphere, would be press'd inwards very forcibly; especially, if, by a second stroke of the pump, the little receiver were farther exhausted: and this pressure continues, till the air be re-admitted into the receiver. If a more sensible conviction be desir'd, 'tis easy to give it, by turning the larger orifice uppermost, and proceeding, as before; but this ought not much to exceed two inches and a half in diameter, lest the great weight of the air should break, or considerably hurt the hand: as I once much endanger'd my own, thro' mistake of the pumper, who fell to his work, while I held it upon the orifice of a vessel too large in diameter.

Mercury subsiding in the Torricellian tube to a level with the stagnant, by exhausting the air.

19. A barometer being included in a receiver, made of a long bolt-head, with the lower part of the ball cut circularly off; upon the first exhaustion of the air, the quick-silver, that before stood at twenty-nine inches, would fall, and rest, at nine or ten inches; and, in about three strokes more, it would be brought quite down to the level of the stagnant quick-silver, and somewhat below: but the air, being let into the receiver, the mercury would be impell'd up slow, or fast, as we pleas'd, to the former height of twenty-nine inches.

If the air were suffer'd to go hastily out of the receiver, the mercury would, at the very first stroke, descend, till it reach'd within an inch or two of that in the vessel; tho' it would, presently after a few risings and fallings, settle at the height of nine, or ten inches, till the next stroke brought it down lower.

And

And if, when the mercury was re-impell'd up to its due height, instead of rarifying the air, it were a little compress'd; the quick-silver would be easily made to rise an inch, or more, above the former standard of twenty-nine inches.

We, also, took a glass-tube, seal'd at one end, much shorter than the due length, and having fill'd it with mercury, and inverted it into a vessel of stagnant mercury, we placed all in the former receiver; where the mercurial cylinder, for want of the requisite height, remain'd totally suspended; but, upon the first, or second stroke, subsided, and, after two or three more, fell to a level with the stagnant mercury, or a little below it: and, upon the letting in the air, it would be again impell'd to the very top of the tube, bating an aerial bubble, which seem'd to come from the mercury itself; and was so little, as not to be at all discernible, but to a very attentive eye.

20. Into a very large glass-tube, hermetically seal'd at one end, and about two feet and a half in length, we pour'd quick-silver, to the height of three or four fingers; then we took two cylindrical pipes, of very unequal bores, and open at both ends, and plung'd the lower ends of both into the quick-silver; fastening them to the former tube, that they might not be mov'd out of their posture; in which the convex surface of the mercury, in both, seem'd almost to lie in a level; the tube, also, being placed, perpendicularly, in a frame: then, by the help of a funnel, we pour'd water, by degrees, in at the top of the tube; and observ'd, that, as the water gravitated, more and more, upon the stagnant mercury; so the included mercury rose equally, in both the pipes; till the tube, being almost fill'd with water, the mercury appear'd to be impell'd, and sustain'd in both, at the height of about two inches above the surface of the stagnant quick-silver. And, having caus'd about half the water, in the large tube, to be suck'd out at the top; we observ'd the quick-silver, in both the others, to subside uniformly, and to re-ascend alike upon the re-affusion of the water.

In small and large open tubes, when no Fuga vacui can be pretended, the weight of water raises quick-silver to an equal height.

We, also, took a very wide tube of glass, a foot long, and pour'd into it a convenient quantity of quick-silver; then we took two pipes, of an equal length, but unequal bores, as before; and these, being fill'd with quick-silver, as in the *Torricellian* experiment, were let down into the tube, and unstopp'd, under the surface of the stagnant mercury: when, that in the pipes, falling to its wonted station, and resting there, we pour'd into the tube about a foot height of water, whereby the quick-silver appear'd equally impell'd above its station, and sustain'd there, in both the pipes; and, upon withdrawing some of the water, it began to subside alike, as to sense, in both: and water, being a second time pour'd down into the tube, the mercury, in both pipes, rose uniformly, as before. By which, and the former experiment, it appears, that a gravitating liquor, as air, or water, may impel, or sustain mercury, at the same height, in tubes of very different capacities; and that liquors balance each other, according to their altitude, and not barely according to their weight. For,

PNEUMATICS. in the last experiment, the additional cylinder of one inch of mercury, was manifestly rais'd, and kept up by the water incumbent on the stagnant mercury. And the same parcel of water counterpois'd, in the different pipes, two mercurial cylinders, which, though of the same altitude, were very unequal in weight.

The height whereat pure mercury, and mercury amalgamated with tin, will stand in barometers.

21. Amalgamating mercury with a convenient proportion of pure tin, that the mixture might not be too thick, we therewith fill'd a cylindrical pipe, seal'd at one end, and of a fit length; and then inverted it into a little glass, furnish'd with the like mixture. The event was, that the amalgam did not fall down to twenty-nine, but stop'd at 31 inches, above the surface of the stagnant parcel. Hence, it appears, that the height of the liquor, suspended in the *Torricellian* tube, depends so much upon its equilibrium, with the external air, that it may be varied as well by a change of gravity in the suspending liquor, as we formerly saw it might by an alteration in the atmosphere.

It might be worth while to try, by comparing the height of the amalgam to what it ought to be by the specific gravities of the mercury, and the tin mix'd in a known proportion, whether these metals penetrate each other, in the same manner as copper and tin have been observ'd to do; when being melted down together, they make a more close and ponderous body than their respective weights seem'd to require.

To make portable barometers.

Fig. 49.

22. We took a hollow cylinder of glass, seal'd at one end, and four or five feet in length; and, by the flame of a lamp, bent it after the manner of a siphon, one of the legs whereof is three or four times longer than the other; whence the shorter leg may serve, instead of the vessel, usually employ'd to contain the stagnant mercury. To fill this, take a small glass funnel, with a long and slender shank, so that it may reach three or four inches, or farther, into the shorter leg of the barometer; and, by the funnel, pour into the shorter leg, as much mercury as may reach about two or three inches, in both legs; then stopping the orifice with your finger, and slowly inclining the tube, the mercury, in the longer leg, will fall to the seal'd end, and the air that was there before, pass by, and give it room. The mercury, in the shorter leg, which ought to be held uppermost, will, by the same inclination of the tube, fall towards the orifice; but being, by the finger, kept from falling out, if you slowly erect the glass again, and then stop it, as before, the mercury will pass out of the shorter leg into the longer, and join with that which was there before: and if all the mercury do not so pass, the orifice is to be stop'd again with the finger, and the tube inclin'd as formerly. This done, the tube is to be erected, and, by the help of the funnel, more mercury is to be pour'd in; and the same process of stopping the orifice, inclining the tube, &c. is to be repeated, till all the mercury, pour'd into the shorter leg, be brought to join with that in the longer; and then the open leg is to be furnish'd with fresh mercury; observing that the nearer the longer leg comes to being fill'd, the less you must raise it, from time to time, when you pour mercury into the shorter; as also, that when the longer leg is quite

quite full of mercury, you need not pour in any more, if the longer much exceed a yard; because, upon erecting the tube, there will subside, from the taller leg into the other, a considerable quantity of mercury. And to free it from bubbles, you must, once more, stop the orifice with the finger, and incline, and re-erect the tube several times, till you have thereby brought most of the smaller bubbles into a single large one; then making this pass leisurely, two or three times, from one end of the tube, to the other, it will unite all the small bubbles to itself: and this may, afterwards, by one inclination more of the tube, be made to pass into the shorter leg, and thence into the free air.

But there is another sort of funnels, with which, if skilfully used, the bended tubes of our portable barometers, may be very expeditiously fill'd. For, if the slender part of the funnel be bent in an obtuse angle, and so long, that the part which is to go into the shorter leg of the siphon, may reach to its flexure; you may, by holding the tube so, that the sealed end be somewhat lower than the other, and by pouring in mercury at this obtuse end of the angular funnel, easily make it run over the flexure, into the longer leg of the siphon; provided you, now and then, as occasion requires, erect, and shake the tube, to help the mercury to get by the air, and expel it.

We accomplish'd another part of our design, by means of a piece of wood, somewhat longer than the tube, and considerably broader in the lower part, than in the upper, to receive the shorter leg of the siphon. In such a piece of wood, which was about an inch thick, we caus'd such a channel to be made, that our siphon might be placed in it so deep, that a flat piece of wood might be laid on it, without touching the glass; so that this piece of wood may serve for a cover to defend the glass, to be put on when the instrument is to be transported; and taken off again, when 'tis to be hung up for observation; the channel'd piece of wood serving both for part of a case, and for an entire frame; which may, for some uses, be a little more commodious, if the cover be join'd to the rest of the frame, by two or three little hinges, and a hasp, whereby the case may be readily open'd and shut, at pleasure.

The third thing we propos'd, is not so easy as the second; nor have we yet had opportunity to try whether the way we made use of, will hold, if the barometer be transported into very remote parts; tho', by smaller removes, we found reason to hope 'twill succeed in greater.

The grand difficulty was, to prevent the spilling of the mercury; for, the upper part of the tube being destitute of air, if the quick-silver, by the motion of the instrument, be made to vibrate, it will hit so violently against the top of the glass, as to break it. To obviate this inconvenience, we incline the tube, till the mercury be impell'd to the very top of it; when yet there will remain a competent quantity in the shorter leg of the glass, if that be not too short; then the remaining part of the shorter leg, is to be fill'd up either with water, or mercury, and the orifice of it very carefully stop'd with cement: by this means, the mercury in the longer leg, having no room to play, cannot strike with violence against the top of the glass.

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When the instrument is to be transported, the height of the mercurial cylinder being taken for that place, day, and hour, and compared with that of another good barometer, which is to continue in the same place; as much of the channel, as is unpossess'd by the glass, may be stuffed with cotton, or the like; and some of the same matter may be put between the rest of the frame, and the cover, which ought to be well bound together. And when the instrument is arrived at the place design'd, the water, that is added, may be taken off again, by pieces of sponge, linen, &c. but, if instead of water, mercury be employ'd, it ought to be taken out, till you have just the weight that was put in. The chief use of this barometer is, by keeping a diary of the heights of the mercury herein, and comparing them with those in the barometer, that was not remov'd, to discover the agreement, or difference of the weight of the atmosphere, in distant places. The structure of this instrument, also, fits it to be securely let down into wells, or mines; to be drawn up to the top of towers, and other elevated places; and, perhaps, by a convenient addition, such barometers may shew very minute alterations of the atmosphere's pressure.

Whether this barometer, furnish'd, at its upper end, with a ball and socket, and at the lower, with a great weight, may be serviceable at sea, notwithstanding the rolling of a ship, I have not try'd; but it may, at least, be apply'd in flat calms, to shew the weight of the atmosphere, in different climates, upon the sea; which may, perhaps, prove useful to navigators, by enabling them to foretel the end of the calm. Besides, having one of these instruments ready, whenever they come on shore, they can presently take notice of the gravity of the atmosphere, in that place; and this, perhaps, compared with other observations, may, in time, help them to guess where they are, and to foresee some approaching changes of weather.

Mercury in a barometer, will be kept suspended higher at the bottom than at the top of a hill.

23. Two persons, whom I employ'd, found the mercury, in a portable barometer, fall a little, as they ascended a hill; at the top whereof they let the fluid settle, and carefully noted the place whereat it rested, which was one quarter of an inch beneath its former station; tho' the hill was not high, and the air and wind seem'd, to them, much colder at the top, than below. And as they descended, they observed that the mercury rose gradually.

The weight of the air will sustain the mercury in the barometer, tho' it press thereon but at a very small orifice.

24. Take the bent tube, mention'd in the twenty-second experiment, and inclining it, till the greatest part of the mercury pass from the shorter leg into the longer, the upper-end of the shorter leg, may, by the flame of a lamp, be drawn out so slender, that its orifice shall not be above an eighth, or tenth part as big as 'twas before. This being done, and the tube erected again, if the tall cylinder of mercury be of the usual, or former height, as we found it, 'twill appear that the weight of the external air may press as much upon the stagnant mercury, thro' a little hole, as when all the upper superficies of that mercury, was directly exposed to it.

Both an oblique pressure of the atmosphere, and the spring of a little included air, will sustain the mercury in the barometer.

25. If, instead of drawing the shorter leg of our siphon directly upwards, or parallel to the longer, as in the foregoing experiment, you bend off the slender

der part, so that, were it continued, it would make a right angle with the longer leg of the siphon, or an acute one, tending downwards; and when the tube is erected, the mercury rests at its usual station; 'twill appear, that the pressure of the atmosphere, may be exercis'd upon it as well obliquely, when the pipe that conveys it, is either horizontal, or opens downwards.

And, if instead of bending this slender pipe, you seal it up hermetically, *Fig. 51.* the continuance of the mercurial cylinder, at the same height, will shew, that the spring of a very little air, shut up with the pressure of the atmosphere upon it, is able to support as tall a cylinder of mercury, as the weight of that part of the atmosphere, which can come to exercise its pressure against the mercury.

If, when the shorter leg of the barometer is sealed, you move the instrument up and down, the mercury will vibrate, by reason of the yielding spring of the imprison'd air; but, because of the resistance of the spring, the motion will be diversified after an odd manner; which may be easily perceiv'd by the impression it makes upon the hand, but not so easily described. And as, when the shorter leg is drawn out slender enough, after the instrument is furnish'd with quick-silver, 'tis easy to seal it up with the flame of a candle, without the help of any instrument at all; I might here observe, that it may, on some occasions, be convenient to seal up the barometer, before it be transported; and, in some cases, to incline the tube before-hand, till the quick-silver have quite fill'd the longer leg: for by this means, the vibrations of the quick-silver will be less; and 'tis easy, when the instrument is brought to the design'd place, to break off the slender apex of the shorter leg, and so expose, again, the mercury to the pressure of the atmosphere.

Having caus'd a portable barometer to be made, with the shorter leg of a more than ordinary length; I afterwards, caus'd the upper part of this leg to be drawn out very slender; and lastly, the same to be, about the middle, bent downwards, so that the small orifice of the slender apex, pointed towards the ground; when neither I, nor some others, took notice that the mercury stood lower than in ordinary barometers: whence we concluded, that the atmosphere could press, not only at a very small orifice, but, when the air must, at this little orifice, tend upwards, to press upon the surface of the stagnant mercury.

26. When it appear'd, by a good barometer, that the atmosphere was considerably heavy, I caus'd a glass pipe, hermetically sealed at one end, and in length about two feet and a half, to be fill'd with quick-silver; except a very little part, wherein some drops of water were put, that we might the better discern the bubbles, if any should be left, after the inversion of the tube into an open glass, containing stagnant mercury. Having, by this means, freed the tube from bubbles, we so order'd the matter, that the quick-silver, and the little water about it, exactly fill'd the tube, without leaving any visible interval at the top; and yet the mercurial cylinder was but very little higher than that of our barometer at that

To make a barometer useful but at certain times.

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that time. Then the pipe was left erected in a quiet place, where the liquors retain'd their former height for several days. A school-philosopher would confidently have attributed this sustentation of so heavy a body, to nature's dread of a vacuum; but either she is not always equally subject to that fear, or some other cause of the phenomenon must be assign'd: for, when, long after, I had observ'd, by the barometer, that the atmosphere was grown much lighter than before, I found the quick-silver, in the short tube, considerably subsided; leaving a cavity at the top, which afterwards grew less, as the atmosphere became heavier.

The ascent of
liquors in very
slender tubes
in vacuo.

27. Some spirit of wine, ting'd with cochineal, being put into the receiver, and the air withdrawn, it bubbled exceedingly for a considerable time. Then, little hollow pipes, of different sizes, were put into it, when the red liquor ascended higher in the more slender, than in the others; but upon extracting the air, there scarce appear'd any sensible difference in the heights of the liquor, nor upon the letting it in again.

Afterwards, two such tubes, of different sizes, being fasten'd together with cement, were let down into the same spirit of wine, when the receiver was well exhausted: notwithstanding which, the liquor ascended in them, for ought we could plainly see, after the ordinary manner; only when the air was let in again, there seem'd to be some little rising, at least in one of the tubes.

In this experiment, tho' there appear'd no bubbles at all in the spirit of wine in the vessel, yet, for a considerable time, there arose bubbles in that part of the liquor which was got into the slender pipes.

A spontaneous
ascent of water
in a tube filled
with a compact
body.

28. I took a strait pipe of glass, open at both ends, and of a moderate bore; and having tied a linen rag to one end of it, that the water might have free passage in, and the powder not be able to fall out, we carefully fill'd the cavity with minium; and then having erected the tube, so that the bottom of it rested upon that of a shallow, open-mouth'd glass, containing water enough to rise an inch or two above the bottom of the tube, it insinuated itself, by degrees, into the cavity thereof, as appear'd by a little change of colour in that part of the minium which it reach'd; till the open glass being, from time to time, supplied with fresh liquor, it attain'd to the height of about thirty inches.

Taking, afterwards, another tube, and some minium, carefully prepared, I prosecuted the experiment, so as to make the water rise in the pipe about forty inches above the surface of the stagnant water.

Making the experiment with beaten glass, pieces of sponge, putty, &c. I did not find any of them succeed so well as the minium. Ting'd liquors, as ink, tincture of saffron, &c. seem'd not to rise near so high as water; as if the dissolved ingredients gradually choaked the pores of the minium.

To have the grains of our powder more minute, and the intervals between them smaller, I chose the best sort of minium, sifted it very fine, and so put it, by little and little, into the tube; that by ramming it, from time to time, it might be made to lie the closer: and this method suc-

ceeded

ceeded well. 'It seem'd, by a trial or two, that if the tube were very slender, the experiment would not succeed.

It may be worth while to observe, in what times the water ascends to certain heights; for, at the beginning, 'twill ascend much faster than afterwards, and sometimes continue rising for thirty hours, or longer.

One end, propos'd in this experiment, is, to discover a mistake in the modern explication of filtration; which supposes, that the parts of the filtre, which touch the water, being swell'd, by the ingress of it into their pores, are thereby made to lift up the water, till it touch the higher parts of the filtre; by which means, these being also wetted and swell'd, raise the water to the other neighbouring parts of the filtre, till it have reach'd to the top of it, whence its own gravity makes it descend: but, in our case, we have a filtre made of solid, metalline corpuscles; where 'twill be very hard to shew, that any such intumescence is produced, as this explanation requires.

Water ascends so few inches, even in very slender pipes, that the rise of the sap in trees, seems hardly accountable for, from the same cause. In the last trial, above-mention'd, I made water to ascend above three feet and a half: and, if by so slight an expedient, water may be rais'd as high as is necessary for the nutrition of some thousands of plants; for such a number there is, that exceed not three feet and a half, in height; I ask why nature may not have us'd other contrivances, to make liquors ascend to the tops of the tallest trees; especially, since besides heat, and something equivalent to valves, &c. many other things, perhaps not yet dreamt of, may probably concur to the effect?

As formerly, by bending these slender pipes, we made short siphons, thro' which the water would run, without being at first assist'd by suction; so I try'd whether I could, in larger pipes, make much longer siphons, by the help of minium. But tho', when the orifices pointed upwards, fine minium were ramm'd into both the legs, and both the orifices clos'd, yet, when they came to be again turn'd downwards, the weight of the minium would make some such discontinuation, as to hinder the farther progress of the water. This impediment, however, I judg'd superable, but had no opportunity to prosecute the experiment.

29. Having in shallow, wide-mouth'd glasses, expos'd a strong solution of common sea-salt, or of vitriol, to the air, which reach'd not, by some inches, to the tops of the vessels; and, having suffer'd much of the aqueous part to exhale very slowly; the coagulated salt, at length, appear'd to have lined the inside of the glasses, and to have ascended much higher than where the surface of the remaining water then rested; or the part whereto the liquor reach'd, when 'twas first pour'd in. And if the experiment were continued long enough, I sometimes observ'd this ascent of the salt, to be of some inches; and that the salt did not only line the inside of the glass, but getting over the brim of it, cover'd the outside, also, with a saline crust; so that, considering what a little liquor remain'd in the glass, 'twas surprizing how it could possibly get thither. Other salts, also, besides these

The spontaneous ascent of salts along the sides of glasses.

Pneumatic.

mention'd, will exhibit the same phenomenon. The cause of this odd effect may be referr'd to that of the ascent of liquors in pipes.

I observ'd in water, and aqueous liquors, that part of the surface next the sides of the glass, to be sensibly more elevated, than the rest of the superficies: and if very minute clippings of straw, or other small and light bodies, floating upon the water, approach near enough to the sides of the glass, they will be apt to run up, as it were, this ascent of water, and rest against the sides of the glass.

We may, also, observe, that sea-salt usually coagulates at the top of the water, in small and oblong corpuscles; so that, as to these, 'tis easy to conceive, how numbers of them may fasten themselves a-round the inside of the glass. And besides sea-salt, I have found several others, which, if their solutions be slowly evaporated, will, whilst yet there remains a large proportion of liquor, afford saline concretions at the top of the water. And the fastening of saline particles to the sides of the glass, may, perhaps, be promoted by a coldness, communicated by corpuscles contiguous to the glass; because the glass may be suppos'd more cold, upon account of its density, than water: but by the evaporation of the aqueous parts of the solution, the surface of the remaining liquor must necessarily subside; and those saline particles that were contiguous to the inside of the glass, and the more elevated part of the water, having no longer liquor enough to keep them dissolv'd, will be apt to adhere to the sides of the glass; and upon the least farther evaporation of the water, become a little higher than the greater part of the superficies of that liquor: whence, by reason of the little inequalities, that will be on the internal surface of the adhering corpuscles of the salt; and perhaps, also, on the internal superficies of the glass; there will be intercepted between the salt and the glass, little cavities, into which the water, contiguous to the bottom, will ascend, or be impell'd by the same power that raises it in slender pipes. And when the liquor is thus got to the top of the salt, and lies expos'd to the air; the saline part may, by the evaporation of the aqueous, be brought to coagulate there; and consequently, to increase the height of the saline film, which, by the like means, may, at length, reach to the very top of the glass; and thence it may easily be brought over to the outside of the vessel, where the natural weight of the solution will facilitate its progress downwards: whence the pellicle of salt, together with the contiguous surface of the glass, may, at length, constitute a kind of siphon.

Thus I have usually observ'd the saline film to be very easily separable from the glass in large flakes; which argues, that they did not stick close to one another, except in a few places; but had a thin cavity interpos'd between them, thro' which the water might ascend.

Nor is it repugnant to this explanation, that in case the water ascended, it should dissolve the salt; (for the liquor being already upon the point of concretion, it is so saturated with salt, that it can dissolve no more. Whence we may also see, why, when the saline film reaches to the outside of the glass, the liquor does not run down to the bottom, but coagulates

by

by the way. And I have suspected, that when the concretion is once began, the film may be raised, and propagated, not only by the motion of the liquor between the inside of that and the glass; but by the same liquor insinuating itself on the outside of the film, into the small interstices of the saline corpuscles; as ink rises into the slit, and along the sides of the nib of a pen, though nothing but the very point touch the surface of the liquor. And, by this means, the impregnated solution may, as it were, climb up to the top of the saline concretion, and, by coagulating there, add to its height.

30. Having caus'd a cylindrical piece of brass to be very carefully turn'd, of an inch in diameter, three inches in length, and open at both ends; to one of these ends we exactly fitted a flat bottom of the same metal, and fasten'd it very close with little screws on the outside. *To estimate the gravity of cylinders of the atmosphere in known weights.*

This instrument, being balanc'd in an exact pair of scales, was carefully fill'd with pure mercury, which we found to weigh one hundred thirty-seven drams, and forty-five grains; and multiplying that by ten, there will arise, for the weight of a mercurial cylinder of one inch in diameter, and thirty inches in height, about fourteen pound, two ounces, and three drams, troy.

The weight of a mercurial cylinder in an equilibrium with the atmosphere, and of an inch in diameter, being thus settled, we may easily compute the weight of a cylinder of quick-silver of another diameter, and consequently the force of the pressure of an atmospherical column of the same diameter. For, since cylinders of equal heights are to one another, as their bases; and the bases of cylinders to each other as the squares of their diameters; and lastly, since we here suppose mercury a homogeneous body; the mercurial cylinders will be to each other in weight, as they are in bulk: if then, for instance, we would know the weight of a cylinder thirty inches high, whose diameter is two inches, the rule is this: as the square of the diameter of the standard cylinder, whose weight is known, to the square of the diameter of the cylinder propos'd; so is the bulk of the former to the bulk of the latter, and the weight of that to the weight of this. Thus the square of one inch, the diameter of the standard cylinder, being one, and the square of two, the diameter of the cylinder given, being four; the bulk or solid content of this latter cylinder, and consequently its weight, will be four times as great as those of the standard cylinder.

31. We took a small vigorous load-stone, cap'd and fitted with a loose plate of steel, so shaped, that when sustain'd by the stone, we could hang at a little crook that came out of the midst of it, and pointed downwards, a scale; into which, we put weights; and then, by shaking the load-stone, as much as we guess'd it would be by the motion of the engine, we found the greatest weight, that we presum'd it would support, notwithstanding the agitation whereto 't would be expos'd, was, besides the iron plate and the scale, six ounces troy: and, if we added half an ounce more, the whole weight appear'd too easy to be shaken off. This done, we hung the load-stone with all the weight it sustain'd, at a button of glass fasten'd to the top of *The attractive virtue of the lead-stone in an exhausted receiver.*

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the inside of the receiver, when 'twas first blown; and, tho' in about twelve exsuctions we usually emptied such receivers, as much as was requisite for most experiments; yet, this time, we made above twice that number: when, violently shaking the engine, without thereby shaking off the weight that hung at the load-stone, the iron seem'd to be very nearly as firmly sustain'd by it, as before the air began to be pump'd out; for the extraction of the air, tho' it be not suppos'd to weaken the precise power of the load-stone; yet, it must lessen its power to sustain the steel, because this in so thin a medium must weigh heavier than in the air.

The pressure of the external air being taken off, the sucker of a syringe is easily drawn up, tho' the lower orifice be stopp'd.

Fig. 52.

32. We took a brass syringe, the barrel about six inches in length, and the diameter about an inch and three eighths; and having, by placing a thin bladder about the sucker, and pouring oil into the barrel, made the instrument stanch, whilst the sucker mov'd without much difficulty; we thrust this to the bottom of the barrel to exclude the air; and having laid aside the slender pipe of the syringe, we carefully stopp'd the orifice to which the pipe, in these instruments, is usually screw'd; then drawing up the sucker, we let it go, to judge, by the violence with which it would be driven back again, whether the syringe were fit for our purpose; and finding it to be so, we fasten'd a ponderous piece of iron to keep it down; and then fixing to the handle of the rammer one end of a string, whose other end was ty'd to the turn-key, we convey'd this syringe and the weight belonging to it, into a receiver; and having pump'd out the air, we began to turn the key, thereby to shorten the string that ty'd the handle of the syringe to it, and found no resistance in drawing up the sucker from the bottom of the cylinder.

And repeating the experiment with the like success, when the receiver being exhausted, we had drawn up the sucker, almost to the top of the barrel by a weak string, we kept the parts of the syringe in that posture, till a passage was open'd to the outward air; upon which, the sucker was so forcibly depress'd, that it broke the string, and was violently driven back to the lower part of the barrel; tho' the string had sustain'd between four and five pound weight, and broke long before all the air, that flow'd in to fill the receiver, had found entrance.

Again, we took the same syringe, and having found it tight enough for our purpose, we carefully clos'd the vent with a cork and cement, and having ty'd a weight of two pound two ounces to the barrel, we suspended the rammer of the syringe, by a string, in a large receiver; and causing the pump to be ply'd, we made eleven or twelve exsuctions, without finding any appearance of change in the syringe: but causing the pumping to be continued, I perceiv'd, within two or three exsuctions more, the cylinder began to be drawn very slowly down, by the weight hanging at it; and likewise try'd, that, just upon a fresh exsuction, the descent would be manifestly accelerated. And, when we had suffer'd the barrel and weight to slide down as far as we thought fit, we let in the external air, which rais'd them both again, much faster than they had subsided.

And,

And, substituting a far heavier weight for the former, the depression of the barrel of the syringe succeeded for two or three times, successively, much sooner than before.

33. Having cemented up the hole at the bottom of the syringe, we ty'd to the barrel a hollow piece of iron, that serv'd for a scale; into which we put weights, successively, to try if, when the sucker was forcibly drawn up, and held steddily, in its highest station, the weight, fasten'd to the barrel, which was held down whilst the sucker was drawn up, and, afterwards, let go, would be considerably rais'd. And, when we perceiv'd, that the addition of half a pound, or a pound, more, would make the weight too great to be so rais'd; we forbore to put in that increase of weight: and, having ty'd the handle of the rammer to the key, we convey'd the syringe, together with its clog, into a receiver; out of which, a convenient quantity of air being pump'd, we were, thereby, easily enabled to draw up the sucker, without the cylinder: after which, having let in the air, so that the weight was rais'd a little, I caus'd two pound to be taken out; and then the receiver, being somewhat exhausted, and the air admitted; the clog, which amounted to about sixteen pound, was swiftly rais'd, and, as it were, snatch'd up from the middle, to the upper part of the rammer.

A syringe causing the pressure of the air to raise a considerable weight.

Fig. 52.

34. We took a small receiver, shaped like a pear, cut off, horizontally, at both ends; we, also, took the syringe, formerly mention'd, and, having cemented thereto, instead of its own brass-pipe, a small pipe of glass, about half a foot in length, we put this syringe in at the narrow end of the receiver; to whose orifice was, afterwards, carefully cemented the brass-cap, with the key, whereto we ty'd the handle of the rammer: then, having conveniently placed, upon the engine, a very short thick conical glass, containing a sufficient quantity of quick-silver; we set the receiver over it, so that the lower end of the pipe of the syringe reach'd almost to the bottom of this glass; and, consequently, was immers'd far beneath the surface of the quick-silver: when, all things being prepared, the air was pump'd out of the receiver, and, consequently, out of the little glass that held the mercury; the sucker being warily drawn up; we could not see the quick-silver ascend to follow it; but the air, being let slowly into the receiver, the mercury was quickly impell'd up to the top of the glass-pipe.

The ascent of liquors in syringes owing to the pressure of the air.

Fig. 53.

And, for farther satisfaction, when the experiment was repeated, we plainly observ'd, that tho', when the receiver, being not yet exhausted, the sucker was drawn up but one inch, the mercury would be rais'd to the upper part of the glass-pipe of the syringe; yet, after the exhausting of the receiver, tho' the sucker was drawn up twice as high, there appear'd no ascent of the mercury in the pipe.

To confirm this experiment, we caus'd the syringe to be ty'd fast to a ponderous body, that might keep the cylinder unmov'd, when the sucker should be drawn up; we, also, cemented to the vent, or screw, at the bottom of the syringe, a pipe of glass, about two inches long; and, having placed

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placed the heavy body upon a pedestal of a convenient height, that the glass-pipe might be all seen beneath it; and a very low vial, almost fill'd with quick-silver, might be so placed underneath the pipe, that the stagnant mercury reach'd far above the immers'd orifice of the said pipe: when things being thus provided, and the handle of the rammer ty'd to the key, belonging to the brass-cover of the receiver, this vessel was cemented to the engine, and exhausted.

We then look'd upon the glass-pipe, above-mention'd, and, being able to see thro' it, we, by the string, drew up the sucker to a considerable height, but could not perceive the pipe to be fill'd with any succeeding mercury; but, warily letting in some air, we quickly saw the mercury impell'd to the very top of the pipe; and concluded, from the quantity rais'd, that some was, also, driven into the cavity of the cylinder. This experiment, also, we successfully try'd with tinged spirit of wine. Hence it appears, that, if a syringe were made use of above the atmosphere, neither the stopping of the pipe would hinder the easy drawing up of the sucker; nor the drawing up the sucker, tho' the pipe were not stopp'd, raise, by suction, the liquor wherein the pipe was immers'd.

The adhesion of cupping-glasses depends upon the pressure of the air.

Fig. 54.

35. We took a glass, about an inch and a half in diameter, but much longer than an ordinary cupping-glass of that breadth; we, also, provided a receiver, shaped like a pear, and open at both ends, at the sharper whereof, there was a small orifice; but, at the obtuser, a short neck, wide enough to admit the cupping-glass, without touching the sides of it. The smaller orifice of the receiver, being cemented to the engine, I caus'd the cupping-glass to be well fasten'd, with the mouth upwards, to the palm of a person's hand; then caus'd him to put it into the receiver, and lay his hand so upon the orifice, that it might serve for a cover to it, and hinder any air from getting in between them: but, upon the first suck, the cupping-glass fell off; the weight of the atmosphere pressing so strongly upon the person's hand, that he complain'd, he could very hardly take it from the glass, into which it was almost thrust. We repeated the experiment, fastening the cupping-glass more strongly than before; the tumour, occasion'd whereby, was very visible: but now, also, as before, at the very first turning of the stop-cock, to let the air out of the receiver, the cupping-glass fell off.

A great weight rais'd by a cupping-glass without heat.

Fig. 55.

36. We took the brass-ring, formerly mention'd, and cover'd it with a wet bladder, which was so ty'd on, that the bottom of the bladder cover'd the upper orifice of the ring, and lay stretch'd upon it, whilst the neck of the bladder was ty'd with a string, near the middle of the lower orifice of the ring; and, in this lower part of the bladder, we made two or three small holes, for the air to pass in and out at: then, having placed, at the bottom of our capp'd receiver, a thick piece of wood, perforated to receive the neck of the bladder; we placed the cover'd ring upon this piece of wood, so that the upper part of the bladder lay parallel to the horizon; then we suspended, at the key belonging to the cap of our receiver, a blind glass-head, instead of a cupping-glass, which name it may bear; and

and to the upper part of this glass, we fasten'd a large ring of metal to press it against the bladder. The receiver being now cemented on to the engine, we, by the help of the key, let down the cupping-glass till it almost touch'd the level superficies of the bladder; and when the receiver was moderately exhausted, we let down the cupping-glass a little lower, so that it rested upon the bladder, and touch'd it with all the parts of its orifice; whence the cupping-glass with its subjacent bladder was become an internal receiver wherein the air was considerably expanded. Then we warily let the air into the receiver, and thereby the air that surrounded the cupping-glass or internal receiver, having now a stronger pressure than that in the cupping-glass could resist, the bladder on which the cupping-glass rested, was considerably thrust into the cavity of the glass, and made to stick very close to the orifice of it.

Repeating the experiment, and exhausting the receiver further than before, we took out the cupping-glass and the bladder, which, together with the included brass-ring was hanging at it; and having ty'd the glass to the hook of a statera, and a large scale to the neck of the bladder, we put weights, by degrees, into the scale, till we thus forced off the bladder from the glass; which hapned not till the weight amounted to thirty-five pound.

37. We caus'd a pair of bellows to be made different from ordinary ones, their boards being circular, without handles, and without clack or valve, the nose but an inch long, to be lengthned, if occasion required, with a pipe, and about six inches in diameter, the leather being limber; so that when the bellows were open'd to their full extent, by drawing up the upper basis at a button purposely made in the midst of it, they resembled a cylinder sixteen or eighteen inches high; but there was some little, and unperceiv'd leak in them, whereat air had passage, when the nose was accurately stopp'd; however, if we drew up the upper basis from the lower, the external air would, on all sides, press the leather inward, and render the shape of the instrument very far from cylindrical. Then carefully stopping the nose, after we had brought the bases to touch each other, and conveying the instrument into a large receiver, it quickly appear'd, when the pump was set on work, that, at every exsuction, the air in the folds of the leather, and the rest of the little cavity left between the bases, made the upper of them manifestly rise; tho' its own weight would soon after depress it again, either by driving out some of the air, where the instrument was not sufficiently tight, or by making it, as it were, strain thro' the leather itself: and if the pump were ply'd faster than ordinary, the upper part of the bellows, would be soon rais'd to a considerable height; as appear'd more evidently, if we hastily let in the external air, whereby the bases would be clapt together, and the upper of them considerably depress'd; so that the imperfection of the bellows render'd the experiment rather more than less conclusive: for since there was no external force apply'd to open them, if, notwithstanding some of the included air could get out, the spring of the internal air was strong enough to open the

Fig. 56.

Bellows, with the nose stopp'd, open of themselves, when the pressure of the air is taken off.
Fig. 5.

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An attempt to
examine the hy-
pothesis of æther,
as to its exist-
ence.

the bellows, when the ambient air was withdrawn, much more would the effect have been produced, if the bellows had been perfectly stanch.

38. Since, if there be such a thing as a celestial matter, or æther, it must compose far the greatest part of the universe known to us; it deserves to be enquir'd, whether we can, by sensible experiments, discover its existence, or qualities. To this end I thought our pneumatical engine might contribute, if I could manage therein such a pair of bellows as I design'd; for I propos'd to fasten a convenient weight to the upper basis, and clog the lower with another, able to keep it horizontal, and immoveable, so that when, by the help of the turn-key, the upper basis should be rais'd to its full height, the cavity of the bellows might be brought to its full dimensions. This done, I intended to exhaust the receiver, and, consequently, the bellows, thus open'd; so that both the receiver, and they, might be carefully freed from air: after which, I purpos'd to let go the upper base of the bellows, that being hastily depress'd by the incumbent weight, it might suddenly fall down to the lower; and by thus greatly lessening the cavity, expel thence the matter, if any there were, before contain'd in it; and that, if it could, by this way, be done, at the hole of a slender pipe, fasten'd either near the bottom of the bellows, or in the upper basis, against, or over the orifice of which pipe, there might be placed, at a convenient distance, either a feather, or the sail of a little wind-mill, made of some other light body, fit to be put into motion by the impulse of any matter which should be forc'd out of the pipe.

Now, if by this means, notwithstanding the absence of the air, it should appear, that a stream of other matter, able to set visible bodies in motion, should issue out at the pipe of the compress'd bellows, it would also appear, that there may be, plentifully, found a much subtler body than common air, in places deserted by such air; and that it is not safe to conclude, from the absence of the air, in our receivers, and the upper part of the *Toricellian* tube, that there is no body, but an absolute vacuity. But if, on the other side, there should appear no motion at all to be produc'd, so much as in the feather, it should seem, that either the cavity of the bellows was absolutely empty; or that it would be very difficult to prove, by any sensible experiment, that it was full. And if, by any other means, it be demonstrable, that it was replenish'd with æther, we might suppose, from our experiment, that 'tis not easy to make it sensible by mechanical experiments; and that 'tis really so subtle, and yielding a matter, as does not either easily impel light bodies, or sensibly resist, like air, the motions of other bodies thro' it; but is able, freely, to pass the pores of wood, leather, and closer substances, which the air, in its natural state, doth not.

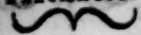
And, to make the trial more accurate, I caus'd a small pair of bellows to be made with a bladder; and that this might remain entire, we glued the two bases, the one to the bottom, and the other to the opposite part thereof; so that the neck came out at a hole, purposely made for it, into the upper basis; whence, into the neck it was easy to fix what pipe we judg'd fit. We had, also, thoughts of procuring another pair of tight bellows,
made



made with a very little clack in the lower basis; that, by hastily drawing up the other basis, when the receiver and bellows were very carefully exhausted, we might see whether the subtil matter that was expell'd by the upper basis, in its ascent, would, according to the modern doctrine of the circle, made by moving bodies, be impell'd up, or not.

We, likewise, thought of placing the little pipe of the bladder-bellows, beneath the surface of water, exquisitely freed from air, to see whether, upon the depression of them, by the incumbent weight, when the receiver was carefully exhausted, there would be any thing expell'd at the pipe, productive of bubbles in the liquor, wherein its orifice was immers'd.

To bring our conjectures to a trial, we put into a capp'd receiver, the bladder, accommodated as already mentioned, containing between half a pint, and a pint; and to depress the upper basis of these little bellows the more easily, and uniformly, we cover'd the round piece of past-board, that made the upper basis, with a pewter plate; a hole being made in it for the neck of the bladder: which, upon trial, prov'd not ponderous enough without weight of lead. And to secure the feather above-mention'd, from being blown aside, we made it to move in a perpendicular slit in a piece of past-board, fasten'd to one part of the upper basis; as that whereto we glued the feather, was to another part. Things being thus provided, the pump was work'd; and as the ambient air was, from time to time, withdrawn, that in the bladder expanded itself so as to lift up the metalline weight, and yet, in part, to sally out at the little glass pipe of our bellows; as appear'd by its blowing up the feather, and keeping it suspended, till the spring of the air in the bladder was too far weakned. In the meantime, we did, now and then, by the help of a string fastned to the turn-key, and the upper basis of the bellows, let down the basis a little, to observe how, upon its sinking, the blast, against the feather, would decrease, as the receiver was further exhausted. And when we judg'd it to be sufficiently freed from air, we let down the weight, but could not perceive that, by shutting the bellows, the feather was at all blown up as before; tho' the upper basis were more than usually depress'd. And yet it's somewhat odd, that when, in order to a further trial, the weight was drawn up again; as the upper basis rose from the lower, the sides of the bladder were sensibly press'd, or drawn inwards. The bellows being thus open'd, we let down the upper basis again, but could not perceive that any blast was produced; for tho' the feather, which lay just over, and near the orifice of the little glass pipe, had some motion, yet this seem'd plainly to be but a shaking, and almost vibrating motion, whereinto it was put by the upper basis, which the string kept from a smooth and uniform descent; but not to proceed from any blast, issuing out of the cavity of the bladder. And, for further satisfaction, we caus'd some air to be let into the receiver; because there was a possibility that the slender pipe might, by some accident, be choaked: but tho', upon the return of the air into the receiver, the bases of the bellows were press'd closer together, yet it seem'd that some little air got thro' the pipe, into the cavity of the bladder; for



when we began again to withdraw the air that was let into the receiver, the bladder began to swell again, and, upon letting down the weight, to blow up, and sustain the feather, as happen'd before the receiver had been so well exhausted.

Continued.

Fig. 59.

39. I caus'd a crooked pipe to be made for the syringe, formerly mention'd, instead of its strait one, whose shorter leg was parallel to the longer. And this pipe, after being screw'd on carefully, was cemented to the barrel; and because the brass-pipe could scarce be made small enough, we caus'd a short and slender pipe of glass, to be put into the orifice of the shorter leg, and carefully fasten'd to it with cement. Then the sucker being made to go smooth, without lessening the staunchness of the syringe, there was fasten'd to the handle of the rammer, a weight made in the form of a ring, or hoop; which, by reason of its figure, might be suspended from the handle of the rammer, and hang loose on the outside of the cylinder, and which, both by its figure and weight, might easily, and swiftly depress the sucker, when drawn up. The syringe, thus furnish'd, was fasten'd to a broad, heavy pedestal, to keep it in its vertical posture, and to hinder it from tottering, notwithstanding the weight that clogg'd it. Besides all this, we took a feather, about two inches long, of which there was left, at the end, a part about the breadth of a man's thumb-nail, to cover the orifice of the slender glass pipe of the syringe; for which purpose, the other extremity of it was so fasten'd, with cement, to the lower-part of the syringe, that the broad end of the feather stood just over the little orifice of the glass, at such a convenient distance, that when the sucker was a little drawn up, and let go again, the weight would depress it fast enough to blow up the broad part of the feather. The handle of the rammer, being now ty'd to the turn-key of a capp'd receiver, the syringe, and its pedestal, were inclosed in a capacious receiver; and the pump, being set on work, we, after some quantity of air was drawn out, rais'd the sucker a little, by the help of the turn-key: and, then, turning the same key the contrary way, we suffer'd the weight to depress the sucker, to see how the feather would be blown up; and, finding that it was impell'd, forcibly, we continu'd to pump, by pauses; during each of which, we rais'd and depress'd the sucker, as before; and observ'd, that as the receiver was gradually exhausted of air, the feather was less briskly driven up, till, at length, when the receiver was well empty'd, the usual elevations and depressions of the sucker would not blow it up at all, tho' they were far more frequently repeated than before.

After we had long tried, in vain, to raise the feather, some air was let into the receiver; and tho', when but very little air was admitted, the motions of the sucker had scarce any sensible effect upon the feather; yet, when the quantity began to be considerable, the feather began to move a little upwards; and so letting in air, not all at once, but successively, and moving the sucker up and down, in the intervals of those times of admission; we observ'd, that as the receiver contain'd more air, the feather was more briskly blown up.

But,

But, not content with a single trial, we caus'd the receiver to be again exhausted, and prosecuted the experiment with the like success; only having, after the receiver was exhausted, drawn up, and let fall the sucker, several times, ineffectually; having, hitherto, not, usually, rais'd it by more than one turn of the hand; we now used an instrument, that was tolerably long, and fit to take hold of the turn-key, so that we could easily raise the sucker between two and three inches at a time, and suddenly depress it again: yet, for all this, which would much have increas'd the blast, if there had been a matter fit for it in the cavity of the syringe, we could not, sensibly, blow up the feather, till we had let a little air into the receiver. But, now, instead of the brass-pipe, hitherto employ'd, we cemented one of glass to the syringe; its shorter leg, after it had, for a while, run parallel to the other, being bent off so, that above an inch and a half of it tended downwards; whereby the orifice of it might be immers'd in the water contain'd in a small open jar. The design of this contrivance was, that when the receiver should be well exhausted, we might try whether, by raising and depressing of the sucker, any such matter would be driven out at the nose of the pipe, as would produce bubbles in the incumbent water; which, air, tho' highly rarify'd, is capable of doing.

Fig. 60.

The only particulars, wherein this experiment differ'd from the former, were these. As the air was here pump'd out of the receiver; that in the glass-pipe made its way thro' the water, in bubbles. And a little air having once, by a small leak, got in, and forc'd some of the water out of the jar into the pipe; when the receiver was again well empty'd, both that water, and the little stagnant quantity contain'd in the immers'd part of the pipe, produced so many bubbles, of several sizes, as quite disturb'd our observations. Wherefore, we let alone the receiver, exhausted as it was, for six or seven hours, that the water might free itself from air; and then causing what air might have stolen in, to be again pump'd out, till we perceiv'd, by the gage, that the receiver was well exhausted, we caus'd the sucker of the syringe to be rais'd and depress'd several times; and tho', even then, a bubble would, now and then, disturb our observations, yet, when we were not thus confounded, we sometimes observ'd, that the elevation and fall of the sucker, tho' repeated, did not drive out at the pipe, any thing that made discernible bubbles in the incumbent water: for tho' some small bubbles would rarely appear on the surface of the water, yet I could not perceive, that the matter which made them, issued out of the pipe; and some of them manifestly proceeded from aerial particles, still lurking in the water, as I concluded from the place and time of their rising. But, at length, we observ'd, the water, in the immers'd part of the pipe, which was very slender, to be about an inch higher than the rest of the stagnant water, and to continue at that height in the pipe, tho' the sucker were, several times together, rais'd and depress'd, between two and three inches at once; which seem'd to argue, that there was a vacuum in the cavity of the syringe: or if it were full of æther, this was so subtile, that the impulse it

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A light body falling in the exhausted receiver.

Fig. 61.

receiv'd from the falling sucker, would not make it displace that very slender thread of water in the small pipe; though it appear'd by the bubbles, which sometimes disclos'd themselves in the water, after the receiver had been exhausted, that far more water would be displaced, and carried up by a small bubble, consisting of air, so rarified, that, according to my estimate, the particles of it did not, before the pump was first set on work, possess, in the water, a five hundredth part of the space of a pin's head.

40. We took a receiver twenty-two inches high; and, that we might let a body fall therein, we so fasten'd a small pair of tongs to the inside of its brass-cover, that, by moving the turn-key, we might, by a string, open them; which their own spring would, otherwise, keep shut: we then join'd, cross-wise, four broad light feathers, each about an inch long, at their quills, with a little cement; into which we, also, stuck, perpendicularly, a small label of paper, about the eighth of an inch in breadth, and somewhat more in height; by which the tongs might take hold of our light instrument, without touching the cement, which, else, might stick to them. By the help of this small piece of paper, the little instrument, of which it made a part, was so held by the tongs, that it hung horizontal; and then the receiver, being cemented to the engine, the pump was diligently ply'd, till it appear'd, by a gage, that the receiver had been thoroughly exhausted. Lastly, our eyes, being attentively fixed upon the connected feathers, the tongs were, by the help of the turn-key, open'd, and the little instrument let fall; which, tho', in the air, it had made some turns in its descent from the same height, whence it now fell; yet it here descended like a dead weight, without being perceiv'd to make a single turn, or a part of one. However, I caus'd the receiver to be taken off, and put on again, after the feathers were taken hold of by the tongs; whence, being let fall in the glass, unexhausted, they made some turns in their descent; as they, also, did, being let fall a second time, after the same manner.

But when, after this, the feathers being placed, as before, we repeated the experiment, carefully pumping out the air, we could not perceive any turning in the descent; yet, for farther security, we let them fall twice more, in the unexhausted receiver; and found them to turn in falling: but when we did, a third time, set them loose in the receiver, well exhausted; they fell, after the same manner they had, in the same case, done before.

The propagation of sounds in an exhausted receiver.

41. We caus'd a cylinder of box to be turn'd of a length suitable to that of the receiver, wherein it was to be employ'd. Out of the lower basis of this cylinder, which was about an inch and a half in diameter, there came a smaller cylinder or axle-tree, not a quarter so thick as the other, and less than an inch long: this was turn'd very true, that it might move smoothly in a little ring of brass made for it in the midst of a fix'd trencher, or piece of solid wood, shap'd like a mill-stone; being four or five inches in breadth, and between one and two in thickness: and the large round groove, purposely made, in the lower part of this trencher, I caus'd to be fill'd up with lead, to keep the trencher steady: and in the uppermost part of this trencher we intended to have holes made, to place bodies in at several distances, as occasion should require. The upper basis of the cylinder had, also, another axle-tree coming out of the midst of it, but wider than the former, that

that into its cavity it might receive the lower end of the turn-key, to which 'twas to be fasten'd by a slender peg of brass, thrust thro' two correspondent holes, the one made in the turn-key, and the other in the socket of the axle-tree. There were also several horizontal perforations made in the pillar itself, to which this axis belong'd; which pillar we call the vertical cylinder. The general use of this contrivance, is, that the end of the turn-key being put into the socket, and the lower axis of the vertical cylinder into the trencher; by the motion of the key, a body fasten'd at one of the holes to the cylinder may be brought to, or remov'd from, or made to strike against another body, fasten'd, in a convenient posture, to the upper part of the trencher.

We caus'd then a hand-bell without its handle and clapper, to be so fasten'd, to a strong wire, that one end of the wire being fixed in the trencher, the other, which was bent downwards, took hold of the bell. In another hole, made in the circumference of the same trencher, was wedg'd a steel spring, to the upper part whereof was wedg'd a gad of steel less than an inch long, but considerably thick; the length of this spring made the upper part of the hammer, or piece of steel, of the same height with the bell; and the distance of the spring from the bell was such, that when forc'd back the other way, it might, at its return make the hammer strike briskly upon the outside of the bell. The trencher being thus furnish'd and plac'd in a capp'd receiver, the air was diligently pump'd out, and then, by the help of the turn-key, the vertical cylinder was made to go round, by which means, as often as one of the two stiff wires, or small pegs, that were fasten'd at right angles into holes made near the bottom of the cylinder, pass'd by the spring, they forcibly bent it in their passage from the bell, so that as soon as the wire was gone by, and the spring ceas'd to be press'd, it would fly back with violence enough to make the hammer give a smart stroke upon the bell. And, by this means, we could both continue the experiment at discretion, and make the percussions more equally strong than it would otherwise have been easy to do.

Now, when the receiver was well emptied, it sometimes appear'd doubtful whether any sound were produc'd or no; but to me, for the most part, it seem'd, that, after great attention, I heard a very faint and languid sound, and yet methought it had some shrillness in it, and seem'd to come from afar. But letting in the air, at competent intervals, it was easy to observe, that the vertical cylinder being still made to go round, when a little air was let in, the stroke of the hammer upon the bell became very audible: when more air was admitted, the sound grew greater, and so increas'd till the receiver was again replenish'd with air; tho', even then, the sound was observ'd to be much less than when the receiver did not interpose between the bell and the ear.

We now, also, suspended in the receiver a watch with a good alarm; and to make this experiment the more accurate, we employ'd a receiver that consisted of but one piece of glass furnish'd on the inside with a glass knob or button, to which a string might be ty'd: we also hung the watch, not by its chain, but by a very slender thread, whose upper end was fasten'd to the glass button. Then the air being carefully pump'd out, we silently expected

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ted the ringing of the alarum; but hearing no noise so soon as we expected, it might have been doubted, whether the watch continued going, if we had not contriv'd a way to discern its motion: wherefore, I desired a gentleman to hold his ear exactly over the button, at which the watch was suspended, and very near to the receiver; who told us, that he could just perceive something of a sound, which seem'd to come from far; tho', neither we, who listen'd very attentively near other parts of the receiver, nor he, if his ears were no more advantageously plac'd, were satisfied, that we heard the watch at all. Then letting in some air, we did, with attention, begin to hear the alarum, whose sound was odd; and by returning the stop-cock, to keep any more air from entering, we kept the sound thus low for a considerable time; after which, a little more air, that was permitted to enter, made it become more audible; and when the air was yet more freely admitted, we could plainly hear the alarum at a considerable distance from the receiver.

A glass-drop
broke in an ex-
hausted receiver.

42. The blunter part of a glass-drop being fasten'd to a stable body, and convey'd into the receiver, and the crooked stem being ty'd to one end of a string, whose other end was fasten'd to the turn-key, we carefully pump'd out the air; when the stem, by shortning the string, being broken off, the glass-drop was shatter'd into a thousand pieces.

This experiment was, afterwards, repeated with the like success; and having, at that time, no gage to try how far the air had been drawn out, we let the external air impel up the water out of the pump into the receiver, and thereby found, that the vessel had been well exhausted.

Light produced
in the exhausted
receiver.

Fig. 62.

43. Knowing, that hard sugar, being briskly scraped with a knife, affords a sparkling light; so that one would sometimes think sparks of fire flew from it; we caus'd a lump of hard loaf-sugar to be conveniently, and firmly plac'd in the cavity of our capp'd receiver; and, to the vertical cylinder, formerly mentioned, we fasten'd some pieces of a steel-spring, which, being but thin, might, in their passage along the sugar, grate or rub forcibly against it; and, then the receiver being well exhausted, in the night-time, and in a dark room, the vertical cylinder was made, for a pretty while, to move round, by help of the turn-key. Thus the irons that came out of the vertical cylinder, making, in their passage, vigorous impressions upon the sugar, that stood in their way, there were manifestly produced many little flashes; and sometimes too, tho' not frequently, there seem'd to be struck off small sparks of fire †.

44. We

* That sound cannot be propagated thro' a vacuum, appears more fully from an experiment of the late Mr. Hawksbee, who included a large bell in a receiver full of common air, and cover'd them both with another glass, out of which, the air being extract'd, tho' sound was actually produced in the innermost, it could not be heard by the by-standers. *Philos. Trans.* N^o. 321. p. 367.

From some other experiments of the same person, 'tis also evident, that sounds are as well augmented in condensed air, as diminish'd in that, which is rarified. See his *Physico-mechan. Experiments.* p. 129. 134.

† From a variety of experiments, relating to the attrition of bodies in vacuo, made by the late Mr. Hawksbee, it appears, that

44. We took a large inverted cucurbit for a receiver, made very clear by wiping, and observ'd, that when the pump began to be work'd, if a large candle were held on the other side of the glass, upon turning the stop-cock to let the air out of the receiver into the cylinder, the glass would seem to be full of fumes, and a kind of halo appear about the flame of the candle; and this, at first, was commonly between a blue and a green, but after some sucks, turn'd of a reddish or orange colour, both very vivid. The phenomenon, in my opinion, proceeded from hence, that the cement being somewhat soft, and abounding with turpentine, and having a hot iron apply'd to it, whereby it was both soften'd and heated, it seems rational to expect, that, upon withdrawing the air in the receiver, the aerial particles in the cement freed from their former pressure, would extricate themselves, and with the looser steams of the turpentine, and perhaps of the bees-wax, expand themselves, with a kind of explosion, in the receiver; and by their interposition between the light and the eye, exhibit those delightful colours we had seen. And, I afterwards found, that I could plainly perceive the colouring steams, just upon turning the stop-cock, to fly up from the cement towards the top of the glass; and, if we continued pumping, the receiver would grow clearer, and the colours more dilute, possibly because the aerial and volatile particles of the upper part of the cement did, in that time, spend themselves; and also, because the agitation they receiv'd, from the heat communicated by the iron, continually decay'd. Besides, when the receiver is more exhausted, the want of air makes it more difficult for steams to float, and be supported in it.

But, for a farther confirmation, I caus'd some cement to be put into a small crucible, warm enough to melt it; and conveying this into a clear receiver, I caus'd the pump to be work'd: upon which, it manifestly appear'd, that, opening the stop-cock, to let out the air, the steams would copiously be thrown about from the crucible into the capacity of the receiver; and, after having play'd there a little, fall down again. But, in these phenomena, the vividness, and sometimes the kind of the exhibited colours seem'd much to depend on circumstances, such as the degrees of heat, the magnitude and shape of the receiver, the quantity of air that remain'd therein, and the nature of the cement itself.

45. Cross the stable trencher, formerly mention'd, we fasten'd a strong spring of steel, shaped almost like the lathe of a cross-bow; and to the midst of this spring was strongly fix'd on the outside a round piece of brass, hollow'd almost like a concave burning-glass. To this piece of brass, which was thin, and about two inches in diameter, we fitted a convex piece of the same metal, almost like a gage for a tool to grind glasses in, which had belong-

that different sorts of bodies afford lights greatly differing in colour, force, and vigour; that the effects of attrition vary with the different preparation and management of the bodies which sustain it; that bodies, which have yielded a parti-

cular light, may, by attrition, be brought to yield no more thereof; and that a considerable light is producible, by the attrition of glass on glass, both in *vacuo*, in common air, and even in water. *Hawksb.*

Physico-mechan. Exp. p. 40-44.

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A kind of halo, and colours produced in the exhausted receiver.

Heat produced by attrition in the exhausted receiver.

Fig. 63.

EXPLANATION ing to it a square handle, whereinto, as into a socket, was inserted a square piece of wood, proceeding from the basis of a square wooden pillar, which we made use of, on this occasion, instead of our vertical cylinder. By the help of another piece of wood, coming from the other basis of the same pillar, the turn-key was join'd to this pillar, and made of such a length, that when the turn-key was forcibly kept down as low as the brass-cover, it was a part of, would permit, the convex piece of metal just describ'd, depress'd the concave piece a pretty way, notwithstanding a vigorous resistance of the subjacent spring. A little fine powder of emery was also put between the convex and concave pieces of brass, to make them fit the better, and to facilitate the motion that was to be made; and, to the upper part of the turn-key was fasten'd a good wimble, without which, we presum'd, that the turning of the key would not produce a sufficient motion. Things being thus in readiness, and a mercurial gage convey'd into the receiver, we caus'd the air to be diligently pump'd out, and then order'd a strong man to turn the wimble, and to continue to lean a little on it, that he might be sure to keep the turn-key from being lifted up by the spring. Whilst the man, with much agility and strength was moving the wimble, I watch'd the gage, to observe, whether the agitation of the stop-cock, and consequently the engine, did not prejudice the experiment; and for greater caution, I caus'd the pump to be almost all the while kept working. When the man was almost out of breath, we let in the air at the cover of the receiver, by lifting up the turn-key; and nimbly removing the receiver, we felt both the pieces of brass, betwixt which the attrition had been made, and found them very sensibly warm.

We afterwards caus'd the man to lay hold of the wimble again, when, by the gage, it appear'd, that the receiver was well exhausted; so that by further pumping the quick-silver seem'd not to be further depress'd. And, in this second trial, when we did, as before, hastily let in the air, and take out the bodies that had been rubb'd against one another, they were both of them, especially the uppermost, so hot, that I could not endure to hold my hand on either; and they did, for some time, retain a considerable degree of warmth. I also caus'd two bodies of wood to be turn'd, for size and shape like those of brass, which we had just before employ'd; the upper of these was of hard oak, the other of beech: but, tho' the wimble was swiftly turn'd, as before, by the same person, the wood seem'd not to me to have manifestly acquired any warmth; yet, that there had been a considerable attrition, appear'd by the great polish, which part of the wood had evidently acquired: however, upon repeating the experiment, with more obstinacy than before, the wood, especially the upper piece of it, was brought to a warmth unquestionably sensible.

*Quick-lime
placed in the
exhausted re-
ceiver.*

46. Into an evaporating glass, we put a convenient quantity of water; and having convey'd it into a receiver, and well drawn out the air, we let down into it, by the turn-key, a large lump of strong lime; and observ'd not, that, at the first emersion, nor for some time after, there appear'd any considerable number of bubbles; but within about a quarter of an hour, the lime began

began, (the pump continuing to be ply'd, from time to time,) to flake with much violence, and with bubbles wonderfully great, appearing at each new exsuction; so that the inside of the receiver, tho' large, was, at length, lined with lime-water; and much of the mixture did, from time to time, overflow the vessel, a great part whereof was purposely left unfill'd: nor did any thing, but our weariness, put a period to the bubbling of the mixture, whose heat was sensible even on the outside of the receiver, and continued considerably hot, in the evaporating glass, for a quarter of an hour after the receiver was remov'd. The lime, employ'd in this experiment, was of a very good and strong kind, made of hard stones, and not of chalk, as is that commonly used at London, which, probably, would not have been strong enough to have afforded us the same phenomenon.

47. To try, by means of our syringe, formerly mention'd, what weight a cylinder of uncompress'd air included in it, and consequently of the same diameter with the cavity of the barrel, would be able to sustain; we provided a stable frame, wherein the syringe might be kept firm and erect: we also provided a weight of lead, shaped like our brass-ring, formerly describ'd, that, by the advantage of its figure, it might be made to hang down, by strings, from the top of the handle of the rammer, and so press evenly on all sides, without rendring the upper part of the instrument top-heavy. We took care to leave between the bottom of the syringe, which was firmly clos'd with strong cement, and that part of it, where the sucker was, a convenient quantity of air to expand itself, and lift up the weight, when the air external to that included, should be pump'd out of the receiver. And lastly, the handle of the rammer, from which the annular weight depended, was so fasten'd to the turn-key of the cover of the receiver, that the weight might not compress the air included in the syringe, but leave it in its natural state, till the air was withdrawn from the receiver.

An attempt is made to measure the force of the spring of included air.

By this method, the included air would lift up a weight of seven, or eight pound; yet, when the rammer came to be clogg'd with a greater, the instrument prov'd not so stanch, but that it was easier for some particles of air to get away between the sucker, and the inside of the barrel, than to raise so great a weight. But, if an exact syringe can be procured, this seems to be one of the likeliest, and least exceptionable ways of measuring the force of the air's spring.

But, being unable to procure such a syringe as I desired, I got two hollow cylinders to be turn'd, whose sides were of a sufficient thickness to resist the pressure of the air to be imprison'd in them; one an inch in diameter, and the other two: their depths were also unequal, that the one might receive a much larger bladder than the other. With the lesser of these, I made a diligent trial; but found it very difficult to procure a bladder small, and fine enough for the cylinder: and that which we, at length, procured, would not continue stanch for many trials; but, after a while, parted with a little air in the well exhausted receiver, when 'twas clogg'd with the utmost weight it could sustain: but whilst it continued stanch, we made

Fig. 64.

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one fair trial with it; from whence we concluded, that a cylinder of air of an inch in diameter, and less than two inches in length, was able visibly to raise a weight of above ten pound, averdupoize.

At another time, into a hollow cylinder of wood four inches deep, and two in diameter, furnish'd with a broad and solid body or pedestal, we put a lamb's-bladder very strongly ty'd at the neck; on which, we set a wooden plug, mark'd with ink, where the edge of the cylinder was contiguous to it: this plug being loaded with weights, amounting to thirty-five pound, the receiver was exhausted, till the mark appear'd very manifestly above the brim of the cylinder; and then, tho' the string was, by turning the key, quite slacken'd, yet the mark on the plug continued very visible. And, when so much air was let into the receiver, as made the weight depress the plug quite beneath the mark, upon pumping out the air again, the weight was, without the help of the turn-key, lifted up; and by degrees, all the mark of the plug was rais'd above three eighths higher than the edge of the cylinder. Wherefore, we substituted for the seven pound weight, one of fourteen; and using the same bladder, we repeated the experiment; only a little supporting the uppermost weight by the turn-key, till the bladder had attained its expansion; and then the weight, being gently let go, depress'd not the plug so low, but that we could yet see the mark on it; tho' that part of the plug where the mark was, appear'd manifestly more depress'd than the other.

An easy way of making a small quantity of included air raise a great weight.
Fig. 65.

48. We took a brass-vessel, made like a cylinder, and having one of its orifices exactly cover'd with a flat plate firmly fasten'd to it, the other orifice being wide open: the depth of this vessel was four inches, and the diameter three and three quarters. To this hollow cylinder we fitted a wooden plug, like one of those described in the foregoing experiment; only it was not quite so long, and was furnished with a lip, which we, purposely, made of a considerable breadth, that it might afford a stable basis to the weight that should rest upon it: then, taking a middle-sized limber bladder, strongly ty'd at the neck, but not near full blown; we pressed it, by the help of the plug, into the cylinder, that it might the better fit itself to the figure thereof: then, taking notice, by a mark, how much of the plug was extant above the orifice of the vessel, we laid the weights upon the plug, whose lip hinder'd it from being depressed too deep into the cavity of the vessel; and, having convey'd them into the receiver, we found, that a common half hundred weight would very soon be manifestly raised by the spring of the included air.

Fig. 66.

In another experiment, the bladder in a cylinder four inches broad, raised 75 pound weight, till the wooden plug disclosed the mark design'd to shew the height at which the air kept the plug, before it was compressed; and this, visibly, at the fifth exsuction; and at the seventh, that mark was raised $\frac{1}{2}$ above the edge of the cylinder. In the gage, where the mercury, in the open air, usually stood, about an eighth above the uppermost glass-mark, it was depressed an eighth below the second mark; and after we let in the air, it was a pretty while before the weight manifestly began to subside. The bladder being taken out, and the place it had possessed in the cylinder

cylinder being supply'd with a sleeve, or some such thing, and the weight laid again upon the plug; we found, that, at twenty-four exsuctions, the mercury was depressed to the lowest mark of the gage; and the thirty-fourth, or thirty-fifth exsuction was made, before the receiver appear'd to be so exhausted, as to stop the sinking of the mercury, which was then above one eighth beneath the lowest mark. But, having caused leaden-weights to be, purposely, cast flat, and as broad as we could conveniently put into the receiver, that, by the advantage of this shape, we might be able to pile up the more of them, without much danger of their being shaken down; we laid several of them one upon another: and, then, the upper part of the receiver growing too narrow to admit any more; we added a weight, or two, less broad; when, exhausting the receiver, till we perceiv'd, by the gage, that the air was manifestly withdrawn; we found, by the help of a mark, and a pair of compasses, the plug to be so far rais'd, that 'twas concluded, the elevation would have been much greater, if the included air had not found it easier to produce some leak at the neck of the bladder, than to lift up so great a weight; which was about a hundred pounds, averdupoize.

49. We weigh'd a seal'd bubble in the receiver, and found it above half a grain heavier when much of the air was exhausted, than when it was full: afterwards, we took out this bubble, and found it to weigh sixty-eight grains and a half; then, breaking off the small tip of it under water, we found, that the heat, by which it was seal'd up, had rarify'd its included air, so that it admitted a hundred and twenty-five grains of water: for the admitted water and glass weigh'd a hundred ninety-three grains and a half. Then, filling it full of water, we found it to contain, in all, seven hundred and thirty-nine grains; for it weigh'd eight hundred and seven grains and a half: whence, 'tis evident, that the difference between the weight of water and air, was less than 1228 and 1. We, also, weigh'd, in the receiver, a bubble, the glass of which amounted to sixty grains; the air that fill'd it, weigh'd, *in vacuo*, $\frac{2}{3}$ of a grain; the water that fill'd it, weigh'd seven hundred twenty grains and a quarter: so that, by this experiment, the proportion of the weight of water to air, is as $853 \frac{1}{2}$ to 1.

To shew the weight of air to that of water.

But it is so desirable a thing, and may prove of such importance, to know the proportion in weight betwixt air and water, that I shall here mention an attempt I made to discover it by another way.

A small receiver, being exhausted of air by the engine, and counterpois'd; whilst it continu'd so, the stop-cock was turn'd, and the air re-admitted; which made it weigh thirty-six grains more than before: and this happen'd, also, upon repeating the experiment.

We, next, took a small glass-receiver, fitted with a stop-cock; and, having exhausted it of the air, counterpoiz'd it, and let in the outward air; we found the weight of the vessel to be increas'd, by that admission, thirty-six grains. This done, we took the receiver, after having well counterpoiz'd it, out of the scale; and having apply'd it a second

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time to the engine, we once more withdrew the air; and then turning the stop-cock, to keep out the external air, we took care that none of the cement employ'd to join it to the engine, should stick to it: when, weighing it again, we found it thirty-five grains heavier, than when 'twas last counterpois'd in the same balance. Then we immers'd the stop-cock into a basin of fair water, and let in the liquor, that we might find how much of it would succeed in the place of the air drawn out. When no more water was impell'd in, we turn'd the stop-cock once more, to keep it from falling out; and, then, weighing it in the same scales, we found the water to be forty-seven ounces three drams, six grains; which, divided by thirty-five grains, the weight of the air, equal in bulk to this water, the quotient, is, nearly, six hundred and fifty grains, for the proportion of weight between air and water, of the same bulk, at the time when the experiment was made: the atmosphere then appearing, by the barometer, wherein the mercury stood, at twenty-nine inches three quarters, to be very heavy.

Two marbles
strongly join'd
together, separa-
ted by withdraw-
ing the air from
them.

50. We took a pair of flat round marbles, each of them two inches, and about three quarters, in diameter; and, having put a little oil between them, to keep out the air; we hung, at a hook fasten'd to the lowermost, a pound weight, to surmount the cohesion which the tenacity of the oil, and the imperfect exhaustion of the receiver might give them: then, having suspended them in the cavity of a receiver, by a stick that lay cross it, and the engine being made ready to work, we shook it more strongly than we concluded it would be by the operation; and, beginning to pump out the air, we observ'd the marbles to continue join'd, till it was so far drawn out, that we suspected they would not separate. But, at the sixteenth stroke, upon turning the stop-cock, which let the air pass out of the receiver into the pump, the shaking of the engine being over, the marbles, spontaneously, fell asunder; tho' they hung parallel to the horizon, and adhered very firmly together, when they were put in: and tho' a weight, of above eighty pounds, fasten'd to the lowermost marble, might be drawn up, together with the uppermost, by virtue of their firm cohesion.

Fig. 67.

But, fastening to the lowermost of the two marbles, a weight of a very few ounces, and having cemented a capp'd receiver, with the marbles in it, as before, to the pump; we, by means of a string, (whereof one end was tied to the bottom of the turn-key, and the other to the uppermost marble, and passing thro' the hook belonging to the brass-cover) and by turning round the key, drew up the upper marble; and, by reason of their coherence, the lowermost, also, together with the weight that hung at it. Being thus sure that the two marbles stuck close together, we began to pump out the air; and, after a while, the marbles fell asunder.

But, having so order'd the matter, that the lower could fall but a little way from the other; we were able, by inclining and shaking the engine, to place them together again: and, then, letting in the air hastily, that, by its spring, it might press them hard together; we could not only, by
turn-

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turning the key, make the uppermost marble take up the other, and the annex'd weight; but were oblig'd to make a much more laborious exhaustion of the air, to procure the disjunction of the marbles, this second time, than was necessary to do it at the first.

And, when the marbles were thus asunder, and the receiver exhausted, we did not let in the air, till we made them fall upon one another, as before; but the little highly expanded air, that remain'd in the receiver, having not a spring near strong enough to press them together, we very easily, by turning the key, rais'd the uppermost marble alone, without finding it to stick to the other. We, therefore, once more join'd the marbles together, and, then letting in the external air, found them, afterwards, to stick so close, that a very strong man could not separate them.

51. Into a small earthen melting-pot of a cylindrical figure, and well glaz'd, we convey'd a small cylinder of iron, about an inch long, and an inch and a half in diameter, made red hot in the fire; and having suddenly exhausted the receiver, wherein we plac'd them, we let down a piece of paper, containing a convenient quantity of flowers of sulphur, upon the heated metal; whereby, the paper being immediately destroy'd, the included sulphur would lie upon the iron, whose upper part was a little concave, to contain the flowers, when melted. But all the heat of the iron, tho' it made the paper and sulphur smoke, would not actually kindle either.

That 'tis difficult to produce flame without air, shewn by an attempt to kindle sulphur in vacuo.

Into a glass-bubble of a convenient size, furnish'd with a neck fit for our purpose, we put a little flower of brimstone, and having exhausted the glass, and secur'd it against the return of the air; we laid it upon burning coals, where the sulphur did not take fire, but rose to the opposite part of the glass in the form of a fine powder; and that part being turn'd downward, and laid on coals, the brimstone without kindling rose again in the form of an expanded substance, which, when remov'd from the fire, was, for the most part, transparent like a yellow varnish.

52. To examine whether, when a heated iron would not keep the melted brimstone so hot, as was requisite to make it burn, without air, or with very little, it would yet suffice to kindle the sulphur, if the air had access to it, we made two or three several trials; and found, that, if soon after the flame was extinguish'd the receiver were remov'd, the sulphur would presently take fire again, and flame as vigorously as before. But, we suspected, that the agency of the air, in the production of the flame, might be somewhat less, than these trials would persuade; because, by taking off the receiver, the sulphur was not only expos'd to fresh air, but also advantaged, by a free liberty for the avolation of those fumes, which, in a close vessel might be unfavourable to the flame.

The efficacy of air in the production of flame.

And, to try at how great a degree of rarification of the air, it was possible to make sulphur flame, by the assistance of an adventitious heat, we repeated the same experiment; the pumping being continued for some time after the flame of the melted brimstone seem'd quite extinguish'd, till the receiver was judg'd to be very well exhausted: then, without stirring the glass, we very warily let in a little air; upon which we could perceive,

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ceive, tho' not a constant flame, yet several little flashes, as it were, disclose themselves, by their blue colour, to be sulphureous; yet the air that had sufficed to re-kindle the sulphur, was so little, that two exsuctions drew it out again, and put an entire stop to the phenomenon. And, when a little air was cautiously let in again, the like flashes began again to appear; which, upon two exsuctions more, quite vanished: tho', upon letting in a little fresh air, the third time, they, once more, re-appear'd.

An attempt to
fire gun-powder
in vacuo, by
the sun's rays.

53. Having conveniently placed three or four grains of gun-powder in our receiver, and carefully drawn out the air, we threw the sun-beams, united by a good burning-glass, upon the powder, and kept them there, for a considerable time, to little purpose; till, at length, the powder, instead of taking fire, only melted, like a metal. And this was not the only experiment we then made, which discover'd a great indisposition, even in gun-powder, to be fir'd *in vacuo*.

By means of a
hot iron.

54. We took a convenient weight of gun-powder, that was extraordinary strong, and well made; and, having placed a red-hot iron in our receiver, that was capable of holding sixteen pounds of water; when the air appear'd, by the mercurial gage, to have been well exhausted, we let down a small piece of thin paper, wherein the powder had been put, till it reach'd the plate; by whose heat, we hoped, the paper would be destroy'd, and the powder made to go off. But, tho' both of them had been previously well dry'd by the fire, no explosion of the powder ensued; yet there appear'd, upon the iron-plate, a broad blue flame, surprizingly durable, and resembling that of brimstone. At length, taking off the receiver, we found, that the paper, contiguous to the iron, was, in part, destroy'd by the heat; but most of the grains of the powder seem'd unalter'd, and retain'd their disposition to be fir'd, notwithstanding the consumption made of their brimstone.

Upon repeating this experiment, we found no explosion to be made for so long a time, that, thinking it in vain to wait, we let in the air; and, after we had, also, despair'd of any effect from hence, the powder suddenly went off, with a great flash, and a considerable shake of the receiver, that was yet standing on the engine: which shews, that such experiments should be made with caution; for tho' this receiver would contain two gallons of liquor, the powder, here employ'd, weigh'd but one grain.

A heated glass,
emptied of air.

55. Into a large strong glass-bubble, we put a few small corns of gun-powder; and, having carefully exhausted the glass, and secured it against the return of the air, we put it upon live-coals, superficially cover'd with ashes; by the heat whereof, the sulphureous ingredient of the powder was, in part, kindled, and burn'd blue for a pretty while, and with a flame considerably great; upon the ceasing whereof, the powder, which, after all, did not take fire, appear'd to have sent up, besides the flame, a large quantity of sulphureous sublimate, that stuck to the upper part of the glass: and, being held against a lighted candle, it exhibited several vivid colours, like those of the rain-bow.

56. We

56. We took a small, and very short pistol, and having well fasten'd it, with strings, to a great weight, that was placed upon the iron-plate of our engine, we drew up the cock, and primed the pan with dry powder; then, over both the weight and pistol, we whelm'd a receiver, capable of containing two gallons of liquor; and, having carefully cemented it on, we caused the air to be diligently pump'd out; having, before, put in a mercurial gage, to help us to discern when it was well exhausted. Lastly, ordering the pump to be plied, in the mean time, for fear some air should steal in; we, by shortning a string that was tied to the trigger of the pistol, did all we could towards firing of the powder in the pan: but tho' the pan were made to fly open, the powder did not go off; then, letting in the air, and cocking the pistol again, we drew out a little air, to be sure that the receiver was closely cemented on; when, letting in the air at the top of the receiver, and stopping it in, we pull'd the trigger again: whereupon, tho' there had been no new powder put into the pan, nor any left in it, but the little that remain'd after the late trial; yet that little readily took fire, and flash'd in the pan: which made it the more probable, that, in the former trial, sparks of fire had been struck out, by the collision of the flint and steel. Besides, in another trial, made, the same hour, in the same exhausted receiver, a spark, or two, were seen to fly out, upon the falling of the cock. It appears, therefore, that, notwithstanding the great indisposition of gun-powder to be reduced into flame, *in vacuo*, yet even solid matter is not incapable of being fir'd there, if put into a motion sufficiently vehement.

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And by means
of sparks of fire
in vacuo.

57. The rays of the sun, being thrown upon some *Aurum fulminans*, placed in an exhausted receiver, made it go off, and violently scatter about the cavity of the glass a yellowish dust, which other trials, in the free air, made us look upon as particles of the gold, that was the principal ingredient of this odd composition.

Two ways of
making *Aurum
fulminans* go off
in vacuo.

This experiment we repeated, long after, in another place, with other vessels, and found the like success. And once, in the night-time, putting upon a heated iron, $\frac{1}{4}$ of a grain of good *Aurum fulminans*, of our own preparing, loosely tied up in a piece of thin paper, we found, that after the powder had lain long enough upon the iron, to be thoroughly heated, it went off all together, and with a considerable flash.

58. Upon a thick, metalline plate, we put a convenient quantity of flowers of sulphur; and, having kindled them in the air, suddenly convey'd them into a receiver, and made haste to pump out some of the included air; as soon as the pump began to be ply'd, the flame appear'd to be sensibly decay'd; and continued less at every exsuction of the air; and, in effect, expired before the air was quite drawn out. And, upon the sudden removal of the receiver, it only afforded, for a very little time, somewhat more smoke in the open air, than it appear'd to do before.

Flame difficultly
preserved, with-
out air, in sul-
phur.

59. Upon a larger cylinder of iron, than the former, made red-hot, we let down a moderate lump of brimstone, in a receiver moderately exhausted; when, being kindled, it sent up a great flame, with large fumes.

How-

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However, we still ply'd the pump, drawing out, together with the air, much sulphureous, and offensive smoke; whereby, though the flame seem'd somewhat gradually impair'd, yet it manifestly continued burning much longer than, by the short duration of other flames in our receivers, one could expect. And once, particularly, in making this experiment, the flame lasted, till the receiver was judg'd to be thoroughly exhausted; and some thought it so surviv'd the exhaustion, that it went not out for want of air-fewel; the brimstone appearing, when we took off the receiver, either to have been consumed by the fire that fed on it, or to have casually ran off from the iron, the heat whereof had kept it constantly melted.

A durable flame
of a metalline
substance in va-
cuo.

60. Having obtain'd a saline spirit, which, by an uncommon way of preparation, was made exceeding sharp, and piercing, we put into a vial, capable of containing three or four ounces of water, a convenient quantity of new filings of pure steel; which, being moisten'd in the vial, with a little of the saline menstruum, were, afterwards, drench'd with more; whereupon the mixture grew very hot, and yielded large and fetid fumes. And so inflammable was this smoke, that, upon the approach of a lighted candle, it would readily take fire, and burn with a bluish, and somewhat greenish flame, at the mouth of the vial, for a considerable time together; and that, tho' with little light, yet with more strength than one would easily suspect.

This flaming vial, therefore, we convey'd into a receiver, which he who used to manage the pump affirm'd, would be exhausted by about six ex-suctions; and the receiver being well cemented on, upon the first suck, the flame suddenly appear'd four or five times as great as before; because, as we supposed, upon the withdrawing of the air, and, consequently, the weakening of its pressure, numerous bubbles were produced in the menstruum; which breaking, supply'd the neck of the vial with inflammable steams; and these, we thought, took fire, with some noise. Upon the second exsuction of the air, the flame blazed out, as before; and so it, likewise, did upon the third; but, after that, it went out: nor could we re-kindle any fire, by suddenly removing the receiver; only we found, that there remain'd such a disposition in the smoke to inflammability, that holding a lighted candle to it, a flame quickly ensued.

The flame of
spirit of wine
impregnated
with a metal
in vacuo.

61. Having so united highly rectified spirit of wine with a prepared metal, that they would afford a visibly ting'd flame; we put this mixture into a small glass lamp, furnish'd with a very slender wick, which the mixture would not burn, whilst there was liquor enough left to moisten it well; and putting this lighted lamp into a convenient part of a receiver, able to hold two gallons of water, we made haste to cement on the glass to the engine; yet found not, in two or three several trials, that, after the pump began to be work'd, so little a quantity of ting'd flame lasted more than half a minute.

We also observ'd, in repeating this experiment, that when the flame began to decay, the turn-key, being now and then drawn almost out, the ting'd flame once lasted a minute and half, and another time longer; that

longer ; that the turn-key being, from the first, taken out, the flame lasted two minutes ; that, in the same case, a pipe being bedded in the cement, at the bottom of the glass, and open at both ends, each almost as big as the orifice fill'd by the turn-key, the ting'd spirit seem'd to burn as if the flame would have lasted very long, had we permitted it ; and lastly, that the orifice, at the top, being stopp'd with the turn-key, tho' the pipe were left open at the bottom, it plainly, in a short time, seem'd greatly to decay, and ready to expire ; but causing one to blow in gently at the pipe, with a pair of bellows, tho' this did not keep the flame vigorous, yet it continued alive for above four minutes ; and then observing it to be manifestly stronger than it was, when we began to refresh it with the bellows, we ceas'd from blowing, and found, that tho' the glass pipe was still left open, yet, within about one minute, the flame entirely vanish'd.

62. Eminent writers, both ancient and modern, tell us, without scruple, that naphtha and camphire will burn under water ; but I had never the good fortune to see them do so ; and doubt, these writers deliver not what they affirm from experience. And tho', in celebrated authors, I have met with many compositions, that will not only burn under water, but be kindled by it ; yet I found those I have had occasion to consider, to be so lamely, or so darkly, and some of them, I fear, so falsely set down, that by the following composition, how slight soever it may seem, I have been able to do more than with things they speak very promisingly of.

Flame preserv'd under water.

We took of gun-powder, three ounces ; of well burn'd charcoal, one dram ; of good sulphur or flower of brimstone, half a dram ; of choice salt-peter, a dram and half : these ingredients being reduced to powder, and diligently mix'd without any liquor, we fill'd a large goose-quill with it ; for the kindling whereof, the open orifice of the quill, or pipe, was carefully stopp'd with a convenient quantity of the same, made up with as little chymical oil, or water, as would bring it to a fit consistence. This wild-fire we kindled in the air ; and the quill, together with a weight to which 'twas tied, to keep it from ascending, we slowly let down to a convenient depth, under water ; where it would continue to burn, as appear'd by the great smoke it emitted, and other signs, as it did in the air ; because the shape of the quill kept the dry mixture from being accessible to the water, at any other part than the orifice ; and there the stream of fired matter issued out with such violence, as incessantly beat off the neighbouring water, and kept it from entering into the cavity that contain'd the mixture, which, therefore, would continue burning, till 'twas consumed.

63. In trying to kindle a combustible substance, in our exhausted receiver, it happen'd to fall beside the iron, whereby our intended experiment was defeated ; but whilst we were considering what was to be done on this occasion, and had not yet let in the air, nor brought in the lights that were removed out of the room, we were surprized to see something burn like a pale, bluish flame, almost in the midst of the cavity of the receiver ; and, at first, suspected it to be some deception of the sight : but, all the by-

An odd phenomenon of the flame of a metal in vacuo.

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standers perceiving it alike, and observing that it grew very broad, we look'd at it with great attention, and found it to last much longer, than I remember to have seen any flame in an exhausted receiver. I should have expected that it proceeded from some brimstone sticking, unobserv'd, to a part of the iron we had formerly employ'd to kindle sulphur, had we not, just before, kept it red-hot in the fire. But tho' we much wonder'd whence this flame proceeded, we did not hasten its extinction; and at length, when it expired of itself, we let in the air, and perceiv'd, upon the concave part of the iron, which we judg'd to be the place where the flame had appear'd, a piece of melted metal, supposed to have been fasten'd to the string where-to the fewel we design'd to kindle, had been tied, in order to let it down the more easily; and this made us conceive, that the string happening to be burned, by the excessive heat of the iron, the piece of metal fell into the cavity of it; and, that by the same heat, the more combustible part, which the chymists call the sulphur, was melted, and kept on fire, and continued burning, as we have related. The piece of metal was judg'd to be lead, but having not, formerly, observ'd such a disposition in lead, to be inflam'd, I consider'd it attentively, and perceiv'd, that 'twas some fragment of a mixture of lead and tin, that I caus'd to be melted in a certain proportion. Upon this account, it seems, the mixture of the ingredients had acquired such a new texture, as fitted the mass to afford this odd phenomenon; which argues, that there may be flames of metalline sulphurs produced as easily, without the concurrence of the air, as that of common sulphur; and continue to burn longer than that in our vacuum.

Actual flame propagated with difficulty in vacuum.

64. Having placed our cylindrical plate of iron, first brought to be red-hot, in a receiver, capable of containing two gallons of water; and having, also, diligently pump'd out the air, we kindled a little sulphur, upon the heated plate; and then a piece of dry'd spunk, tied to a string, was let down to the flame. When the experiment was finish'd, and the spunk taken out, we found it, in several places, not manifestly alter'd so much as in colour; and, in those parts that had been most expos'd to the flame, it was turn'd to a substance very different from ashes; being black, and brittle as tinder, and, like that, exceedingly disposed to kindle, upon the touch of fire.

An attempt to make flame kindle camphire, without the help of air.

65. Into the same receiver, we convey'd the same cylindrical plate of iron; and, when the air had been thoroughly pump'd out, we let a piece of such brimstone down upon the hot iron, as would there kindle with the heat. A little above this sulphur, we had tied to the same string, a piece of camphire; that being a body exceedingly apt to take fire, or, as it were, to draw it at the flame of lighted brimstone: but our sulphur, melting with the heat of the iron, dropp'd from the string 'twas fasten'd to. As soon as it came to the bottom, where it was distant from the vehement heat of the metal, the flame expired; but a part of it, that happen'd to stick to the side of the iron, was inflam'd by it, and the flame reach'd the camphire, without being able to make it blaze.

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We, also, attempted to kindle one piece of sulphur *in vacuo*, by the flame of another, tied a little lower on the same string, that it might first touch the heated iron, and be thereby set on fire; but tho' we could find nothing amiss in the kind of sulphur, we then used, yet we were not able, even by a repeated trial, to make it take fire upon the iron; where, nevertheless, it melted, and seem'd, a little to boil.

A third trial was not so unsuccessful; for, having, in the receiver, well exhausted, let down a card-match, upon a very hot iron, the lower extreme of it was kindled thereby. But though the sulphurated part of the match thus flamed away, yet the remaining part, which was a mere piece of card, was not thereby turn'd into flame; nor, in most places, so much as sensibly scorch'd, or black'd, though it had been purposely dry'd before-hand.

66. Upon a paper, laid on a convenient part of the plate of the engine, we made a train of dry powder, as long as the glass would well cover; then, carefully fastening on the receiver, we exactly pump'd out the air; which done, we took a good burning-glass, and, about noon, cast the sun-beams thro' it, upon a part of the train; but the indisposition of the powder to fire was so great, that it smoked, and melted, without going off. We afterwards employ'd a thinner, and more transparent receiver, which so little weaken'd the sun's rays, that being kept obstinately upon the same part of the train, they were able to fire several parts, one after another, tho' they could not cause the flame to propagate; only those parts that were melted, did, at length, kindle, and fly away, leaving the rest unalter'd: so that I found several little masses of dissolved matter, in several parts of the train, with the powder unchang'd in all the others. And some of these masses were contiguous to grains of the powder, which both appear'd unchang'd, and kindled readily, and flash'd all away, as soon as I caus'd the burning-glass to be applied to them in the open air.

67. For farther confirmation of so odd an experiment, I shall add, that to try whether by the help of one of those little instruments, wherewith the strength of powder is commonly examin'd, we could find any difference made by the absence and presence of the air, in the resistance of the instrument, or the effects of the powder on it; we fasten'd it to a competently heavy, and commodiously shaped weight of lead: and when 'twas carefully fill'd, and primed with powder, we placed it in a receiver of a convenient bigness; whence we pump'd out the air after the usual manner, tho', perhaps, with more than usual diligence. But tho', at length, after the powder had long resisted the beams of the sun, thrown on it by a good double convex-glass, it took fire at the touch-hole, and fill'd the receiver with smoke; yet this kindled powder could not propagate the flame, to that which was in the box, how contiguous soever the parcels were to one another: though, when the instrument was taken out into the air, where the touch-hole appear'd to be free; as soon as ever new priming, with the same sort of powder, was put in, the whole very readily went off. And when we caus'd the instrument to be new charged; and, upon its

X x x

firing

Gun-powder, tho' fired itself, fires not the contiguous grains in vacuo.

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firing only at the touch-hole in the exhausted receiver, order'd new priming to be added, without so much as taking the instrument out of the glass; tho', afterwards, this was clos'd again, but, without being exhausted, the powder, closely shut up in the glass, readily went off; as well that which was in the box, or cavity, of the powder-tryer, as that which lay on the outward part of it. And this experiment was repeated, with the like success.

Two different trials with different events, to kindle gun-powder in vacuo.

68. A few corns of gun-powder, being included in a very small bubble, freed from air, and secured against the return of it, and then apply'd warily to coals cover'd with ashes, did neither go off, nor burn; but afforded a little yellow powder, that seem'd to be sulphur, sublimed to the upper part of the glass. But two larger bubbles, tho' strong, whereof one had the air but in part, and the other totally evacuated, being provided, each of them, with a greater quantity of powder; a while after they were put upon quick-coals, they were both blown to pieces, with a report almost like that of a musquet: but, tho' this was done in a dark place, yet we did not perceive any real flame produced.

Experiments, showing the relation betwixt air, and the Flamma vitalis of animals; and, first, an animal, included with the flame of spirit of wine.

69. We put a spoonful of highly rectify'd spirit of wine, into a small glass-lamp, conveniently shaped, and purposely blown, with a very small orifice, at which we thrust in a slender cotton-wick; we, also, provided a tall glass-receiver, in length eighteen inches, that contain'd above twenty pints of water. This receiver, which was open at both ends, had its upper orifice cover'd with a brass-plate, fasten'd on very close with good cement; and, for the lower orifice, which was far the widest, we had provided a brass-plate, furnish'd with a competent quantity of the cement we employ'd to keep the air out of the pneumatical engine; by means whereof, we could sufficiently close the lower orifice of our receiver, and hinder the air from getting in at it. We, then, lighted up the small glass-lamp, and placed it, together with a green-finch, upon the brass-plate, and, in a trice, fasten'd it to the lower orifice of the receiver, and then watch'd the event; which was, that, within two minutes, the flame, after having, several times, almost disappear'd, was utterly extinguish'd: but the bird, tho', for a while, he seem'd to close his eyes, as tho' he were sick, appear'd lively enough, at the end of the third minute, when I caus'd him to be taken out.

After he had, by being kept in the free air, recover'd, and refresh'd himself, the former trial was repeated; and, at the end of the second minute, the flame of the lamp went out: but the bird seem'd not to be endanger'd, by being detain'd a while longer.

After this, we put in, with the same bird, two lighted lamps at once, whose flames lasted not one whole minute, before they went out together; but the bird appear'd unhurt, after having been kept five or six times as long, before we took off the receiver.

In the tall receiver, above-mention'd, we included a mouse, with a lighted lamp, fill'd with the spirit of wine; but, before the experiment was near finish'd, the mouse, being at liberty within the glass, made shift to

extinguish the flame; which, being revived, without taking out either the lamp, or the animal, the spirit of wine burned about a minute longer; during which time, the mouse appear'd not be grown sick, no more than when, for some minutes after the extinction of the flame, he had been kept in the same close and infected air.

We, afterwards, placed the same mouse in another receiver, which seem'd less, by a third, than the former; and in it we, also, fix'd a piece of slender wax-candle, which continu'd burning, in this receiver, but for one minute; and, during that, it emitted much smoke: the animal, nevertheless, appear'd lively, even after we had kept him much longer in that infected air. And the same candle, without being taken out, was lighted again, but burned not so long as before; yet it sufficed to darken the receiver, and, therefore, probably, much clogg'd the included air: in which, nevertheless, the mouse being kept for eight or ten minutes longer, he appear'd, neither when taken out, nor a while before, to have receiv'd any considerable harm from his detention.

70. We included a green-finch, and a piece of lighted candle, in a great capp'd receiver, capable of containing two gallons of water, and very carefully cemented on to the pump: in this glass, we suffer'd the candle to burn, till the flame expired, which it did within less than two minutes; whilst the bird seem'd to be in no danger of sudden death; and, tho' kept a while longer in that clogg'd and smoaky air, he appear'd well, when the receiver was remov'd. We, afterwards, put the same bird into the receiver, with a piece of a small wax-taper; whose flame, tho' it lasted longer than the other, yet the bird out-liv'd it: and, 'twas judg'd, he would have done so, tho' the flame had been much more durable. After this, we included the same bird, with the former candle, in the receiver, which we had caus'd to be often blown into with a pair of bellows, to drive out the smoke, and infected air; and, then, beginning to pump, we found, that the flame began to decay more suddenly, and the bird to be much more discompos'd, than in the former experiments: but still he surviv'd the flame, tho' not without convulsive motions.

We repeated the experiment with a piece of wax-taper, and the same bird, which, tho' cast into dangerous symptoms, upon the gradual evacuation of the air, out-liv'd, not only the flame, but the smoke too, that issued from the kindled wieck; a circumstance that was, also, observ'd in the preceding trial. Lastly, having freed the receiver from smoke, and supply'd it with fresh air; we put in, with the same bird, a piece of charcoal, of about two inches in length, and half an inch in breadth, which, just before, had been well blown with a pair of bellows; immediately pumping out the air, till none of the fire could be discern'd, and till it seem'd irrecoverable, by the admission of the outward air; which being, afterwards, admitted, the bird was, indeed, very sick, yet capable of a very quick recovery. And this experiment we, with the same animal, and re-kindled coal, made over again, with the same success.

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 Glow-worms,
 and their lumi-
 nous matter, in
 vacuo.

71. We took two glow-worms, that shone vividly, especially one of them, whose light appear'd strong, and ting'd, as if it had been transmitted thro' a blue glass; these we laid upon a little plate, which we included in a small receiver, of finer glass than ordinary; and, having remov'd the candles, that no other light might obscure that of the insects, we waited in the dark, till it was conspicuous, and then order'd the air to be pump'd out; and, upon the very first exsuction, there began to be a diminution of the light, which grew gradually dimmer, as the air was more withdrawn; till, at length, it quite disappear'd. This darkness, having been suffer'd to continue a long while in the receiver, we let in the air again, whose presence restor'd, at least, as much light as its absence had depriv'd us of. This experiment was repeated, with the addition of one more of those insects; when they all three gradually lost their light, by the exhaustion of the receiver, and regain'd it, by the return of the air. And here we let in the air by degrees, and with an interval or two; and observ'd, that as the light was gradually diminish'd, upon withdrawing the air; so the returning splendor was gradually increas'd, as we let more in upon the worms.

72. 'Tis known, that if glow-worms be kill'd, whilst they are shining, their luminous matter may continue to shine long after 'tis taken out of their bodies. And, having put some of that, we took out of the fore-mention'd insects, upon a little paper, and included it in the receiver we employ'd; the candles being remov'd; we perceiv'd it to shine vividly, before the pump was set on work; and, afterwards, to grow dimmer, by degrees, as the air was exhausted, till, at length, it quite vanish'd: but it re-appear'd immediately upon the air's return. This experiment was repeated twice more, with the same success. But we took notice, that the luminous matter, after the air was let in, seem'd not only to have regain'd its former degree of light, but to have acquir'd a greater; as it once happen'd, also, in the experiment made on the living worms. It was somewhat strange, to observe, that so very small a quantity of air, as we at first let in, before the light revived, was sufficient to make it become plainly visible, tho' dim: in which state it continu'd, till we thought fit to admit more air.

73. Having, at another time, procur'd two more of those insects, whereof one was judg'd to be as large as three ordinary ones; when we had brought them out of the country to *London*, the great worm appear'd to be dead; but, finding him to retain a considerable degree of light, in the under part of his tail, we put him into the small receiver, formerly mention'd, to try whether, after the death of the animal, the shining matter would retain its former properties; and, at the first stroke of the pump, the light was not abolish'd, but continu'd vivid: and so it did, when the air being let in, and again withdrawn, the trial was made a second time. I, afterwards, caus'd the receiver to be exhausted, once or twice more; and, at length, perceiv'd, that the light began to diminish, as the air was withdrawn; and, at last of all, it so disappear'd, that we could not see it: but, upon the re-admission of the air, the light shone vividly, as before, if not more bright.

This

This experiment was repeated, with the same success, and both times, the like happen'd to the light of the dead one, and of the living one, that we included with it; tho' there was this disparity betwixt them, that the luminous part of the dead worm appear'd much larger than that of the living one: and the light of the latter was of a very greenish blue; but of the former, a white yellow.

74. A mouse, weighing about three drams and an half, being put in one scale of a very nice balance, was counterpois'd, together with a string, put about his neck in a noose; and soon after, by drawing the ends of that, we strangled him: when we judg'd him quite dead, we weigh'd him again; and, tho' nothing was seen to fall from him; yet, contrary to the receiv'd tradition, that bodies are much heavier dead than alive, we found his weight diminish'd about $\frac{1}{7}$ of a grain; which, probably, proceeded from the avolation of several subtile particles, upon his violent and convulsive strugglings in death.

Animals weigh'd before death, and after.

Afterwards, in a larger balance, made for nice experiments, we took a very young kitten, between ten and eleven ounces in weight, and caus'd him to be strangled on the same scale, wherein he had been put. But, being not immediately dispatch'd, as young animals of this kind are not easily destroy'd for want of respiration; we found him, by that time he was quite dead, lighter, by four grains.

75. Nature, having furnish'd water-fowl with a peculiar structure of some vessels about the heart, to enable them, when they dive, to suspend, for a while, the act of respiration under water, without prejudice; I thought fit to try, whether such animals, would, much better than others, sustain the want of the air in our exhausted receiver.

Experiments to show the nature of respiration made upon ducks.

We put a full grown-duck into a receiver, whereof she fill'd, about a third part; but was unable to stand in any easy posture therein: then pumping out the air, tho' she seem'd, at first, to continue well, somewhat longer than a hen in her condition would have done; yet, within one minute, she appear'd much discompos'd, and, between that and the second minute, her convulsive motions encreased so much, that, her head, hanging carelessly down, she seem'd to be just at the point of death; from which, we presently rescu'd her, by letting in the air. And, to manifest, that it was not the closeness and narrowness of the vessel, that produced this great, and sudden change, we, soon after, included the same bird in the same receiver; and, having cemented it on very close, we suffer'd her to stay, thus shut up with the air, five times as long as before, without perceiving her to be discompos'd; and, she might, probably, have continued longer in the same condition.

76. Having procured a duckling, that was yet callow, we convey'd her into the same receiver, wherein the former had been included, and observed, that, tho', for a while, she appear'd not much disorder'd, whilst the air was pumping out; yet, before the first minute was ended, she gave manifest signs of being much discomposed: and the operation being continued a while longer, convulsive motions ensued so fast, that, before the second

PRELIMINARY. second minute was expired, we were obliged to let in the air, whereby she quickly recover'd.

When the receiver was pretty well exhausted, the included bird, appear'd manifestly bigger, than before the air was withdrawn, especially about the crop; tho' that was very turgid before. We, also, kept the same duckling, in the same receiver, very close, for above six minutes, without perceiving her to grow sick upon her imprisonment; which, yet lasted above thrice the time, that before sufficed to reduce her to a gasping condition.

It not being intended, that water-fowl, should, any more than other birds, live in an exceeding rarified air, but, only be able to continue, upon occasion, under water, it may suffice, that the contrivance of these parts relating to respiration, be barely fitted for that purpose.

Upon vipers.

Vipers being endowed with lungs, tho' of a different structure from those of other animals; and their blood being, as to sense, actually cold; I thought, it might be worth trying, what effect the absence of the air would have upon them.

77. Jan. 2. We included a viper in a small receiver, and as we drew out the air, she began to swell, and afforded us these phenomena.

1. It was a long while after we had left pumping, before she began to swell, so much as forced her to gape, which, afterwards, she did.

2. She continued, above two hours and a half, in the exhausted receiver, without giving clear proof of her being killed.

3. After she was once so swelled, as to be compell'd to open her jaws, she appear'd slender and lank again; and yet, very soon after, appear'd swell'd again, and had her jaws disjoin'd as before.

78. Including a viper in a small receiver, we emptied it very carefully; when the viper moved up and down within, as if it were to seek for air; and, after a while, foamed a little at the mouth, and left of the foam, sticking to the inside of the glass: her body swelled, not considerably, and her neck less, till a pretty while after we had left off pumping; but afterwards the body and neck grew prodigiously tumid, and a blister appear'd upon her back. An hour and an half after the receiver was exhausted, the distended viper, gave, by motion, manifest signs of life; but, we observed none afterwards. The tumor reach'd to the neck, but did not seem much to swell the under-jaw. Both the neck, and a great part of the throat, being held betwixt the eye and the candle, were transparent, where the scales did not darken them. The jaws remain'd mightily open'd, and somewhat distorted; the *Epiglottis*, with the *Kimula Laryngis*, (which remain'd gaping) was protruded almost to the further end of the under-jaw. As it were, from beneath the *Epiglottis*, came the black tongue, and reached beyond it, but seemed, by its posture, not to have any life; and the mouth also was grown blackish within: but, the air being re-admitted, after twenty-three hours in all, the viper's mouth was presently closed, tho', soon after, it was opened again, and continued long so; whilst scorching or pinching the tail, made a motion in the whole body, that argued some life.

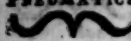
79. April

79. *April 25.* We included an ordinary, harmless snake, together with a gage, in a portable receiver, which, being exhausted, and well secured against the ingress of the air, was laid aside in a quiet place, where it continued from about ten or eleven a-clock in the forenoon, till about nine the next morning: and then, looking upon the snake, though he seemed to be dead, and gave no signs of life, upon the shaking of the receiver, yet, upon holding the glass, at a convenient distance, from a moderate fire, he, in a short time, manifested himself alive, by several tokens; and even by putting out his forked tongue. In this condition I left him, and came not to look upon him again, till the next day, early in the afternoon; at which time, he was grown past recovery, and his jaws, which were formerly shut, gaped exceeding wide, as if they had been stretched open by some external violence.

80. *Sept. 9.* We included a large, lusty frog, in a small receiver, drew out the air, and left her not very much swell'd, and able to move her throat; tho' not so fast as when she breathed freely, before the extraction of the air. She continued alive for about two hours, that we took notice of, sometimes removing from one side of the receiver to the other; she swell'd more than before, and did not appear, by any motion of her throat, or thorax, to exercise respiration; but her head was not very much swell'd, nor her mouth forced open. After she had remained there above three hours, perceiving no sign of life in her, we let in the air, upon which the tumid body shrunk very much, but seemed not to have any other change wrought in it; and tho' we took her out of the receiver, yet, in the free air, she continued to appear stark dead. But, having caused her to be laid upon the grass, in a garden, all night, we found her, the next morning, perfectly alive again.

81. *June 29.* About eleven of the clock in the fore-noon, we put a frog into a small receiver, containing about fifteen ounces, and one quarter, troy-weight, of water; out of which we had, tolerably well, drawn the air: (so that when we turn'd the cock under water, it suck'd in about thirteen ounces, and one quarter, of water,) the frog continued, the receiver being all the while under water, lively, till about five of the clock in the afternoon, when she expired. At the first she seemed not to be much alter'd by the extraction of the air, but continued breathing, both with her throat and lungs.

82. *Sept. 6.* We included into a pretty large receiver, two frogs newly taken; the one not above an inch long, and proportionably slender; the other, very large and lusty. Whilst the air was drawing out, the lesser frog skip'd up and down very lively, and, several times, clamber'd up the sides of the receiver, so that he sometimes wrested himself against the sides of the glass. When his body seemed to be perpendicular to the horizon, if not in a reclining posture, he continued to skip up and down a while, after the extraction of the air; but, within a quarter of an hour, we perceived him to lie stark dead, with his belly upwards. The other frog, that was very large and strong, tho' he began to swell much upon withdrawing the air, and

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seemed to be distress'd, yet he held out half an hour ; when it was remarkable, that the receiver, though it had withstood the pressure of the outward air, during that space, notwithstanding a piece of it had been crack'd out, but cemented in again, yet at the end of the half hour, the weight of the outward air suddenly beat it in, and thereby gave the imprison'd frog relief.

83. *Sept. 11.* We convey'd a small frog into a very small portable receiver, and began to pump out the air. At first she was lively, but when the air was considerably withdrawn, she appear'd to be very much disquieted ; yet not so, but that, after the operation was ended, and the receiver taken off, she was perfectly alive, and continued to appear so, for near an hour, tho' the abdomen was very much, and the throat somewhat extended ; the latter having, also, left off the usual panting motion, which is supposed to argue and accompany the respiration of frogs. At the end of about three hours and a quarter, after the removal of the receiver from the pump, the air was let in ; whereupon the abdomen, which, by that time, was strangely swell'd, not only subsided, but seemed to have a great cavity in it, as the throat, also, proportionably had ; which cavities continued after the frog was gone past all recovery.

84. *April 14.* A large frog was convey'd into a plated receiver, and the air being withdrawn, her body, by degrees, distended. The receiver, with the gage, was kept under water near seven hours ; at the end of which, I found it tight, but the frog dead, and exceedingly swell'd : upon letting in the air, she became more hollow and lank than ever.

Kitlings.

85. We took a kitling one day old, and put him into a very small receiver, that it might be the sooner exhausted ; and within about one minute after the air first began to be withdrawn, the little animal, which, in the mean time, gasped for life, and had some violent convulsions, lay as dead, with his head downwards, and his tongue out ; but, upon letting in the air, he, in a trice, shew'd signs of life ; and, being taken out of the receiver, quickly recover'd. We then sent for a kitling of the same litter, which being put into the same receiver, quickly began, likethe other, to have convulsions, and after to lie as dead ; but, observing very narrowly, I perceived some little motions, which made me conclude him alive. And accordingly, tho' we continued pumping, and could not perceive that the engine leaked, the kitling began to stir again, and, after a while, had stronger and more general convulsions than before ; till at the end of full six minutes after the exsuction of the air was begun, the animal seeming quite dead, the outward air was re-admitted into the receiver ; which not reviving him, as it had done the other, he was taken out of the vessel, and lay with his mouth open, and his tongue lolling out, without any sensible respiration and pulsation ; till having order'd him to be pinched, the pain, or some internal motion, produced by the external violence, made him, immediately, give manifest signs of life ; tho' there was yet no sensible motion of the heart, or lungs : but afterwards gaping, and fetching his breath in an odd manner, and with much straining, as I have seen some *fœtus's* do, when cut out of the womb, he,

he, by degrees, within about a quarter of an hour, recover'd. We, afterwards, sent for another, kittend at the same time; and inclosing that, also, in the receiver, observ'd the violent convulsions, and, as it were, gasping for breath, into which he began to fall, at the second or third suck, ended in a seeming death, within about a minute and a half. But, causing the pump to be ply'd, the kitling gave manifest signs of life, after he had endured several convulsions, as great as those of the first fit; if not greater. When seven minutes, from the beginning of the exhaustion, were compleated, we let in the air; upon which, the little creature, that seem'd stark dead before, made us suspect he might recover: but, tho' we took him out of the receiver, and put *Aqua Vita* into his mouth, yet he, irrecoverably, died in our hands.

86. To determine the quantity of air harbour'd in the pores of fluids, seems as difficult as it would be useful. To shew how little the air, contain'd in water, would appear to lessen the bulk of the water, if it were suffer'd to fly away in an open tube; we let it escape, in an exhausted receiver, without any artifice to catch it: in which trial, the water did not part with any thing of its bulk, that made a diminution sensible to the eye. We, therefore, endeavour'd to make this loss visible, by some other experiments.

A glass-tube, seal'd at one end, and about thirty-six inches in length, being fill'd with water, and inverted into a glass-vessel, not two inches in diameter, and but a quarter of an inch, or little more, in depth; the glasses were convey'd into a fit receiver, and the air leisurely pump'd out, and somewhat slowly re-admitted; when, the numerous bubbles, that had ascended, during the operation, constituted, at the top, an aerial aggregate, amounting to $\frac{8}{10}$, wanting about an hundredth part of an inch.

87. Presently after, another tube was fill'd with the same water, and inverted; when, the water, being drawn down to the surface of that in the vessel, and the air let in again, the water was impell'd up to the very top, within a tenth, and half a tenth of an inch.

The latter tube was forty-three inches and a half above the surface of the stagnant water; the air, collected out of the bubbles, at the top of the water, was, the first time, above three quarters of an inch; and the second time we estimated it, at one half, and one sixteenth. The first time, the water, in the pipe, was made to subside full as low as the surface of the stagnant water; the second time, the lowest that we made it subside, seem'd to be four or five inches above the surface of the water in the open vessel.

But the air, at the top of the tube, possess'd more room than its bulk absolutely required; because it was somewhat defended from the pressure of the atmosphere, by the weight of the subjacent cylinder of water, which, perhaps, was about three or four feet long.

88. We provided a clear round glass, furnish'd with a pipe, or stem, about nine inches in length; the globular part of the glass being, on the outside, about three inches and a half in diameter: the pipe of this glass

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was,

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was, within an inch of the top, melted at the flame of a lamp, and drawn out, for two or three inches, as slender as a crow's quill, that the decrease of the water, upon the recess of the air, harbour'd in its pores, might be the more easily observ'd, and estimated. Above this slender part of the pipe, the glass was, nearly, of the same size with the rest of the pipe; that the aerial bubbles, ascending thro' the slender part, might there find room to break, and so prevent the loss of any part of the water.

This vessel being filled, till the liquor reach'd to the top of the slender part; where, not being uniformly enough drawn out, it was somewhat broader than elsewhere; we convey'd the glass, together with a pedestal for it to rest upon, into a tall receiver; and, pumping out the air, there disclosed themselves numerous bubbles, ascending nimbly to the upper part of the glass, where they made a kind of froth: but, by reason of the figure of the vessel, they broke at the top of the slender part, and so never came to overflow.

This done, the pump was suffer'd to rest a while, to give the aerial particles, lodg'd in the water, time to separate themselves, and emerge; when, the pump was ply'd again. These vicissitudes of pumping, and resting, lasted for a considerable time; till, at length, the bubbles began to be very rare: soon after which, the external air was let into the receiver; when, it appear'd somewhat strange, that, notwithstanding so great a multitude of bubbles as had escaped out of the water, I could not, by attentively comparing the place where the surface of the water rested at first, (to which a mark had been affix'd) with that where it now stood, discern the difference to amount to above an hair's breadth: and the chief operator in the experiment profess'd he could perceive no difference at all.

89. Filling a glass of the same shape, and much of the same bigness as the former, with claret-wine, and, placing it upon a convenient pedestal, in a tall receiver, we caus'd some of the air to be pump'd out; whereupon there emerged, thro' the slender pipe, so very great a multitude of bubbles darted, as it were, upwards, as both pleas'd and surpriz'd us; but forc'd us to go warily to work, for fear the glass should break, or the wine overflow: wherefore, we, seasonably, left off pumping, before the receiver was near exhausted, and suffer'd the bubbles to get away as they could, till the danger was past: then, from time to time, we pump'd a little more air out of the receiver; the withdrawing a moderate quantity of air at a time sufficing, even at the latter end, to make the bubbles copiously and swiftly ascend, for above a quarter of an hour together.

The little instrument made use of in these experiments, being design'd to examine, among other things, the quantity of bubbles lurking in several liquors, may be apply'd to spirit of wine, and chymical oils. And some circumstances of our trials made us think, that (it might be worth examining, what kind of substance may be obtain'd by this way of treating aerial and spirituous corpuscles.

90. An oyster, being put into a very small receiver, and kept there long enough to have, successively, kill'd three or four birds, or beasts, &c. was not thereby kill'd, nor, for ought we could perceive, considerably disturbed; only at each suck we perceiv'd, that the air, contain'd between the two shells, broke out at their commissure; as we concluded from the foam which, at those times, came out all around that commissure. About twenty-four hours after, I found, that both this oyster, and another, that had been put, at the same time, into the receiver, were alive.

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Shell-fish in an exhausted receiver.

On the same day we put a pretty large craw-fish into a large receiver, and found, that tho' he had been before injur'd by a fall, yet he seem'd not to be much incommoded, by being included, till the air was, in great measure, pump'd out; and then his former motion presently ceas'd, and he lay as dead, till, upon letting in a little air, he soon began to move afresh; and, upon withdrawing the air again, he presently, as before, became moveless. Having repeated the experiment two or three times, we took him out of the receiver, when he appear'd not to have suffer'd any harm.

91. Having put an oyster into a vial, full of water, before we included it in the receiver, that thro' the liquor the motion of the bubbles, expected from the fish, might be the better seen, and consider'd; this oyster prov'd so strong, as to keep itself close shut, and repress'd the eruption of the bubbles, that, in the other, forc'd open the shells, from time to time; and kept in its own air, as long as we had occasion to continue the trial.

92. A craw-fish, that was thought more vigorous, being substituted in the place of the former craw-fish, tho' once he seem'd to lose his motion together with the air; yet, afterwards, he continu'd moving in the receiver, in spite of our pumping.

93. We took a receiver, shaped almost like a bolt-head, containing near a pint; and the globular part of it, being almost half full of water, we put into it, at the orifice, a small gudgeon, about three inches long; which, when it was in the water, swam nimbly up and down therein: then, having drawn out the air, so that about nineteen parts of twenty, or more, were exhausted; we secur'd the return of the air from injuring our experiment.

A scale-fish in an exhausted receiver.

Now the neck of the glass, being very long, tho' there appear'd numerous bubbles all about the fish; yet the rest of the water, notwithstanding the withdrawing of so much air, emitted no froth, and but few bubbles.

The fish, both at his mouth and gills, for a long time, discharg'd such a quantity of bubbles, as appear'd strange; and for about half an hour, when ever he rested a while, new bubbles would adhere to many parts of his body, (as if they were generated there) especially about the fins and tail; so that he would appear almost beset with bubbles: and if, being excited to swim, he was made to shake them off, he would quickly, upon a little rest, be beset with new ones, as before.

Almost

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Almost all the while, he would gape, and move his gills, as before he was included; tho', towards the end of the time, I watch'd him, he often neither took in, nor emitted any aerial particles that I could perceive.

After a while, he lay almost constantly with his belly upwards; and, yet, would, in that posture, swim briskly, as before. Nay, soon after, he seem'd to be more lively than at first putting in.

In about an hour and a half after he had been seal'd up, I found him almost free from bubbles, with his belly upwards, and seeming somewhat tumid, yet lively as before. But, an hour and a quarter after that, he seem'd to be moveless, and somewhat stiff; yet, upon shaking the glass, observing faint signs of life in him, by some languid motions he attempted to make, when excited; I open'd the receiver, under water, to try if that liquor, and air, would recover him; when, the external water rushing in, till it had fill'd the vacant part of the ball, and the greatest part of the stem, the fish sunk to the bottom of it, with a greater appearance than ever of being alive: in which state, after he had continu'd a pretty while, I, by the help of the water he swam in, got him thro' the pipe, into a basin of water, where he gave more manifest signs of life. But, yet, for some hours, he lay on one side or other, without being able to swim, or rest on his belly, which appear'd very much shrunk in.

All the while he continu'd in the basin of water, tho' he mov'd his gills, as before he had been seal'd up; yet I could not perceive, that he did, even in his new water, emit, as formerly, any bubbles; tho', two or three times, I held him by the tail in the air, and put him into the water again; where, at length, he grew able to lie constantly upon his belly; tho' that retain'd much of its former lankness. And he lived, in the basin, eight or ten days longer; tho' several gudgeons, since taken, died there, in a much less time.

Two animals,
with large
wounds in their
abdomen, in-
cluded in the
pneumatical
receiver.

94. Sept. 12. A small bird, having the abdomen open'd almost from flank to flank, without injuring the guts, was put into a little receiver, and the pump being set a-work, continued, for some little time, without giving any signs of distress; but at the end of about a minute and an half from the beginning, she began to have convulsive motions in the wings; and, tho' the convulsions were not universal, or appeared violent, as is usual in other birds when the air is withdrawn, yet, at the end of two full minutes, letting in the air, and then taking off the receiver, we found the bird irrecoverable, tho' there appear'd no notable alteration in the lungs; and the heart, or, at least, the auricles of it, continued beating for a while after,

95. We took, also, on the same day, a pretty large frog, and having, without violating the lungs, or the guts, made two such incisions in the abdomen, that the two curl'd bladders, or lobes of the lungs, came out, almost totally, at them; we suspended the frog, by the legs, in a small receiver, and, after we had pump'd out a large part of the air, the animal struggled very much, and seem'd to be much disorder'd; and, when the receiver was well exhausted, she lay still, for a while, as if she had been dead;

dead; the abdomen and thigh being very much swell'd, as if some rarify'd air, or vapour, forcibly distended them. But as, when the frog was put in, one of the lobes was almost full, and the other almost shrunk up; so they continu'd to appear, after the receiver had been exhausted: but, upon letting in the air, not only the body ceas'd to be tumid, but the plump bladder appear'd, for a while, shrunk up as the other; and the receiver being remov'd, the frog presently revived, and quickly began to fill the lobe again with air.

96. The heart of an eel being taken out, and laid upon a plate of tin, in a small receiver; when we perceiv'd it to beat there, as it had done in the open air, we exhausted the vessel, and saw, that tho' the heart grew very tumid, and, here and there, sent out little bubbles, yet it continued to beat as manifestly as before, and seem'd to do so more swiftly; as we tried by counting the pulsations it made in a minute, whilst it was in the exhausted receiver, and when we had re-admitted the air, and also, when we took it out of the glass, and suffer'd it to continue its motion in the open air. The heart of another eel, being, likewise, taken out, continued to beat in the empty'd receiver, as the other had done.

The motion of the separated heart of a cold animal in the exhausted receiver.

97. The heart of another eel, after having been included in an exhausted receiver, and then accurately secured from leaking, tho' it appear'd very tumid, continu'd to beat there an hour; after which, finding its motion very languid, and almost ceas'd; by breathing a little upon that part of the glass where the heart was, it quickly regain'd motion, which I observ'd a while; and, an hour after, finding it almost quite gone, I was able to renew it, by the application of a little more warmth. At the end of the third hour, a bubble, that appear'd to be placed between the auricle and the heart, seem'd to have, now and then, a little trembling motion; but it was so faint, that I could no more, by warmth, excite it, so as plainly to perceive the heart to move: wherefore, I suffer'd the outward air to rush in, but could not discern, that, thereby, the heart regain'd any sensible motion, tho' assist'd with the warmth of my breath and hands.

98. Sept. 10. A green-finch, having his legs and wings tied to a weight, was gently let down into a glass body fill'd with water; the time of his total immersion being mark'd. At the end of half a minute after that time, the strugglings of the bird seeming finish'd, when being suddenly drawn up again, he was found quite dead.

The times where in animals may be kill'd by drowning, or withdrawing of the air, compared.

A sparrow, very lusty and quarrelsome, was tied to the same weight, and let down after the same manner; but tho' he seem'd to be more vigorous under water than the other bird, and continued struggling almost to the end of half a minute, from the time of his total immersion; (during which, there ascended, from time to time, large bubbles from his mouth) yet, being drawn up as soon as ever the half minute was compleated, we found him, to our wonder, irrecoverably gone.

99. A small mouse, being held under water by the tail, emitted, from time to time, several aerial bubbles out of his mouth; and, at last, as a spectator affirm'd, at one of his eyes: being taken out, at the end of half a minute,

nute, and a few seconds, he yet retain'd some motions: but they prov'd only convulsive ones, which, at last, ended in death.

100. We so tied a considerable weight of lead to the body of a duck, as not to hinder her respiration, yet keep her under water; which we had found a small weight unable to do, by reason of her strength; and even a great one, if ty'd only to her feet, in such a middle-sized vessel as ours was; because of the height of her neck and beak. With this clog, the duck was put into a tub full of clear water, under the surface whereof, she continued quietly for about a minute; but afterwards began to be much disturb'd: the fit being over, and perceiving no motion in her, at the end of the second minute, we took her out of the water; and, finding her in a good condition, after we had allow'd her some breathing-time, to recruit herself with fresh air, we let her down again into the tub, which, in the mean time, had been fill'd with fresh-water; lest the other, which had been troubled with the steams and foulness of the body of the animal, might either hasten her death, or hinder our perceiving what should happen.

The bird being thus under water, after a while, began, and, from time to time, continued to emit bubbles at her beak. There, also, came out at her nostrils several real bubbles, from time to time; and when the animal had continued about two minutes under water, she began to struggle very much, and to endeavour either to emerge, or change posture; the latter of which, she had libetty to do, but not the former. After four minutes, the bubbles came much more sparingly from her: then, also, she began to gape, from time to time; which we had not observ'd her to do before, but without emitting bubbles; and so she continued gaping till near the end of the sixth minute; at which time all her motions, some whereof were judg'd convulsive, and others that had been excited by rousing her, appear'd to cease, and her head to hang carelessly down, as if she were quite dead. Notwithstanding which, we, for greater security, continued her under water a full minute longer; and then, finding no signs of life, we took her out; when, being hung by the heels, and gently press'd in convenient places, she was made to void a considerable quantity of water: but whether any of it had been received into the lungs themselves, we wanted time and opportunity to examine. All the means we used to recover the bird, proving ineffectual, we concluded, she had been dead a full minute before we removed her out of the water: so that, even this water-fowl, was not able to live in cold water, without taking in fresh air, above six minutes.

101. A duckling, having a competent weight ty'd to her legs, was let down into a tub of water, which reached not above an inch or two above her beak: during the most part of her continuance wherein, there came out numerous bubbles at her nostrils; but there seem'd to proceed more and greater, from a certain place in her head, almost equi-distant from her eyes, tho' somewhat less remote from her neck than they. Whilst she was kept in this condition, she seem'd, frequently, to endeavour at diving lower under water; and, after much struggling, and frequent gaping, she had, several

veral convulsive motions; and then let her head fall down backward, with her throat upwards. To this moveless posture she was reduced at the end of the third minute, if not sooner; but, a while after, there appear'd a manifest tremulous motion in the two parts of her bill; which continued for some time, and was, perhaps, convulsive: but this also, ceasing, at the end of the fourth minute, the bird was taken out, and found irrecoverable.

102. A viper, that we kept in an exhausted receiver, till concluded to have been quite dead, was, nevertheless, not thrown away, till I had try'd what could be done, by keeping her all night in a glass-body, and a warm digestive furnace: upon which, this viper was found, the next morning, not only reviv'd, but very lively.

We, therefore, put her into a tall glass-body of water, fitted with a cork to its orifice, and depress'd it with a weight, so that she could have no air. In this case, we observ'd her, from time to time; and, after she had been duck'd a while, she lay, with very little motion, for a considerable time. After an hour and a quarter, she often put out her black tongue: at the end of near four hours, she appear'd lively; and, as I remember, about that time, also, put out her tongue; swimming, all this while, as far as we observ'd, above the bottom of the water. At the end of about seven hours, or more, she seem'd to have some life; her posture being manifestly chang'd in the glass, from what it was a while before. Not long after, she appear'd quite dead; her head and tail hanging down moveless, and directed towards the bottom of the vessel; whilst the middle of her body floated as much as the cork would permit it.

103. In the generality of our pneumatical experiments upon animals, it suited with our purpose, to rarify the air as much, and as suddenly as we could; but I had other trials in view, wherein an extraordinary degree of rarification, yet not near the highest to which the air might be brought by our engine, seem'd likeliest to conduce; as particularly to afford some light in the nature of those diseases, that are thought, primarily, to affect respiration, or its organs.

Wherefore, having gages, by help of which such experiments might be much the better perform'd, I attempted several of them in the following manner.

Aug. 16. A linet being put into a receiver, able to hold about 4 pints and a half of water, the glass was well clos'd with cement and a cover; but none of the air drawn out with the engine, or otherwise. And tho' no new air was let in, nor any change made in the imprison'd air, yet the bird continued there three hours, without any apparent approach to death: and tho' she seem'd somewhat sick, yet being afterwards taken out, she recover'd, and liv'd several hours.

104. Aug. 18. From the receiver above-mention'd, we drew about half the air, whilst a linet was in the glass; and in that rarified air (which appear'd by a gage to continue in the same state) the bird liv'd an hour and near a quarter before she seem'd in danger of death: after which, the air being let in, with-

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out taking off the receiver, she manifestly recover'd, and leap'd against the side of the glass; and being taken out into the open air, she flew out of my hand to a considerable distance.

105. *Sept. 9.* Into a receiver, able to hold about 4 pints and a half of water, we convey'd a lark, together with a gage, by the help whereof we drew out $\frac{1}{4}$ of the air; then observing the bird, we perceiv'd it to pant very much. Having continued thus for a little above a minute and a half, the bird fell into a convulsive motion, that cast it upon the back. And altho' we made great haste to let in the air; yet, before the expiration of the second minute, preceding the convulsion, the lark was gone past all recovery, tho' various means were used to effect it.

106. *Sept. 9.* Presently after, we put into the same receiver, a greenfinch; and having withdrawn half the air, we soon began to observe the bird, and took notice, that, within a minute after, she appear'd to be very sick; and, shaking her head, vomited a certain substance against the inside of the glass. Upon this evacuation, the bird seem'd to recover, and continue pretty well, but not without panting, till about the end of the fourth minute; when, growing very sick, she vomited again, but much more unquestionably than before; and, soon after, eat up again a little of her vomit; upon which, she very much recover'd. And though she had, in all, three fits of vomiting; yet, for the last seven or eight minutes, that we kept her in the receiver, she seem'd to be much more lively than was expected: which may, in part, be attributed to a little air that, by an accident, got in, tho' it were immediately pump'd out again. At the end of a full quarter of an hour, from the first exhaustion, the bird appearing not likely to die in a great while, we took her out.

107. *April 12.* A new-caught viper was included, together with a gage, in a portable receiver, able to hold about three pints and an half of water. This vessel being exhausted, and secured against the return of the air, the animal was observ'd, from time to time, not only to be alive, but nimbly to put out, and to draw back her tongue, for about thirty-six hours, after she was shut up: we, therefore, continued the vessel longer, in the same shady place; where, over-night, at the end of sixty hours, she appear'd very dull and faint, and not likely to live much longer. And, the next, by the afternoon, I found her stark dead, with her mouth open'd to a strange wideness; wherefore, suffering water to be impell'd, by the outward air, into the cavity of the receiver, we found, by the water that was driven in, and afterwards pour'd out again, and measur'd, that five parts in six of the air in the vessel, had been pump'd out: so that in an air rarify'd, till it expanded itself to five or six times its usual dimensions, our viper was able to live sixty hours, and, perhaps, might have done so longer.

Animals in the same parcel of air changed, as to rarity and density.

108. In the preceding experiments, the animals were recover'd from a gasping condition, by letting in fresh air, and not the same that had been withdrawn from them: wherefore, I thought proper, to try, whether the same portion of air, without being renew'd, would, by being expanded much beyond its usual degree, and reduced to it again, serve to bring an animal

animal to near the point of expiring, and revive him again ; since, by the success of such a tryal, it would notably appear, that the bare change of the consistence of the air, as to rarity and density, may suffice to produce the above-mention'd effects.

We included a mouse in a fine, limber, clear bladder, made more transparent by oil, rubb'd on the outside, that the smell of it might less offend the animal, to be included ; clipping off as much of the bladder, at the neck, as we judg'd absolutely necessary for letting in a mouse : we, also, provided a round stick, somewhat less than the orifice ; that, the wood being laid over, with a close and yielding cement, we might tye the bladder fast, and close enough, upon the stopple thus fitted. In the bladder was left as much air, as we thought might suffice him, for the time the experiment was to last. Then, putting this limber, or dilatible receiver into an ordinary one of glass, and, placing this engine near a window, that we might see through both of them ; the air, was, by degrees, pumped out of the external receiver, and, thereupon, the air included in the bladder proportionably expanded itself, and so distended the internal receiver, till, being arriv'd at a degree of rarification, which rendred it unfit for respiration, I perceiv'd signs, in this animal, of his being in great danger of sudden death. Whereupon, the outward air being hastily let into the external receiver, compress'd the swell'd bladder to its former dimensions, and thereby, the included air to its former density ; by which means, the mouse was quickly revived. Having given him some convenient respite, the experiment was repeated with the like success.

109. We put a large parcel of tadpoles, with a convenient quantity of water, into a portable receiver, of a round figure, and observ'd, that, at the first exsuction of the air, they rose to the top of the water ; tho', most of them subsided again, till the next exsuction rais'd them. They seem'd, by their active and wrigling motion, to be very much discompos'd. The receiver being exhausted, they, all of them, continued moving, at the top of the water ; and, tho' some of them seem'd to endeavour to go to the bottom, and dived part of the way, especially with their heads, yet, they were immediately buoy'd up again. Within an hour, or a little more, they were all moveless, and lay floating on the water ; wherefore, I open'd the receiver ; upon which, the air rushing in, almost all of them presently sunk to the bottom, but none of them recover'd life.

110. We, afterwards, included a less number of tadpoles in a smaller glass, which was also exhausted, with the like circumstances, as the former. And, when I found the other tadpoles to be dead, I hasten'd to these, which did not, except, perhaps, one, give any signs of life ; but, upon letting in the air, these having not been long kept from it, some few of them recover'd, and swam up and down lively enough ; tho', after a while, these also died.

111. I repeated the same experiment in a portable receiver, of a convenient kind ; and, tho', after the exhaustion was perfected, the tadpoles, for a while, moved briskly enough, on the top of the water, only ; yet,

An attempt to prevent the necessity of respiration, by the production or growth of animals, in vacuo.

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at the end of an hour, they seem'd to be, all of them, quite dead, but continu'd floating. And, though, within half an hour after that, I let in the air; yet all the effect of it was, that the most of them, immediately sunk to the bottom, as the rest, soon after, did; none of them, that I could observe, recovering vital motion.

112. We procur'd, by preserving some rain-water, four or five of those odd insects, whereof gnats have, by some, been observed to be generated about the end of *August*, or beginning of *September*. These, for some weeks, live all together in the water, as tadpoles do; swimming up and down therein, till they are ripe for a transmigration into flies: but including them, with some of their water, in a small glass-receiver, which being exhausted, and very exactly closed, we kept, in a south-window; these little creatures continued to swim up and down therein, for some few days, without seeming to be much incommoded; but at length, and all much about the same day, they put off the habit they had, whilst they lived as fishes, and appeared with their *Exuviae*, or cast-coats under their feet; shewing themselves to be perfect gnats, that stood, without sinking, upon the surface of the water, and discovering themselves to be alive, by their motion, when they were excited to it; but I could not perceive them to fly in that thin medium: to which inability, whether the viscosity of the water might contribute, I know not; tho' they lived a pretty while, till hunger, or cold destroyed them.

The expansion of
the blood and o-
ther animal flu-
ids.

113. The warm blood of a lamb or a sheep, being taken as it was, immediately, brought from the butcher's, where the fibres had been broken, to hinder the coagulation, was, in a wide-mouth'd glass, put into a receiver, made ready for it; and the pump being set on work, the air was diligently drawn out: but the operation was not always, especially at first, so early manifest, as the spirituousness of the liquor would make one expect; yet, after a long expectation, the more subtle parts of the blood would begin to force their way thro' the more clammy, and seem to boil in large clusters, some as big as great beans or nutmegs; and, sometimes, the blood was so volatile, and the expansion so vehement, that it boiled over the containing glass; of which, when it was put in, it did not fill above a quarter.

114. Having, also, included some milk, warm from the cow, in a cylindrical vessel, about four or five inches high, tho' the pump was long ply'd, before any intumescence appear'd, yet, afterwards, when the external air was fully withdrawn, the milk began to boil, in a way, that was not so easy to describe, as pleasant to behold: and this it did for a pretty while, with so much impetuosity, that it threw several of its parts out of the wide-mouth'd glass that contain'd it; tho' there were not above two or three ounces of the liquor, which only half fill'd the glass.

A yet greater disposition to intumescence, we thought, we observ'd in the gall; which was but suitable to the viscosity of its texture.

The two last experiments were made with a design to shew, how far the destructive operation of our engine, upon the included animals, might be

be imputed the withdrawing of the air, whereby, the little bubbles generated in the blood, juices, and soft parts of the body, may, by their vast number, and conspiring distension, variously contract the vessels in some places, and stretch them in others; especially the smaller, that convey the blood and nourishment; and so, by choaking up some passages, and vitiating the figure of others, disturb, or hinder the due circulation of the blood: for, such distensions may cause pains in some nerves, and membranous parts, which, by irritating them into convulsions, may hasten the death of animals, and destroy them sooner by that irritation, than they would be destroy'd by the bare absence or loss of what the air is necessary to supply them with. And, to shew, that this production of bubbles reaches, even to very minute parts of the body, I shall add, that, I once observed in a viper, furiously tortured in our exhausted receiver, the creature had a conspicuous bubble moving to and fro, in the aqueous humour of one of its eyes.

115. To shew, that not only the blood and liquors, but also the other soft parts, even in cold animals, have aerial particles latent in them; we took the liver and heart of an eel, as, also, the head and body of another fish of the same kind, cut asunder, cross ways, beneath the heart; and putting them into a receiver, upon withdrawing of the air, we perceiv'd, that the liver manifestly swell'd every way; and, that both the upper part and lower of the fish, did so, likewise. At the place, where the division had been made, there came out, in each portion of the fish, various bubbles; several of which seem'd to rise from the *Medulla Spinalis*, the cavity of the back-bone, or the adjoining parts: and the external air being let in, both the portions of the eel presently sunk; some of the skin seeming to be grown flaccid in each.

116. We included, in a vial with a wide neck, (the whole glass being able to contain about eight ounces of water,) a small young mouse; then tyed strongly upon the upper part of the glass's neck a fine thin bladder, out of which the air had been carefully express'd; and convey'd this vessel into a middle-siz'd receiver, in which, we also plac'd a mercurial gage. This done, the air was, by degrees, pump'd out, till it appear'd by the gage, that there remain'd but a fourth part in the external receiver; whereupon, the air in the internal receiver, expanding itself, appear'd to have blown the bladder almost half full; and the mouse seeming very ill at ease, by leaping, and otherwise endeavouring to pass out at the neck of his prison; we, for fear the over-thin air would dispatch him, let the air flow into the external receiver; whereby the bladder being compress'd, and the air in the vial reduced to its former density, the little animal quickly recover'd.

The power of
use to enable
animals to sup-
port themselves
in air, by rari-
faction made
unfit for respira-
tion.

117. A while after, without removing the bladder, the experiment was repeated, and the air, by help of the gage, reduced to its former degree of rarification; when, the mouse, after some fruitless endeavours to get out of the glass, was kept in that thin air for full four minutes; at the end of which, he appear'd so sick, that, to prevent his dying immediately, we remov'd

remov'd the external, and took out the internal receiver; whereupon, tho' he recover'd; yet 'twas not without much difficulty; being unable to stand any longer upon his feet; and, for a great while after, he continu'd, manifestly trembling.

118. But, having suffer'd him to rest for a reasonable time, presuming that use had inured him to greater hardships, we convey'd him, again, into the external receiver; and, having brought the air to the former degree of rarification, we were able to keep him there for a full quarter of an hour; tho' the external receiver did not at all considerably leak; as appear'd both by the mercurial gage, and by the remaining distension of the bladder. And, 'tis worth noting, that, till near the latter end of the quarter of an hour, the animal scarce at all appear'd distress'd; remaining still very quiet. And tho', when he was put in, his tremblings were yet upon him, and continu'd so for some time; yet, afterwards, in spite of the expansion of the air he was then in, they soon left him. And, when the internal receiver was taken out, he not only recover'd from his fainting sooner than before, but escaped those subsequent tremblings.

119. Encouraged by this success, after we allowed him some time to recover his strength, we re-convey'd him, and the vessel wherein he was included, into the former receiver, and pump'd out the air, till the mercury, in the gage, was drawn down near half an inch lower than before, that the air might be yet farther expanded. And, tho' this, at first, seem'd to discompose the little creature; yet, after a while, he grew very quiet, and continu'd so for a full quarter of an hour; when, we caus'd three exsuctions more to be made, before we discover'd him to be in manifest danger, (at which time, the bladder appear'd much fuller than before;) but, then, we were obliged to let the air into the outward receiver; whereupon, the mouse was more speedily revived, than one would have suspected.

Now the air, in which the mouse liv'd all this while, had been clogg'd, and infected, with the excrementitious effluvia of his body; for 'twas the same all along; we having, purposely, forbore to take off the bladder, whose regular distensions, and shrinkings, sufficiently manifested, that the vessel, whereof 'twas a part, did not leak.

120. We took a mouse, of an ordinary size, and, having convey'd him into an oval glass, fitted with a somewhat long, and considerably broad neck, that it might be wide enough to admit a mouse, in spite of his struggling; we convey'd in, after him, a mercurial gage, in which we had carefully observ'd, and mark'd the station of the mercury; and which was so fasten'd to a wire, reaching to the bottom of the oval glass, that the gage, remaining in the neck, was not in danger of being broken by the motions of the mouse in the oval part. The upper part of the long neck of the glass was, notwithstanding the wideness of it, hermetically seal'd, by means of a lamp, and a pair of bellows, that we might be sure the imprison'd animal should breathe no other air, than what fill'd the receiver, at the time when it was seal'd. This done, the mouse was watched,

Air, become unfit for respiration, may retain its usual pressure.

watched, from time to time : and tho', by reason of the largeness of the vessel, he seem'd rather drooping, than very near death, at the end of the second hour ; yet, in about half an hour after, he was judg'd to be quite dead, tho' we shook the vessel, to rouse him. The gage manifested no sensible change in the station of the mercury ; but, causing the seal'd part of the glass to be broken off, I obtain'd, after a while, some faint tokens of life : tho', I am not sure, that they would have continu'd in a vessel, where the air was so clogg'd and infected, if fresh air had not been frequently blown in by a pair of bellows, whose nose was inserted into the neck of the glass. This fresh air seem'd evidently, tho' but slowly, to revive the gasping animal, which I could not, conveniently, take out of the glass, till he had gain'd strength enough to make use of his legs ; but, after that, without breaking the glass, we took him out, and soon found him able to walk up and down.

121. A like experiment we, also, made with like success, upon a small bird, included, with a gage, in a receiver that would hold about a quart of water. The bird, in about half an hour, appear'd to be sick, and drooping ; the faintness, and difficulty of breathing, increasing for about two hours and a half after ; at which time, the animal died ; the gage being not sensibly alter'd, unless, perhaps, the mercury appear'd to be impell'd up a little higher than when put in ; which, yet, might proceed from some accidental cause.

122. To shew, that it is not want of coldness, but something else in the included air, that makes it destroy birds pent up therein, which, by the hot exhalations of their bodies, may be supposed to over-warm it ; we made the following experiment.

In a glass-vial, able to hold about three quarts of water, we, hermetically seal'd up a small bird ; and found, that, in a few minutes, she began to be sick, and pant. These symptoms I suffer'd to continue, and increase, till they had lasted just half an hour ; at which time, having provided a vessel of water, with sal-armoniac, newly put therein, to refrigerate it ; and the liquor being thus made exceeding cold, the vial, with the sick bird, was immerfed in it, and so kept there for six minutes : yet it did not appear, that the great coldness which must be thus procured to the imprisoned air, sensibly revived or refresh'd the drooping animal, who manifestly continued to pant exceedingly. So that this remedy, proving ineffectual, the vial was remov'd out of the water ; and the bird, some time after, many times strain'd to vomit : and, afterwards, had evacuations downward, before she quite expired ; which she did, in almost an hour, from the beginning of her imprisonment.

123. We made, by distillation, a blood-red liquor, chiefly consisting of such saline, and spirituous particles, as may be obtained from human blood ; which is of such a nature, that if a glass-vial, about half filled with it, be kept well stopp'd, it will rest as quietly as an ordinary liquor, without sending up any smoke, or visible exhalation ; but, if the vial be open'd so, that the external air is permitted to come in, and touch the surface of the liquor,

The use of the air to raise and support the steams of bodies, considered, with regard to respiration.

with-

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within a quarter of a minute, or less, there will be elevated a copious white smoke, which not only fills the upper-part of the glass, but plentifully passes out into the open air, till the vial be again stopp'd.

When this vial has lain stopp'd and quiet for a competent time, the upper half of it appears destitute of fumes, whereof the air, it seems, will imbibe, and constantly retain but a certain moderate quantity; which may give some light towards the reason, why the same air, quite clogg'd with steams, will not long serve for respiration. And if the unstopp'd vial were placed in our vacuum, it would emit no visible steams at all, not so much as to appear in the upper part of the glass itself that held the liquor; but when the air was, by degrees, restored at the stop-cock, without moving the receiver, to avoid injuring its closeness, the returning air would presently raise the fumes, first into the vacant part of the vial, whence they would ascend into the capacity of the receiver: and likewise, when the air, requisite to support them, was pumped out, they also accompanied it, as their unpleasant smell made manifest; whilst this red spirit, though it remain'd unstopp'd, emitted no more fumes, till new air was let in.

*Snails, a slow-
worm, and a
leech, in vacuo.*

124. Two ordinary white snails, without shells, differing in size, (the biggest being about an inch and a half, and the other about an inch in length) were included in a small portable receiver, which was carefully exhausted, and secured against the return of the air; and presently after, being removed from the engine, it was easy to discern, that both the snails thrust out, and drew in their horns, at pleasure; though their bodies had, in the softer places, numerous newly generated bubbles sticking to them: and tho' they did not lose their motion near so soon, as other animals, in our vacuum; yet, after some hours, they appear'd moveless, and very tumid; and, at the end of twelve hours, the inward parts of their bodies seem'd to be almost vanish'd, whilst they appear'd to be two small full-blown bladders: and, on letting in the air, they immediately so shrunk, as if the bladders having been prick'd, the receding air had left behind it nothing but skins: nor did either of the snails, afterwards, tho' kept for many hours, give any signs of life.

125. We included in a receiver, whose globular part was about the bigness of a large orange, one of that sort of animals, vulgarly call'd, efts, or, slow-worms: having withdrawn the air, and secur'd the vessel against the return of it, we kept him there about forty-eight hours; during which, he continued alive, but appeared somewhat swell'd in his belly; his under-chap moving on the very first night, but not the day and night following. At length, by opening the receiver, under water, we perceiv'd, that about half the air had been drawn out. As soon as the water was impell'd into the glass, the animal, which was before dull and torpid, seem'd, by very nimble and extravagant motions, to be strangely revived.

126. We took a leech, of a moderate bigness, and having included it, together with some water, in a portable receiver, able to hold about ten or twelve ounces of that liquor; the air was pump'd out, after the usual manner;

manner, and the receiver being remov'd to a light place, we observ'd, that, the leech keeping herself under water, there emerg'd from several parts of her body, numerous bubbles, some of them in a dispersed manner, but others, in rows, or files, that seem'd to come from determinate points. Tho' this production of bubbles lasted a pretty while, yet the leech did not seem to be very much discomposed. This done, we set the receiver, which was well secur'd from the outward air, in a quiet place, where we visited it, at least, once a-day; and found the leech somewhat fastned by her tail, to that part of the glass which was under water, and sometimes wandering about that which was quite above water; and always, when we endeavour'd to excite her, she quickly manifested herself to be alive; and, indeed, appear'd so lively, after the full expiration of five natural days, that expecting something might have happen'd to the receiver, I open'd it under water; when the outward air, impell'd in so much of that liquor, as satisfy'd me the receiver was well exhausted.

127. Five or six caterpillars, all of the same sort, being put into a separable receiver of a moderate size, had the air drawn away from them, and carefully kept from returning. But, notwithstanding this, I found them, about an hour after, moving to and fro in the receiver; and even above two hours after that, I could, by shaking the vessel, excite some motions in them, that I did not suspect to be convulsive. But looking upon them again, about ten hours after they were first included, they seemed to be quite dead; and, tho' the air were forthwith restored to them, they continued to appear so: yet, leaving them all night in the receiver, I found, the next day, that three, if not four of them, were perfectly alive.

128. We took from an hedge a branch, that had a large cob-web of caterpillars in it, and dividing it into two parts, we put them into like receivers; and in one of them shut up the caterpillars, together with the air, but from the other it was exhausted. Now, in that which had the air, the little insects, after a small time, appeared to move up and down as before, and so continued to do for a day or two: but in that glass, whence the air had been extracted, and continued kept out, they shewed, after a very little while, no motion that we could perceive.

129. Nov. 12. About 8 a-clock at night, there were taken four middle-sized flesh-flies, which, having their heads cut off, were inclosed in a portable receiver, furnish'd with a large pipe, and a bubble at the end. As soon as the receiver was exhausted, the flies lost their motion; an hour or two after, I approached them to the fire, which restored not their motion: wherefore I let in the air upon them; after which, in a very short time, they began, one after another, to move their legs, and one or two of them to walk. And having kept them all night, in a warm place, they manifested, for a while, some small motion.

130. Sept. 11. About noon we closed up several ordinary flies, and a bee, or wasp; all which, when the air was fully withdrawn, lay as dead; only, for a very few minutes, some of them had convulsive motions in their legs. They continued in this state forty-eight hours; after which, the

PNEUMATICS. air was let in upon them; and that not producing any signs of life, they were laid in the noon-day sun: but none of them seem'd, in any degree, to recover.

131. *Dec. 11.* We put a great flesh-fly into a very small portable receiver, where, at first, he appear'd to be very brisk and lively; but, as soon as the air was drawn out, he fell on his back, and seem'd to have convulsive motions in his feet, and *Prohospis*; from whence he presently recover'd, upon letting in the air; which being drawn out again, he lay as dead: but, within a quarter, or half an hour after, I perceiv'd, upon shaking the receiver, that he stirr'd faintly up and down. This was done pretty late over night, and next night I found the fly not to be soon revived, either by warmth, or letting in the air. However, in a while after, he recover'd; and being, next morning, sealed up again in that glass, and kept forty-eight hours, tho' over the chimney, he died beyond recovery.

132. We took a large grass-hopper, whose body, besides the horns and limbs, was about an inch in length, and of great thickness, in proportion to that length; and convey'd him into a portable receiver of an oval form, and able to hold about a pint of water: and having, afterwards, pumped out the air, till, by the gage, it appear'd to have been pretty well drawn out, we took care that no air should re-enter. The success was this. First, tho' before the exhaustion of the air begun, the grass-hopper appear'd lively, and continued so for a while: yet, when the air began to be considerably rarified, he seem'd to be very ill at ease, and to sweat out of the abdomen, many little drops of liquor, which being united, trickled down the glass like a little stream, that made, at the bottom, a small pool of clear liquor, amounting to near a quarter of a spoonful; and by that time the receiver was ready to be taken off, the grass-hopper was fallen upon his back, and lay as dead. Secondly, tho' having, a little after, laid the glass in a south-window, on which the sun then shone, I perceived some slow motions in the thorax, as if he strained to fetch breath; yet, I was not sure, but they were convulsive motions; however, they lasted but a while, and then the animal appeared to be quite dead, and to continue so for three hours, from the removal of the receiver. Thirdly, that time being expired, the glass was open'd, and the air let in; notwithstanding which, there appear'd no sign at all of life: but letting the glass rest in a convenient posture, that the water which came from him, might not endanger him, for a quarter, or half an hour; tho' I then perceived no signs of life, yet I caused him to be carried into a sun-shiny place, where the beams of a declining sun presently began to make him stir his limbs, and, in a short time, brought him perfectly to life again.

133. *April 15.* We took one of those shining beetles called rose-flies, and having included it in a very small round receiver, which we exhausted, it struggled much whilst the air was withdrawing; yet presently after, I could perceive but little motion: about six hours after, the fly seem'd quite dead, and discover'd no motion upon that of the glass. And within about an hour more, tho' I let the air rush in, yet no sign of life ensued, neither immediately, nor for a pretty while after. So that suspecting the beetle to

be

be really dead, I yet, three or four hours after, found him lively. Whereupon, I caus'd the glass to be again exhausted, and secur'd from the air; during which time, the animal seem'd to be much disquieted, but did not lose his motion, soon after.

134. With butter-flies I made several tryals, and, having observ'd them, not only to live, but to move, longer than was expected; I chose to include several of them in receivers, somewhat large, that I might see, whether, in so thin a medium, some or other of them, by help of their large wings, would be able to fly. But, tho', whilst the air continu'd in the glasses, they flew actively, and freely up and down; and, tho', after the exhaustion of the air, they continu'd to live, and were not moveless; nay, tho', at the bottom of the receiver, they would even move their wings and flutter a little; yet, I could not perceive any of them to fly, or have a progressive motion, supported by the medium, only. And, by frequently inverting the receiver, which was long, they would fall, like dead animals, without displaying their wings, tho', just as they came to touch the bottom, some of them, would, sometimes, seem to make use thereof, but not enough to sustain themselves, or to break their falls.

135. A number of ants being included in a small portable receiver, exhausted about noon; between six and seven in the evening, they seem'd to be all quite dead; and the rather, because, tho' they appear'd very lively just before they were seal'd up, running briskly up and down the bubble they were in; yet, they grew almost moveless, as soon as the air was exhausted; and a little while after appeared more so: tho' I a little suspected, that they were much incommoded, by some glutinous substance, that seem'd to have got into the receiver, from the vapours of the cement. When upon opening the glass, the air rushed in, no sign of life appear'd, for a great while, in any of the ants: but next morning, about nine a-clock, I found many of them alive, and moving about.

ex-The necessity of air, to the motion even of ants and mites.

We convey'd a number of mites, together with the mouldy cheese, wherein they were bred, into three or four portable receivers, which were, all of them, very small, and not much differing in size. From all of these, except one, we withdrew the air; and, then, making use of our peculiar contrivance to hinder its return, we took them, one after another, from the engine, and laid them by, for further observation. That wherein, to observe the difference, we left the air, was sealed at a lamp-furnace, after the usual manner. Our tryals afforded us the ensuing phenomena.

(1.) The mites, inclosed in the small glass, that never came near the engine, continu'd alive, and able to walk up and down, for above a full week after they had been put in; and, possibly, would have continu'd much longer, if the glass had not been accidentally broken.

(2.) As soon as ever one of the receivers was remov'd from the engine, I look'd with great attention upon it; and, tho', just before the withdrawing of the air, the mites were seen to move up and down in it; yet, within a few minutes, after the receiver was apply'd to the engine, I could discern in them no life at all; nor was any perceiv'd by younger eyes than mine.

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Nay, by the help of a double convex-glass, I was not able to see any of them stir up and down. And no motion was taken notice of in the other small receiver of a like shape and bigness. About an hour after, I look'd upon the receiver attentively again, but could not perceive any of the mites to stir; and the like unsuccessful observation I made two or three hours after that. And at first letting in the air, to try if the mites were not quite dead; I could not perceive, upon its rushing in, any of them to stir: yet, I left the receiver unstop'd as it was in the window, upon a suspicion, that the air might not be able to exercise its operation upon them, in a short time.

(3.) And, about two or three days after, I found a number of my little animals reviv'd; as an attentive eye might easily perceive, by the motion of certain little white specks, when assisted to observe it by little marks, that I made on the outside of the glass, (which was purposely chosen thin and clear) near this, or that mite, with a diamond; by the approach to, or recess from which marks, the progressive motion became, perhaps, within a minute, plainly discoverable; especially, if, when the eye perceiv'd little white specks, that look'd like mites, the receiver should be so turn'd, that the bellies and feet of those little creatures were uppermost; notwithstanding which, they would not easily drop down, but continue their motion: and these specks being made upon the concave surface of the thin glass itself, were thereby render'd much more easily visible.

(4.) But because it doth not, by the third phenomenon, appear, whether, in case our mites had been kept in a moveless state, for a much longer time, than three or four hours, they would have been recoverable, by the admission of the air; I shall add, that one of the portable receivers, above-mention'd, being exhausted and carefully secured from the air, was kept from monday morning to thursday morning: after all which time, being unable to discover any signs of life, among the included mites, the air was let in upon them, which, soon, had such an operation upon them, that both I, and others could plainly see them creep up and down in the glasses, again.

136. Having procured a large number of silk-worms eggs, and caused three very small receivers to be purposely made, that differ'd very little, either in size or figure; we convey'd into each of them, together with a small stock of mulberry-leaves, such a number of eggs as, we thought, made it morally certain, that, at least, some of them should prove prolific. This done, we carefully exhausted one of the receivers, and secured it against the return of the air; and the two others we left full of air: but, having left in the one a little hole for the air to get in and out at, we stopp'd the other so close, as to hinder all intercourse between the internal and external air. Things being thus prepared, we expos'd the receivers to a south window, where they might be quiet, and where I either came, or sent to look on them, from time to time; the spring being then so far advanced, that, I suppos'd, the heat of the sun would be, of itself, sufficient to hatch them, in no long time. And both I, and others, took notice, that, in the unexhausted receivers, there were several eggs hatch'd into little insects, that perforated their shells, and crept out of them; tho', afterwards, for want of

*An attempt to
produce living
creatures in va-
cuo.*

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of change of food, or air, or both, few, or none of them, proved long-lived. But tho' the eggs, in these receivers, began to afford us little animals, in a few days; yet the eggs, in the exhausted receiver, afforded none in so many more, that we left off to expect any from them.

We took several of those little swimming creatures, which in autumn, especially towards the end of it, are turn'd into gnats; and, having put a convenient number of them together, in a fit quantity of rain-water, wherein they had been found and kept, into a small receiver; the air was pump'd out, and the vessel secured against its return, and then set aside in a place, where I could observe, that, on the day after, some of these little animals were yet alive, and swimming up and down, not without minute bubbles adhering to them; but, in a day or two after that, I could not perceive any of them alive: nor did any of them recover, upon the admission of fresh air. Indeed, the weather was so cold and unseasonable, that a number of these little creatures, put up with water in another small receiver, all died within a few days, tho' none of the air was exhausted. And several that I kept in an ordinary glass, which was often unstopp'd, to give them fresh air, perish'd very fast.

137. We took a round glass-egg of clear metal, and furnish'd with a shank, some inches in length; this we fill'd with water, and convey'd both it, and a vial, containing water, into a receiver, of a convenient size; and by pumping the air out of it, we made bubbles both in the egg, and the vial, to disclose themselves in great numbers; so that the liquor, in the glass-egg, seem'd to boil, and caus'd all that was in the shank, to run over. When we thought the water was sufficiently freed from air, we took out the glasses, and fill'd up the shank of the egg with water taken out of the vial, and inverted it into more of the same water, in such manner, that the egg was quite full, shank and all, excepting a small bubble of air, that we, purposely, left, to gain the top of the egg; where we measured it as accurately as we could, and found it to be a tenth, and less than two hundredths of an inch. Then, putting the glasses again into the receiver, the pump was work'd, and the little bubble, after a while, began to expand itself; which, when it had once done, it, at each suck, strangely increas'd, till, at length, it drove all the water out of the round part of the glass. And, lest it might be objected, that 'twas only the subsiding of the water, upon the withdrawing of the outward air, that before kept it up to the top of the glass; we caus'd the pumping to be continu'd, till the expanded air had, several times, driven the water, in the pipe of the egg, a pretty way beneath the level of the external and surrounding water in the other glass. This done, we let in the air, by degrees, with a design to observe what bubble we should find at the top of the egg, when the water should be again driven up into its cavity. But the expanded air had forced over so much water, that there remain'd not enough to fill the globular part of the egg. We, therefore, made the experiment again; and, when we had proceeded thus far, compar'd the above-mention'd diameter of the small bubble, with that of the spherical part

*The surprising
variation of
air without heat.*

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part of the glass, which we took with a pair of calliper-compasses; and tho' we found it to be somewhat more than twenty times as great, yet we supposed the two diameters to be only as 1 to 20: and, consequently, since the proportion between spheres is triplicate to that of their diameters, the air appear'd to have, by expanding itself, possess'd eight thousand times the space it took up before. Nor was it overseen by us, that such glasses as we used, are scarce ever spherical. But Dr. Wallis, who assisted at the experiment, concluded, with me, that the cavity of the shank, from whence the expanded air drove the water, which we did not compute, would make abundant amends for any inaccuracies. After this, for farther satisfaction, we took water, laboriously freed from air; and, putting it into the same glass-egg, we inverted it, as before, but left not any bubble in it. This we did, that, in case we could make the water subside, the experiment might prevent a suspicion, that some air, latent in the water, increased the bubble, formerly left in it. Having, then, exhausted the receiver, at least as much as before, the water, in the egg, did not at all subside: but, at length, with obstinate pumping, a bubble disclosed itself, and drove all the water clear out of the round part of the glass. And tho', by reason of some small leaks, that we could not find, or stop, we were not able, as before, to make the expanded air depress the water in the shank, beneath the surface of the external water; yet we wanted very little of it: and, then, giving over, we found, that when the water was impell'd up again into the egg, there was, at the top of it, a bubble, whose diameter we measur'd, and found it to be to the diameter of the globular part of the glass, as 1 to 14: so that, tho' the little bubble had been a perfect sphere, it must, when expanded, have been 2744 times as big as when unexpanded. But Dr. Wallis, observing the great thinness of the bubble, positively affirm'd, that he could not estimate it to be, at most, any bigger than the third part of a perfect sphere of that diameter: by which estimate, the expansion of the bubble must have reach'd to 8232 times its natural dimensions. Yet by letting as much water into the receiver as it would admit, we found, that we had not exhausted all the air.

138. At another time, a small, and almost invisible bubble, expanded itself, when the ambient air was pretty well exhausted to more than ten thousand times its former extent. We took a small bolt-head, blown at a lamp, which contain'd, in all, about eighty grains of water; and inverting the small neck into a jar of water, it was included in the receiver; and the ambient air being exhausted, numerous bubbles rose out of the water, and, expanding, quickly drove all the water out of the bolt-head. Then, re-admitting the outward air, the bolt-head was presently almost fill'd, and all the expanded air shrunk into a bubble little bigger than a small pin's head; when, taking the bolt-head out of the water, and inverting it, that the bubble might get out at the neck, we carefully fill'd it up with the water that had been freed from air; and, then, inverting it, as before, into the jar with water, we again included it; and, after some exsuctions, found, that there was got out of the water,

into

into the neck, a very conspicuous bubble, which, upon admitting the air, shrunk almost into an invisible one, and ascended into the head of the glass. Then, again exhausting the receiver very well, we found it expand itself, so as to fill the capacity of the bolt-head, and to drive out almost all the water. And, upon re-admitting the air, it again shrunk into a bubble, whose diameter (according to our best estimate) was not more than a two and twentieth part of the diameter of the head of the above-mention'd glass; so that, to fill the whole cavity of the head only, it expanded itself 10648 times: but, because it fill'd, likewise, the greatest part of the neck, we found, by weighing the water which fill'd that part, and the water which fill'd the head, that the capacity of that part of the neck, was almost a third of the capacity of the head; being as 141 to 481. If, therefore, 481, the capacity of the head, contain'd it 10648 times; 141, the capacity of the neck, must contain it $3121\frac{1}{3}\frac{2}{3}$ times; so that, in all, the small bubble of air was expanded to above 13769 times its former bulk.

The diameter of the small bubble contracted, was $\frac{1}{17}$ inch.

The diameter of the outside of the head of the glass was $\frac{3}{2}$ inch.

The water, that fill'd the head only, weigh'd sixty grains and a half.

The water that fill'd the head, and as much of the neck as the air had before expanded itself into, weigh'd seventy-eight grains, and one eighth; whence that part in the neck weigh'd seventeen grains, and five eighths. The bolt-head itself weigh'd fifteen grains.

139. We tried this experiment again, and found a small bubble, much about one twelfth of an inch in diameter, fill'd not only the ball at the end of the bolt-head, (which was an inch and a half in diameter,) but the whole neck, which contain'd near as much water as the head; and beat down the surface of the water within the pipe, much below that of the water external to it.

These experiments may give rise to inquire, what figures and motions in the particles of the air, can explain such a wonderful rarification, perhaps, without quite losing its durable spring; how the air comes to be rarifiable so many times more without heat, than hitherto we have found it by heat; and, lastly, what might, reasonably, be conjectur'd about that part of the cavity of an exactly closed glass, where, tho' the eye discovers no visible substance, it appears not, that the common air adequately fills so much as the ten thousandth part.

140. It has not, that I know of, been attempted to discover, whether the, The duration of the spring of expanded air. air either in the utmost, or in the intermediate degrees of rarification we can bring it to, retains a constant and durable elasticity; and what other properties it either gains or loses by confinement*.

To attempt something of this kind, I caused a good bubble of glass, with a stem, to be so blown at the flame of a lamp, that whilst the ball was

* Mr. Hawksbee has shewn, by experiment, that the spring of the air may be so disturb'd by violent pressure, as to require a considerable time to recover its

natural tone and temper; and that this tone will be as the force employ'd, or its continuance in such a violent state. *Hawksb. Exprim. p. 110—112. and p. 162—166.*

Experiment 140. yet exceeding hot, and, consequently, contain'd none but highly rarify'd air, the stem was suddenly seal'd up. This bubble, many months after, I inverted into a basin of water; and, having broken off the seal under the surface of it, the liquor was violently impell'd into the cavity, yet was not able to fill it; a considerable part being defended from the farther ascent of the water, by the spring of the remaining air; which, for all the long stretch it had been put to, had not, that we observ'd, lost any thing of its spring. At another time, leaving a very small proportion of air in the folds of a fine limber bladder, whose neck was very closely tied; by the help of the air-pump, it was so expanded, that, at length, it seem'd to fill the whole bladder, and reduce it to the extent it had, just before 'twas empty'd. And the bladder, by a peculiar contrivance, was so included in another vessel, that, being protected from the outward air, it maintain'd its tumid figure; and in that unwrinkled state it continu'd for near three years.

I, afterwards, contriv'd an instrument, fit to discover how long air, brought to the greatest expansion I could conveniently reduce it to in my engine, will retain its spring; and by what degrees, or stages, and periods of time, the decrease, if any happen, is made. But I could not, by its means, observe any remarkable diminution in the air's elasticity, tho' it was press'd, and, as it were, clogg'd with a weight, which one would wonder how it could, when 'twas so highly rarify'd, support for one minute. And, in one of them, we found not, in ten weeks time, any considerable variation; for the little shrinking of the air, discoverable by an attentive eye, might be, probably, ascribed to the change of the weather to a far greater degree of coldness.

I, also, contriv'd a little portable instrument, wherein the air being expanded, as one may guess, to five or six hundred times, (perhaps a thousand times) its wonted extent, has not only, for a long time, preserv'd its spring; but, also, tho' very much dilated, without heat, the heat of the hand, apply'd to the outside of the vessel, has a quick, and very manifest operation; and, upon the withdrawing thereof, the air quickly returns to its former dimensions, and temper: so that it may be employ'd as a kind of weather-glass.

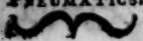
Experiment 141. A cylindrical glass, blown at a lamp, and having a long stem coming out at the unseal'd end, was quite fill'd with water, and inverted into more, placed at the bottom of a large pipe, seal'd at one end, and of three or four feet in length: this external pipe was exhausted, till the air, that disclosed itself in the water of the internal one, had forc'd out the water, in the cylindrical glass, as low as the upper part of the stem; at which great expansion of the air, the external pipe, being speedily and securely closed by a certain contrivance, the air, thus rarify'd, was kept sometimes in my own chamber, that was warmer; sometimes in an under-room; and, after it had been kept, from first to last, about eleven weeks, or three months, without any other remarkable variation, than that in the cold room, the water ascended a little at that part of the internal pipe, where

where the lower-end of the cylinder gradually lessen'd itself into the slender stem. At length we broke off the closed apex, when the water was but leisurely (because of the slenderness of the orifice made for the air to get into it) impell'd up into the deserted cavity of the cylinder, which it wholly fill'd, except a little bubble, exceeding shallow. We made use of our eyes, at a fit distance, and of compasses, both ordinary and callaper, to obtain these measures. The cylindrical part of the internal pipe was three inches in length; and three fifths of an inch, or less, in diameter, on the outside. The bubble was two tenths in diameter, and about two hundredths in depth. From all which, according to Dr. Wallis, who assisted in the experiment, the natural bubble was, to the space it possess'd, when expanded, as 1 to 1350.

142. After the middle of September, on a sun-shiny day, about noon, we took a bolt-head, or round vial, furnish'd with a long stem, and plac'd it in a frame purposely provided, so that the stem was perpendicular to the horizon, and the globular part supported by such a vessel, that thorough a hole made in its middle, the shank reach'd downwards, till the orifice of it was a little immers'd beneath the surface of a glass of water, placed at the bottom of the frame. This done, we took a large proportion of beaten ice, and mix'd it with a due quantity of bay-salt, and not only laid it round about the lower part of the ball; but the vessel, contiguous to that part, being purposely made with turn'd-up brims, we heap'd up the frigorific mixture, so as to bury the whole spherical part of the glass in it, and cover the very top of it therewith to a considerable thickness; whereby the air within being exceedingly cooled, the water, in which the shank terminated, was made to ascend fast along the cavity of that shank, till we perceived it would reach no higher: but, after a while, it began to subside again; which nick of time being carefully watch'd, we made a mark at the highest station of the fluid, and then taking out the bolt-head, we fill'd it with water; allowing for that small part of the stem which was immers'd at the beginning of the operation. This water weigh'd nineteen ounces, and six drams; then weighing as much water, as sufficed to fill the shank up to the mark before-mention'd, we found that to be one ounce and three drams; by which number, the former being divided, the quotient is $14\frac{1}{4}$ drams: so that the proportion of the two quantities of water, being as 11 to 158, the space into which the air was condensed by refrigeration, was to the space it possess'd in its former state of laxity, as 147, to 158; and, consequently, the greatest condensation, that such a time of the year, such weather, and so high a refrigeration could bring the air to, made it lose but $\frac{1}{11}$ of its former extent.

The condensation of the air by cold, and its compression without mechanical engines

But, in the following condensation, or compression of air, tho' cold were, indeed, employ'd, yet that could not contract the air to any thing near such a degree, where the frigorific mixture did not primarily, or immediately, compress the included air; but only so affected the water that was shut up with it in the same vessel, as to make it swell, and, consequently, crowd the aerial particles into less room.



The experiment was this. We took a new glass bolt-head, with a neck not long, and fill'd it so far with common water, that, being hermetically seal'd, the liquor reach'd within three inches of the top; and making an estimate of the sharp end, left so for the conveniency of sealing up the glass, we guess'd, it to be about a quarter of an inch in length; then, applying snow and salt to the lower part of the bolt-head, we readily drove out the water further and further into the neck, till at length it was got up to the basis of the sharp conical end, where the glass was seal'd; and then, just as I was looking upon it, the glass flew, with a noise, about my ears; being broke into many pieces, which argued the compression of the air to have been very great. And Dr. Wallis, who was present, and measur'd it from time to time, desired me to register the experiment, with his estimate; which is, that the air was reduced into the fortieth part of its former dimensions.

This condensation of the air is the more surprizing, because some of the greatest mathematicians of our age, have not, with wind-guns, and other forcible engines, been able to crowd the air into less than the fifteenth part of its usual extent.

The surprizing
difference in ex-
tension of the
same quantity
of air rarified
and compress'd.

143. Tho' we could not find, that cold, in our climate, would reduce the air into near the twentieth part of its natural space, by condensation; yet, heat will advance it to near seventy times its usual laxity, by rarification.

But, as by engines, and artificial contrivances, the air may be two or three times more compress'd, than naturally it is, even in frosty weather; so, on the other side, it may, by means of art and instruments, be much more rarified, and expanded, than has been hitherto found, by the bare application of external heat, even that of an intense fire.

We may, also, observe, how much the utmost degree of its rarification by heat, mention'd by *Mersennus*, falls short of the degree to which it has been advanced in our pneumatical engine; the proportion betwixt the two being that of about 1 to 70.

But the air, we make our trials with, upon the surface of the earth, is not, properly, in a free and indifferent state, with regard to rarification, and condensation; but already highly compress'd by the weight of the atmosphere resting upon it: whilst the air to be rarified, has, by virtue of its spring, a strong tendency to dilate itself.

Here, then, seems to be a surprizing mutability of the air, as to rarity and density; whereby the same quantity of air being, sometimes, compress'd, and sometimes dilated, may change its dimensions to a degree, that seems, almost, to transcend the power of nature and art; and, by consequence, might, probably, be rejected as incredible, if it were abruptly, and nakedly propos'd: for, we can scarce safely put determinate limits to the stupendous rarity, which the upper part of the atmosphere, being, almost totally, uncompress'd, by incumbent particles of air, may be supposed to have by nature, unassisted with art.

But

But to compare together the smallest extent, to which we have reduc'd the air, by condensation, and the greatest to which we have advanc'd it by rarification; the extent of the same quantity, highly rarified, is, to leave out some odd hundreds, 13,000 times greater than before; which, being multiplied by 40, the degrees of the air's compressure, it will amount to 520,000, for the number of times, by which the air, at one time, may exceed itself in bulk at another: a difference truly surprizing, tho', doubtless, it might be carried vastly higher! *

SECT. III.

BEfore we proceed to our other pneumatical experiments, 'tis necessary to premise, what relates to the improvements of the chief engine, wherewith they were made, and to the other instruments employ'd therein.

In our engine, with a double barrel, for exhausting the air, AA, are two pumps made of brass.

BB, two suckers or *Emboli*, hollow within, and open below.

CC, two holes in the upper part of the suckers, with valves opening outwards, to let the air escape, and hinder it from coming in.

DDDD, iron rods, serving to move the *Emboli*, being annex'd to them.

EE, two flat iron stirrups, at the top of the rods DD, on which, the operator must stand to work the engine.

GGG, a cord join'd to the two stirrups, and running in the pully H.

A description of an engine with a double barrel for exhausting the air.
Fig. 68.

* Air, near the earth's surface, possesses about 850 times the space of an equal weight of water; and, therefore, says Sir *Isaac Newton*, "a cylindrical column of air, 850 feet high, is of the same weight with a column of air a foot in height, and of the same diameter. But a column of air, reaching to the top of the atmosphere, is equal in weight to a column of water, of about 33 feet high; if, therefore, the lower part of the whole aerial column of 850 feet high, be deducted, the remaining upper part will be equal, in weight, to a column of water 32 feet high. Now, since the air is compress'd, in proportion to the atmosphere that rests upon it; and since gravity is reciprocally as the square of the distance of the place from the earth's center; I have found," says he, "that air, in ascending from the surface of the earth, to the height of one semi-diameter thereof, is rarer than

"with us in a far greater proportion, than that of all the space below the orb of *Saturn*, to a sphere of an inch diameter. Consequently, such a sphere of our air, of the rarity it has at the height of a semi-diameter of the earth, would fill all the regions of the planets, as far as the orbit of *Saturn*, and vastly farther!" *Newton. Princip. p. 470.*

This prodigious degree of rarification, seems unintelligible to Sir *Isaac Newton*, by feigning the particles of air to be springy and ramous, or rolled up like hoops; or, by any other means than a repulsive power; which is much greater here than in other bodies, because air is very difficultly generated out of very fix'd bodies; and scarce without the assistance of fermentation; for those particles recede from one another with the greatest force, and are most difficultly brought together, which, upon contact, cohere most strongly. *Newton. Optic. p. 371, 372.*

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LL, two valves at the bottom of the pumps, opening inwardly, to admit the air out of the tube MM.

MM, a tube reaching from both pumps to the plate OO, by means of the curvature PPQQ; which ought to be so long, that the tube PQQ, may not hinder the pumper from standing conveniently on the stirrups EE.

OO, a plate bored in the middle, on which, the receivers, to be evacuated, are placed; as R, for example.

Before this engine can be fit for use, it is to be put into a frame of wood, to support it, as *Fig. 69.* and as much water is to be poured thro' the hole Q, in the plate OO, into the pumps, as will fill the cavities of the suckers, and a little more: then, a person must stand on the two iron stirrups EE, and alternately depress and elevate them. By this means, the suckers, following the motion of the stirrups, in their ascent, will leave the space in the bottom of the pumps empty; and since, as all other passage is denied from the air, that alone, which is contain'd in the receiver R, is convey'd into the pumps, by the tube QQPPM, and opens the valve L, which being presently shut, hinders the same air from returning: wherefore, the sucker afterwards descending, compresseth that air; whence of necessity, the valve C, must be open'd, and all the air pass out at it; because, the water in the bottom of the pumps, exactly fills all the space, and also regurgitates thro' the valve C.

This double engine is, upon many occasions, preferable to a single one; since it doth, not only, produce a double effect, but, also, performs it much more easily: for, in those engines, which are furnish'd but with one tube, whilst the sucker is drawn up to evacuate the pump, the whole pillar of the air, incumbent on the sucker, is to be elevated by force; and again, when the sucker returns, it is also, by force to be restrain'd, lest it should be too swiftly impell'd by the air, and so break the bottom of the engine; but, in these double engines, the operator is, in a manner, wholly free from that toil. For, in the first suction, the *Emboli* are easily lifted up, because the air, immediately derived from the receiver R, into the pumps, presseth the suckers downwards, almost as strongly, as the external air, incumbent on the opposite part; and, when the quantity of the internal air is diminish'd, the sucker, to be depress'd, tends downward with the greater force, and so, by means of the cord GGG, compassing the pulley, draws the other *Embolus* upwards, and, at the same time, hinders it from descending with too great velocity; and, by this means, both suckers, at one and the same time, will assist the pumper. And, as the *Emboli* make but a very small resistance, the two pumps of this engine may be ply'd with greater ease and expedition, than one pump in single engines; whence, this contrivance is of great use in those experiments, which cannot well be made slowly.

The whole gage ABCDE, consists of three glass-tubes, all well cemented together, so, that a passage remains open, from one to the other; the first of these tubes AB, being open at the extremity A, is of less capacity, than the tube BCD, but of greater, than ED. The tube BCD,

is

is crooked in the middle, and the tube ED, ought to be hermetically seal'd, PNEUMATICA
at the extremity E; but the part BCD, must be fill'd with mercury.

If this instrument, thus prepar'd, be put into a receiver, out of which, the air is to be extracted, the air remaining in the part ED, will, by its spring, compress the mercury DCB, and force it to ascend into the part BA, and itself will be dilated in the cavity DC. If, then, the following proportions be duly observ'd, between the magnitude and length of the tubes, when the air is extracted, the mercury will almost reach to the top A, and the air in the other leg, being so dilated, that it cannot sustain a greater body of mercury, will remain included in that space.

But, that this instrument may exactly shew the quantity of the air produced in a receiver; the tubes AB, ED, are to be distinguish'd by marks into several parts: and, when the *Torricellian* experiment is made, upon the plate LM, of the pneumatic engine, as *Fig. 70.* a receiver FGE, is to be taken, perforated, at the top F, and the tube HI, is to be transmitted thro' the hole, that so the receiver may be apply'd to the plate: and, then the hole F, being stop'd, and the gage ABCDE, put into the receiver, the air is to be exhausted: the air, then, being dilated in the receiver, the mercury cannot be sustain'd so high in the tube HI, but must descend by degrees; and, at the same time, the air of the tube ED, gradually drives the mercury into the tube AB. Now, when the mercury, in the tube HI, descends to the height of twenty-nine inches, and remains at that height, if we mark how high the mercury hath ascended into the tube AB, we may know, that, as often as the mercury in the gage shall rest at that height, the air, in the same receiver, will be able to sustain, only twenty-nine inches of mercury; whence that place in the gage must be marked with the figure twenty-nine: and so, every inch of the mercury's descent in the tube HI, may be marked in our mercurial gage, when the part AB, will shew all the degrees of the rarification of the air.

But, now, if the air be condens'd in the receiver, above its wonted pressure, and all ways of its escape be stop'd, it may immediately be known, by the tube ED; for the mercury will be impell'd into it, by the incumbent air, thro' the open hole so much the higher, as the compressure of the air in the receiver shall be the greater; and how great that is, and what an altitude of the mercury it can sustain, may easily be found, by computation, thus.

It has been prov'd, that the space possess'd by air, is diminish'd in the same proportion, as the compressing force increases, and *vice versa*.

Let then, the space A, be possess'd by a certain quantity of air, whilst the compressing force is F: if we increase that force by the addition of G, which is equal to it, our self-same quantity of air will be reduc'd to half its space, so that B, the remaining space, will be half of the total space A, as the former pressure F, is half of the total pressure F and G. And, if we further increase the pressure, by the addition of H, so that, the first pressure F, is only one fourth of the total pressure F and G and H, the air can possess only the space C, which is one fourth of the total space A. Thus, the remaining space will always be in the same proportion to the total space, as the first pressure is to the total pressure. So

So that the remaining space, being to the total space, as the first pressure is to the total pressure; three of these terms being known, it will be easy to find a fourth, by the rule of proportion. For instance, in our gage, let the tube ED, be the total space, into which the air is compress'd, by the usual pressure of the air, which, in *England*, is equivalent to thirty inches of mercury; the first pressure, therefore, will be thirty inches of mercury. Now, if that pressure be increased, and the air reduced into a less space, suppose into the space NE; to find the quantity of this pressure, I measure the remaining space NE, and constitute that, suppose six inches, for the first term of the proportion; then, the second term, will be the total space DE, suppose twelve inches; the third term, the height of thirty inches of the mercury, which was the first pressure; and so the fourth term, or total pressure, will be found to be sixty inches of mercury: whence I conclude, that the pressure of the air in the receiver can sustain the mercury to the height of sixty inches; and so of the rest.

From the same principle, it will be easy to find, what ought to be the proportion, between the size of the tubes AB, and ED. For that depends on the length of the legs, which, the higher they are, so much the better they restrain, and keep in the air, but little dilated, in the seal'd part. For instance, let the length AB, be ten inches, which height of the mercury is one third of the accusom'd pressure, and it is sufficient, that the tube HB, be twice as big, as the tube ED; for, after the mercury hath ascended to the top of the tube AB, the air included in the other leg, expanding itself into the space forsaken by the mercury, will possess three times more than its former space; and so one half of the first pressure, which is ten inches, will be sufficient to curb its spring. But, if the legs were shorter, the mercury would be expell'd, by the included air, at least in part. And, therefore, the magnitude of the tube AB, ought to have a greater proportion to the magnitude of the tube ED, that the ascending mercury may afford more space to the air, to be dilated; so that the spring of the air being weaken'd, the weight of the mercury cannot be overcome. And, thus it would happen, if the height of the gage were to the height of thirty inches, in the same proportion with the first space of the air, to the total space it would possess *in vacuo*.

The height of the tube, should rather be too long, than too short; because, if it be too short, the mercury will be expell'd in part, and so, not shew all the degrees of rarification; but, if it be too long, the mercury will, only, not reach to the top, and so the gage will, nevertheless, shew all the variations, tho' they be less sensible.

But the tube DC, ought to contain a sufficient quantity of mercury, at the least, to fill the tube AB, before any passage be open'd for the air included in the tube ED.

In our engine to compress the air, AA, is a glass-vessel, whose orifice is exquisitely fitted to the flat plate BB.

BB, is a flat plate of brass, made to close the vessel AA exactly.

CC, a

CC, a small tube of brass, passing thro' the middle of the plate, and fastened thereto.

E, a little valve, opening inwardly, to shut the small tube C.

F, the spring depressing the valve E.

GGG, the gnomon fastened to the plate BB, made for restraining the spring F.

II, a square lath, sustaining the plate BB, and bored thro' in the middle, to transmit the little tube C.

LLL, LLL, two iron-wires, which, passing thro' the holes in the lath II, and compassing the upper part of the iron-plate KK, hinder the plate from being much moved from the lath.

KK, an iron-plate, with a hole in the middle, formed into a female-screw, to receive the male-screw MM.

MM, an iron-screw, straitly to conjoin the receiver AA, with the plate BB; and, lest the brass-vessel should be broken, it is proper to put some wood and leather between the screw, and the upper part of the receiver: leather, also, is to be put upon the plate BB, both to prevent the breaking of the glass, and the more exactly to shut the receiver.

NN, a pump fastened to the tube C, below the plate BB.

OO, the sucker of the pump NN.

P, a little hole in the lower part of the pump, by which the air enters into it, when the sucker is brought to the lowest part thereof.

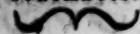
To compress the air by means of this engine, we put the bodies, whereon the experiment is to be made, into the receiver AA; and laying it on the plate BB, firmly bind it thereto, by help of the screw MM. This done, the sucker or plug OO, is to be drawn, till the external air, by the hole P, can fill all the upper part of the pump; then, if the sucker be drawn upwards, the air finding no other passage, will open the valve E, and enter into the receiver AA; from whence there is no regrefs, because the valve E, is presently depressed by the spring F, and shuts the hole C. And so we may repeat the compression of the air into the vessel AA, at pleasure; whilst the quantity thereof is easily known by the mercurial gages.

But I so fashion the pump, that it may be fitted, by a screw, to the tube C; for, thus, when one receiver is full, we may take away the pump, and use it to fill others.

Now, because, in these engines, mercurial gages serve to shew the degrees of compression; there is no occasion for the gages before described; for those are made with more difficulty, and, besides, afford but a small space, wherein to note the degrees of compression. It is, therefore, better to bend the glass-tube, seal'd at one end, in several places, as in the figure T, that a long tube may be contain'd in a short receiver; so that the mercury, being put in thro' the open end, as much as will suffice to fill the length of one inch; all the rest of the space, fill'd with air, will serve for marking the degrees of compression, much more sensibly than can be done in a shorter tube. Fig. 73.

Here

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To mix liquors or
powders in com-
press'd air.
Fig. 72.

Here we must note, that when the mercury tends downwards, in such an inflected gage, the weight thereof forwards the external pressure; but when it is impell'd upwards, the same weight resists it: a difference to be regarded in very accurate experiments.

In order to make mixtures in compressed air, let the receiver be AA, in which we would mix either liquors, or powders.

Let QQ, RR, be two tubes, each of them seal'd at one end, and open at the other.

Let RQS, be a vessel of brass, to be laid upon the orifice of the tubes, as in the figure.

The liquors to be mixed, must be poured into the tubes QQ, RR, each liquor in its own tube; and let the vessel RQS, being inverted, be laid on the orifices of the tubes; and, in that posture, let all be cover'd with the receiver AA; let the screw be driven, and the air intruded after the manner just described: and when the gage TT, shews, that the compressure is arrived at the degree intended, the engine is to be inverted, and so the liquors will flow down from the tubes into the vessel RQS, and be mix'd there. If more liquors, or powders, are to be mix'd, the number of the tubes is to be increas'd accordingly.

To make and
remove artifi-
cial air from
one receiver into
another.

To transmit air out of one receiver into another, we use the following contrivance.

AA, is a flat plate of metal, with a hole in the middle.

BB, is the stop-cock, fastened to the hole in the middle of the plate AA, one of whose ends is form'd into a male-screw.

Fig. 74. & 75.

DC, is a copper-funnel, open below, with a broad orifice, (that it might be easily set upon the pneumatic engine, and there stand firm;) and the upper part of the orifice D, is fashion'd into a female-screw, to receive the male-screw of the stop-cock BB.

EE, is a small tube, open at both ends, which are cut into a female-screw, to receive the male-screw of the stop-cock BB.

FF, is the receiver laid on the plate AA, and exquisitely fitted thereto.

Now, to make factitious air, we must put the matter which is to produce the air, into the receiver FF; and placing that on the plate AA, by means of the screw, we strongly fasten it thereto, as in our engine for compressing the air; the stop-cock BB, we insert into the female-screw D: then the orifice C, and with it the receiver, is to be placed upon the pneumatic-engine, and the stop-cock B, being open'd, the air is to be extracted. When the receiver FF, is emptied of air, the stop-cock B, is to be shut, that all passage to external air into the receiver may be denied; and the stop-cock, being taken out from the female-screw D, the receiver is presently to be immers'd in water; so that, at least the plate AA, with the stop-cock, may be cover'd therewith: thus no air from without can find entrance; and the air, produced out of the matter in the receiver, will be preserv'd unmix'd; whilst the degrees of its rarification, or compression, are known, as those of common air.

But

But if we would transmit that air into another receiver; another receiver FF, with another plate AA, and a stop-cock BB, is to be procured, and evacuated: then, by means of the small tube EE, we join the stop-cocks BB of, both receivers, when all suspected places are to be stopp'd with cement, that no external air may find entrance. Then, the stop-cocks being open'd, the air, produced in the former receiver, flows into the latter; and the stop-cocks being again shut, and pluck'd out from the tube EE, the receivers may be kept a-part: when if there be any matter included in the latter receiver, we may easily view what influence the factitious air hath upon it.

But, because the mercurial gages, lately describ'd, are spoil'd, if they be inverted, and the crooked gages presently expel their mercury, if the air be rarify'd in their receivers; and, since the operation, here describ'd, cannot be perfected, but both receivers must be inverted, and both, likewise, emptied of air; gages of another sort are to be made, after the manner following.

AA, is a glass vial, fill'd with mercury to the superficies DD.

BB, is a glass tube, very well cemented, in the orifice of the vial.

CC, is another tube, transmitted thro' the tube BB, and reaching to the bottom of the glass. This tube must be seal'd above, and open below; neither must it so exactly fill the tube BB, but that passage may be given to the external air, within the glass AA.

If this instrument be put into a receiver, from which, the air must be, afterwards, extracted, both tubes will be exhausted of air; and, when you invert the receiver, to take in new air, as in Fig. 74. the mercury will flow down to the orifices of the vial, and be there kept, below the orifice of the tube BB; when the new air entering, will easily fill both tubes, and the vial: then, the receiver being erected, the mercury will again rest, in the bottom of the vial, and the orifice of the tube CC, will be plung'd in it. And, if any air be produc'd, out of the bodies included in the same receiver, the mercury will ascend into the tube CC, and there, reducing the air into a narrower space, shew the degrees of compression.

The instrument wherewith we filter'd air thro' water, was thus contrived. To filter air thro' water.

AA, is a glass receiver, whose orifice, laid upon the plate BB, agrees exquisitely therewith. Fig. 77.

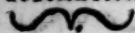
BB, is a plain plate with a hole in the middle, to transmit the tubes CC, DD.

CC, DD, are two tubes cemented to the plate BB; one of which is no higher than the plate, but the other reacheth almost to the top of the receiver.

EEEE, is a stop-cock, to whose holes the extremities of the tubes CC, DD, are fastned.

FF, is the key of the stop-cock unperforated, wherein is only one chink GG.

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HH, is the receiver, compassing the end of the stop-cock, and fastned to it, preventing the entrance of the outward air, and communicating with the pump II.

LL, is a glass vessel.

M, is a hole in the top of the receiver, whose stopple is fastned with a screw.

Fig. 78.

The next figure exhibits a stop-cock, cut transversly, that the two tubes CC, DD, may be the better distinguished, and their insertion into the stop-cock be perceiv'd.

This instrument is thus to be used: we put the thing about which the experiment is to be made, into the vessel; and the receiver AA, being laid on the plate BB, we pour water into the hole M, till the receiver be about half full, and the vessel LL, with the matter contain'd therein, swims on the top thereof; then we stop the hole exactly, and fasten it with a screw. The key is afterwards to be set so, that the chink GG, may communicate with the tube CC; then the plug being brought to the lowest part of the pump, the air of the receiver AA, entring through the upper orifice of the tube CC, will flow down thro' the chink GG, into the receiver HH, and into the pump. Then the key being inverted, so that the chink GG, may answer to the insertion of the tube DD, the plug is to be impelled upward; when the air will be expelled from thence, and, finding no other passage, be driven through the chink GG, into the tube DD; and from thence it will emerge to the upper part, through the water stagnant in the receiver. And by repeating this process, we strain the air thro' the water, as often as we please; and thence know whether it acquires any new qualities, in respect of the body included with it.

How to condense
and rarify the
same parcel of
air.

Fig. 79.

Let the receiver AA be placed upon the plate BB, and screwed on to it.

CC, is the stop-cock, fastned to the hole in the midst of the plate BB.

DD, is a pump joined to the stop-cock C, with a screw.

E, is a vessel, so large, that it may fluctuate in the receiver AA, without danger of being over-turn'd.

Let some animal be put into the vessel E, and let the receiver AA, be put upon it, and screwed to it, as the figure shews. Then let the pump be fill'd with water, and, by a screw, be fitted to the stop-cock; the stop-cock, being then open'd, let the plug C, be forced upwards, and the water ascending through the stop-cock will, in part, fill the receiver AA, and reduce the air, contained therein, into a narrower space, without any addition of new air: if, then, you draw the plug downwards, the same numerical air will be again rarified. Thus you may both condense and rarify the same air as often as you please; and, by this means, you may find, whether the condensation of the air contributes to prolong the life or health of animals.

A wind-gun.

In our wind-gun AA, is a hollow copper globe.

BB, a tube, fastned to the globe.

F, a valve opening inwardly, and shutting the tube BB.

Fig. 80.

G, the

G, the spring depressing the valve.

H, a gnomon affixed to the globe AA, and making fast the spring G.

CC, a tube of iron, fastned to the tube BB, and the globe AA.

DD, a plug exactly fitted to the tube.

EEE, another plug fitted also to the tube BB, with an iron wire, reaching almost to the valve F.

R, the protuberance of the tube CC, somewhat hollowed above, to receive the end of the iron LL.

LL, a crooked iron, moveable about the extremity in R, so that it serves as a lever to lift up the plug EEE.

OPO, a crooked iron, fastned in M, that the thumb resting in the angle P, the rest of the fingers may attract the lever L, and so force the plug EEE, upwards. But the curvature is design'd, that the one end O, might be applied to the shoulder, in aiming at a mark.

TT, a rectangular piece of iron, compassing the lever LL and the iron OPO, to keep the lever in its posture; for, otherwise, the plug EEE, would be thrust far out, whilst the air is intruded into the globe AA.

II, an elliptic hole, in the upper part of the globe, very well shut with a valve, opening inwardly, to give liberty of inspection, and of amending what is amiss; for the valve may be drawn through the hole, by reason of its elliptic figure.

SS, a metalline plate transversly placed above the hole II, and perforated to transmit the screw V, by help whereof the valve, shutting the hole II, is sustained, and applied closely to it.

Q, a hole in the lower part of the tube CC, by which the air enters into the tube, whilst the plug D, is brought to the lowest part thereof.

The air is forced into this engine, by setting the foot upon the crooked end of the plug DD, that it may not be removed from the ground, and lifting the engine upward, till the upper part of the plug comes below the hole Q; and then the air entring through the hole, wholly fills the tube CC.

Then, by forcibly depressing the engine, the air, contained in the tube CC, opens the valve F, and is thrust into the globe AA; whence it cannot return, because the valve presently stops the passage: and thus, by repeated strokes, we may condense the air in the globe, till the force of its spring cannot be overcome by our strength.

If we would discharge the air so condensed, the plug DD, is wholly to be drawn out, and a bullet to be put into the bottom of the tube CC: then, by means of the lever LLL, the plug EEE, is to be impell'd upward, as we said before; when, the extremity of the iron-wire, opens the valve B, and the air breaking out therefrom, expels the bullet through the tube CC, with great violence.

But before the plug DD, is again put into the tube CC, for the compression of the air, about half an ounce of water is to be pour'd into the tube. For, by this means, no air at all can escape out by the plug; and, moreover, that water exactly filling the upper part of the tube CC, the

PNEUMATICS. whole compressed air will be intruded within the cavity AA; and so the condensation be perfected much sooner, than if, at every turn, part of the compress'd air remain'd below the valve F.

This engine has several advantages above the common wind-guns. 1. Because one valve serves, both for the letting in, and discharging the air; whence it is less subject to be spoiled, or impaired, than if two valves were used for that purpose. 2. If any disorder happen in other guns, they remain useless; but here, by the elliptic hole, we may take out the spring and the valve, and so mend whatever is amiss. 3. In other guns, the valves being cover'd with leather, are put in, before the engine is closed on every side; and therefore silver-solder could not be used in joining the parts, but only lead-solder, by which, the air, being much compressed, could, by no means, be restrained; but here all things are well cemented with silver-solder, without danger of burning; since the valve, cover'd with leather, is put in afterwards thro' the elliptic hole II. 4. But this engine is chiefly to be preferred before others, because, here we can put several bodies into the receiver, through the elliptic hole, and so make many experiments in highly compressed air.

An engine
wherewith to
distil in vacuo.
Fig. 81.

We, also, contrived an engine, which should distil *in vacuo*, thus.

AA, is a brass vessel, shut below, and open above.

BB, a diaphragm of tin, whose edges are so polish'd on both sides, that they exquisitely agree and suit with the edges of the vessels AA, DD, which are also polished, and so keep out the external air.

CC, a tube fasten'd to a hole in the middle of the diaphragm BB.

DD, a brass vessel, whose aperture is applied to the diaphragm BB.

EE, a stop-cock fastned to the hole of the diaphragm BB.

FF, a tube reaching from the stop-cock EE, to the hole made for suction in the pneumatic engine.

GG, a metalline vessel, including the junctures of the vessels with the diaphragm, and also the stop-cock, that being filled with water, it may keep all safe from the external air. This is to be solder'd to the vessel AA.

To use this engine, we take away the diaphragm BB, and put the ingredients into the vessel AA, and set it in a convenient place, till it is to be evacuated; then putting on the diaphragm BB, and the vessel DD, we apply all to the pneumatic engine, and by means of the tube FF, the air is pumped out of the vessels, the vessel GG being yet first filled with water. Then the stop-cock is shut; and taking away the tube FF, we may place the evacuated engine on the fire, when the vapours, ascending through the tube CC, are condensed in the upper vessel, and so we have a liquor distilled *in vacuo*. The quantity of the generated air, is known by the mercurial gage H; but that must be kept in the top of the receiver, lest the mercury exhale, by reason of the heat.

Round pieces of paper, perforated in the middle, are to be laid over the orifices of the vessels AA, DD, that they may be the better joined with the diaphragm; the commissures of the tube FF, with the stop-cock, and pneumatic engine, are to be fortified with cement; and the stop-cock EE,

is

is so to be disposed with the vessel GG, that part of the key may be prominent, without the vessel, thro' the hole, to be conveniently turned; nevertheless, the stop-cock, with the diaphragm, may be taken out of the vessel GG, whilst the vessel AA, is to be filled with the designed matter. And that is very easily done, because the key consists of two parts, one of which M, is turned in the stop-cock itself, by means of a certain chink, which receives the small protuberance of the other part OO, that exactly fills the small pipe NN, fastned to the vessel GG; and being prominent outwardly, may easily be turned in it, and communicate its motions to the other part M: but it is drawn outward, whilst the diaphragm BB, is to be taken out of the vessel GG.

Fig. 82. shews another instrument, differing from the former, in that it, almost, wholly consists of glass, and affords a longer passage for the vapours.

BB, is not a diaphragm, but a small tube, polished at both ends, that it may exquisitely suit with the orifices of the vessels A, and D.

AA, DD, are two glass vessels, whose orifices are applied to the tube BB; whence the vapours are easily transmitted from the one to the other.

EE, FF, GG, and I, have the same use as in the former figure; and the whole instrument is to be evacuated after the same manner, and placed upon the fire; except that here the vessel AA, as being made of glass, must not be put on an open fire, but set in *balneo Mariae*, or on sand; and the vapours will be condensed in the vessel DD.

(1.) July 11. 1676. I included a little piece of bread, very moist, and a little kneaded, with a mercurial gage, in *vacuo*. Several ways to forward the production of air; and first, air produced from bread.

July 12. In six hours time, no air was produced yesterday; but this night, a little broke into the receiver, and sustain'd three inches of mercury; for I had neglected to fortify the cover with turpentine.

Towards the evening, I found the mercury higher by about an inch; and am very certain, that nothing had entred from without.

July 13. This night, also, the mercury ascended higher; but my gage was not exact enough to discover how many degrees.

July 26. The bread disjoined its receiver from the cover, by the force the air produced, and the smell of it was acid.

Hence it follows, that water is a fit menstruum to draw air out of bread.

(2.) July 11. I tried to extract air from bread, by the help of a burning-glass, wherewith I burnt bread in *vacuo*, and found it generate much air, which, ever and anon, broke out, as by fulmination; whence it seems probable, that air is contained in bread, but so closely compacted therein, that no easy operation can give it vent; but that if any thing could dissolve and loose that knot, it may then produce great effects.

(3.) Sept. 22. I took eight ounces of dry'd grapes, and, with seven ounces of water, included them in a receiver, able to hold twenty-two ounces of water. The grapes were bruised. From grapes.

Sept. 23. The receiver lay buried under the water all this night, yet the mercury ascended two whole inches.

Sept.

PNEUMATICS.

Sept. 30. In seven days time the mercury rose to the height of thirteen inches.

Octob. 5. In five days more, the mercury ascended twelve inches, and was now twenty-five inches high.

Octob. 18. The mercury continued not to ascend with the same swiftness, and the air began to pass out of the receiver; but not before this day; yet these grapes produced much more air, than those which I included without water.

From raisins.

(4.) *July* 12. I included ten ounces of raisins of the sun, bruised; *in vacuo*, with a sufficient quantity of water, to promote fermentation.

July 14. In two days they had produced ten inches of air.

About evening, the mercury was fifteen inches high: the fifteenth day the mercury had almost reached to its accustomed height.

July 16. In the morning, I found the receiver sever'd from its cover; and the air breaking out thro' the water, in which it was plunged, I included the same raisins again *in vacuo*.

July 18. This day, in the morning, I found the air again breaking out.

July 19. I shut up the same raisins in the same empty receiver.

July 21. This day I found the receiver full, and the air breaking out of it.

I again shut up the same raisins in the same exhausted receiver.

July 23. Yesterday, about noon, I found the whole receiver almost full of air; and this day, in the morning, perceiv'd it to pass out very often.

It appears, then, that grapes without water, can generate but little air; whence it is manifest, that water is a fit medium to draw air out of them: 'Tis also evident that the production of air is not begun presently upon the affusion of water, but proceeds with greater swiftness, after the parts of the water, in five or six days time, have more deeply sunk into, and pervaded the grapes.

From plumbs.

(5.) *Aug.* 13. 1677. I included pears in two exhausted receivers, and plumbs in another.

Aug. 16. In three days time, all my receivers were filled with air, newly generated; and one of them, which included the pears, because I had left it exposed to the sun, was, in the space of 24 hours, separated from its cover: whence we may conjecture, that the production of air is very much promoted by the heat of the sun.

From grapes.

(6.) *Octob.* 16. 1677. I took two ounces of grapes bruised, and secured them from the air, in an exhausted receiver, capable of containing twenty ounces of water.

Octob. 17. The mercury rose higher about one half-inch.

Octob. 18. These last twenty-four hours, the mercury ran up about another half-inch.

Octob. 20. The height of the mercury was two inches.

On the twenty-second, it was almost four. And, on the twenty-seventh it was almost six inches.

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Jan. 2. 1678. The mercury, yet, ascended not to the height of ten inches. PNEUMATICS

Octob. 16. 1677. I put three ounces of bruis'd grapes, with half an ounce of spirit of wine, into a receiver, able to hold thirty ounces of water; and then I exhausted the air.

Octob. 17. The mercury ascended but a very little.

Octob. 18. The mercury came not up to the height of one quarter of an inch.

Octob. 20. The mercurial gage was out of order.

Jan. 2. 1678. I, this, day found my receiver fill'd with air; and, also, when part of the liquor was pour'd out, some bubbles were form'd in the turpentine, about the orifice, and broke outwardly.

From this experiment, made in two receivers together, it seems to follow, that spirit of wine much advances the production of air *in vacuo*; tho', in common air, it wholly hinders it.

(7.) July 19. 1678. I put must, expressed from grapes bruis'd, and kept From must. for ten months in a vessel, stopp'd with a screw, into the same receiver, being also stopp'd with a screw.

July 21. The mercury had not ascended at all.

23d. The height of it was three.

24th. The height was five.

25th. In the morning it was an hundred and four.

Towards the evening, the height was an hundred and thirty-seven; and the must got out.

26th. The must was almost all got out of the receiver; and altho' the air now possess'd double the space it did yesterday, yet it kept up the mercury to the same height.

27th. About half of the remaining must broke out this night, because I had omitted to set the screw, lest the receiver should be broken.

From this experiment it follows, that grapes, kept for so long a time, rather acquire, than lose a fermentative virtue.

(8.) Jan. 30. I put two quantities of apples, boil'd the day before, into From apples. two receivers, stopp'd with screws; with one of them I mix'd a third part of sugar, the other had no sugar at all. These receivers were quite full.

Jan. 31. I included raw apples, bruis'd, in three receivers; in one of them I mix'd a third part of sugar; the second was without sugar, and so was the third; but it differ'd herein from the second, that it was six times as big: for, by this means, we may know, whether the capacity of the vessel, or the mixing of sugar, or the crudity of the fruit, can promote, or retard the production of air.

Febr. 10. In that receiver, only, which contain'd the raw apples, with sugar, some air was produc'd.

Febr. 14. The raw apples, with sugar, had impell'd the mercury up to thirty inches; those that were boiled with sugar, to two only; in the other receivers no air was produced.

Febr.

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Febr. 18. In the receiver, containing the raw apples, with sugar, the mercury came to the height of fifty-six inches; in that containing the boil'd apples with sugar, the height was three: in the other receivers, there was, also, some air produced, except in that wherein the boiled apples, without sugar, were put. I open'd that receiver, in which the apples had produced so great a quantity of air; yet the apples seem'd hardly to be fermented, but had a most pleasant taste.

Febr. 21. The boil'd apples, without sugar, had lost some of their juice; and, opening the receiver, I found the cover broke, and yet the apples were not at all rotten.

March 1. In the great receiver, containing the raw apples, the mercury was twenty-five inches high; in the little one, only seven: but in that where were the apples boil'd with sugar, the mercury had ascended to nine inches.

March 8. In the great receiver, the height of the mercury was twenty-nine; in the lesser, twenty-two and a half; and where the boil'd apples, with sugar, were, the altitude was nine inches.

March 17. The juice got out of the great receiver; in the little one, the height was sixty-seven; where were the apples boil'd with sugar, it was fifteen inches.

From this experiment it seems, that sugar, the crudity of the fruit, and the largeness of the receiver, all contribute to the production of air.

Several ways to hinder the production of air, for instance, in paste.

(9.) *December 21. 1678.* I made paste of wheat-flower, without leaven, and put it into an exhausted receiver; then I put the receiver in an apartment, with a fire, which there kept a greater heat than is usual in the middle of summer; yet the paste produced no air in ten hour's space: whence it seems to follow, that if dough hath once suffer'd too much cold, it can scarce recover its faculty of fermenting; for, some years ago, when I made dough without leaven, in the summer-time, it soon produced very much air *in vacuo*.

(10.) *May 23.* I included three ounces of dough, kneaded with leaven, in a receiver, capable of holding fifty ounces of water; I, also, pour'd upon it some quantity of spirit of wine, to try whether fermentation would be hinder'd by that means.

May 24. The mercury was three inches high.

26. Little change.

27. No change.

May 29. No change.

June 2. It seem'd to have ascended a little higher.

14. No change.

December 14. No more air being produced from the dough, I took it out of the receiver, and found the smell of it not grateful, but inclining to acid: I put it into an empty receiver, and there it swell'd to double its usual space, and made a little ebullition.

May 23. I included three ounces of dough, kneaded with leaven, in a receiver, able to hold fifty ounces of water; but here I mixed no spirit of wine.

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May 24. The mercury was nineteen inches and a half high. | May 26. 'Twas 38 inches high.

27. There was no change.

December 14. The mercury continued at the same height; and, this day, opening the receiver, I found the dough had a very acid smell.

From this experiment it seems to follow, that spirit of wine, even in dough kneaded with leaven, hinders the production of air.

(11.) August 29. I included pears, with a mercurial gage, in a receiver full of water, and then intruded air into it, till the mercury rested twenty-six inches higher than usual; within a quarter of an hour, one of the pears was broken, and, afterwards, almost all of it reduced to pulp.

Aug. 30. In twenty-four hours space, the pears seem'd to have afforded no air; but, on the contrary, the mercury in the gage was depressed an inch and a half.

Aug. 31. I found no change in the height of the mercury.

Sept. 1. The pears began to produce air, and the mercury was almost twenty-seven inches high.

Sept. 2. In twenty-four hours time, the mercury ascended more than eight inches; and now 'twas thirty-five inches high.

Sept. 3. The height of the mercury was increased seventeen inches; so that now it was about fifty-two inches high.

Sept. 4. Within twenty-four hours, the mercury rose seven inches higher, and then rested at fifty-nine.

Sept. 5. It was sixty-four inches high; and a pear, being broken, was become black.

Sept. 6. Three inches, and more, being added to the height of the mercury, it came now to sixty-seven inches, and one fourth, beyond what it was accusom'd.

Sept. 7. It descended three inches, and rested again at sixty-four.

Sept. 8. The mercury was depressed to fifty-eight inches; and some of the water having broke out, I set the receiver with a screw.

Sept. 9. The mercury ascended full three inches, and was now suspended above sixty-seven.

Sept. 10. In twenty-four hours it mounted one and a half, and stopp'd almost at sixty-nine.

Sept. 11. Now it began to descend again, and stood no higher than sixty-seven inches; yet, I am certain, nothing had escaped out of the receiver; but it was a sharp cold night.

Sept. 12. No change happen'd.

Sept. 13. The height of the mercury again decreased, and it was not above sixty-four inches. The cold increased.

Sept. 14. In twenty-four hours, it became higher by six inches, reaching to seventy.

Sept. 16. It was about sixty-nine inches high. | Sept. 20. It again ascended to 71.

19. It remained the same.

23. The mercury was again depressed to sixty-nine.

Octob. 1. It came to the height of seventy-five inches.

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D d d d

Octob.

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Octob. 3. Yesterday I found no change at all in the mercury; but to-day it rested at seventy; and the cold was very severe.

Octob. 5. Yesterday the mercury remain'd in the same place; but this day it reach'd to seventy-five. It was a rainy day.

Octob. 7. It continu'd rainy; and the mercury continu'd at the same height.

Octob. 10. Hitherto the mercury was not changed; but this day I found it had descended to sixty-nine inches; tho' the rain ceas'd not.

Octob. 12. Yesterday the mercury stood still; but this day it was depress'd to sixty-five inches; and the cold weather return'd.

Octob. 13. The height of the mercury was sixty-four.

14. } The height { sixty-nine.
15. } was { seventy-four.

24. The height was sixty-eight. It was a cold season.

Nov. 2. The height was sixty-four. The cold increas'd.

Nov. 5. The height was eighty and a half. The cold abated.

22. The height was sixty-five. It was a hard frost.

27. The height was sixty-eight. It thaw'd.

Decemb. 6. The height was sixty-one. It was a very severe frost.

From this experiment we may learn, that fruits, in a great compression of the air, cannot produce so great a quantity of air; for when I made an estimate of the quantity of the fruits, and of the small space to be fill'd with air; I found that quantity of air was not one eighth part of what had been produced in a large empty receiver: tho' the coldness of the water might, also, hinder the generation thereof, as the following experiment will shew.

'Tis farther manifest, that the air is produced by starts, and, as it were, by reciprocations; as all bodies, in motion, by the force of their gravity, or of their spring, are carried beyond their point of rest, and so make many vibrations, or returnings. And tho' cold and heat are not the sole causes of such reciprocations, yet they seem to contribute much thereto.

In paste again.

(12.) *Feb. 22. 1677.* I included ten ounces of paste in a receiver, that would hold twenty-two ounces of water; and, afterwards, I thrust as much air into it, as sufficed to sustain seventy-three inches of mercury, besides the wonted pressure. In two hours space I perceived no sensible change.

Febr. 23. In eighteen hours time, the mercury rose seven inches only, its height being eighty.

In six hours it ascended three; and its height was eighty-three.

Febr. 24. }
25. } Its height was { 90
26. } { 97
27. } { 101
28. } { 105
March 1. } { 107
 } { 112

And water seem'd to be express'd out of the mass.

March 2. } Its height was { 120
3. } { 121

4 & 5. It remain'd at 121.

March

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March 8. During these two or three last days, the frost breaking, the mercury ran up four inches; and, the height thereof was one hundred and twenty-five.

March 10. Yesterday, the mercury remain'd at the same height; but, this day, mounting six inches, it rested at one hundred and thirty-one.

March 21. The cold continuing long, no air was produc'd; but, in the three last days the mercury ascended seven inches, and remain'd at one hundred and thirty-eight.

April 4. Yesterday, the mercury had ascended, but I deferr'd measuring the quantity, till to-day; in the night, one of the iron wires, that straitned the receiver, was broken, and the receiver thrown to the distance of four or five foot.

Hence we may conjecture, that the compression of the air, very much hinder'd the production thereof; for, that is usually perform'd, in paste, in two or three days time. Cold, also, much hinders its production.

(13.) *March 1. 1677.* I included two ounces of bruised raisins of the sun, *In raisins and* with six ounces of vinegar, in a receiver; upon which, numerous bubbles *vinegar.* broke out.

March 2. The mercury, in twenty-four hours space, ascended not to the height of half an inch; yet, some bubbles still appear'd.

March 25. The vinegar always appear'd interspers'd amongst some of the bubbles; yet, the mercury ascended not to the height of one inch.

Hence it appears, that vinegar hinders the production of air and fermentation; for, raisins, of themselves, afford much air.

(14.) *April 7.* I included ten ounces of paste, in a receiver capable of *In paste.* holding twenty-two ounces of water; afterwards, I intruded as much air into it, as sufficed, to sustain one hundred and twenty-eight inches of mercury, besides its accustom'd height.

In six hours time, the mercury rose four inches, and rested at one hundred and thirty-two.

April 8. In sixteen hours the mercury ran up nine inches higher, and staid at one hundred and forty-one.

Nine hours after, the mercury manifested no change.

April 9. In the morning, I perceiv'd some air had broke forth, and the mercury was depress'd to one hundred and thirty inches; therefore, I screw'd the receiver tighter, and thrust in eleven inches of new air: the height was one hundred and forty-one.

Apr. 10.

11.

12.

13.

The height
was

{ 151
158
168
176

Apr. 14.

15.

16.

17.

The height
was

{ 183
183
187
191

April 27. For eight whole days the mercury kept its station; but, on the two last, it ascended seven inches, and continu'd at one hundred and ninety-eight, above its wonted height.

April 30. The mercury persisting at the same height, I eased the screw, so that some air might break out; and, when the mercury had so

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far descended, as to exceed its accustom'd height, only fifty inches, I presently set the screw; to see, whether, that remission of the spring of the air, would afford any place for new air to be generated; and, in two or three minutes time, I found the mercury to have ascended, sensibly higher.

Three hours after, the mercury was found twelve inches higher; for it came to sixty-two.

In five hours, it ascended one inch and a half.

May 1. In fifteen hours, the mercury rose, only, one inch.

May 3. Yesterday, it appear'd at the same height, but this day, 'twas higher, by one and an half, and remain'd at sixty-six.

May 4. The mercury was not chang'd, and, therefore, I suffer'd all the air to escape; but, I could not quickly set the screw: whence it is probable, that very much air, which, at that time, was produc'd, got out of the receiver; nevertheless, after the receiver was again well stopp'd, I perceiv'd, that two inches of air, and more, had been produc'd in five or six minutes time.

May 7. The mercury, in three days, again amount'd two inches.

May 8. The mercury was higher by half an inch.

May 11. During these two last days, the mercury, again, ran up half an inch. I set this mass, almost unfit, as it seem'd, to produce air *in vacuo*; when, in five minutes space, the mercury ascended to the height of one ch.

May 21. It ascended not quite three inches.

May 30. The mercury rested at the height of four inches and a half.

By this experiment, it appears, that all the air producible from paste, may, after a sort, be generated in a great compression; yet, it is somewhat restrain'd thereby; for, in a less compression, it will soon break out.

Hence, also, we see, that air is producible by starts; and, that it rises more slowly in compress'd than in free air: for, such a production in the latter, is usually over, in two or three days time.

Plumbs and apricocks in artificial air.

(15.) July 30. 1677. I included plumbs and apricocks, many of them being first cut asunder, in a receiver, and, afterwards, as much air, produced out of cherries, as was sufficient to sustain sixty-four inches of mercury.

August 1. The fruits had produced no air, but grew yellower than those which were in common air.

August 3. The mercury rose a little higher, and the apricock, which remain'd whole, seem'd full of drops, like water.

August 7. The whole apricock grew softer; the mercury stood, at fifty-nine inches above its usual station.

Aug. 8.

9. } The height
10. } of it, was
11. }

61
65
71
74

Aug. 13

14 } The height
15 } of it, was

78
80
80

16 and, the days following it remain'd at the same height.

24. The height of it was seventy-seven; tho', I certainly knew, that nothing had issued out of the receiver.

29. Find-

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29. Finding, neither the fruits, nor the height of the mercury, changed any more, I open'd the receiver, and perceiv'd, that the apricocks had kept their colour, very well; but the flesh of them was spongy, and their taste inclining to acid. Many bubbles had broke from them, at the time, they were freed from the surrounding pressure.

July 30. 1677. I included the halves of the fruit, just mention'd, in a receiver full of common air; and, with them, others of the same kind, uncut.

July 31. The mercury had gain'd the height of eight inches.

August 1. At six a-clock, in the evening, the mercury was twenty-one inches high; but, in the other receiver it remain'd unmov'd.

August 3. They kept their firmness much better than those included with artificial air. The height of the mercury was thirty-five inches.

August 4. The height of the mercury was forty-two inches.

August 6. The whole apricock, seem'd not at all alter'd. The height of the mercury was fifty-seven.

Aug. 7 } The height { 81 8 } of it, was { 95	Aug. 9 } The height { 113 10 } of it, was { 124
---	--

The colour of the whole apricock, yesterday, began, and now proceeded to grow yellow. No moisture appear'd.

Aug. 11 } The height { 131 13 } of it, was { 157 14 } { 163	Aug. 15 } The height { 171 16 } of it, was { 171 17 and, the days following, it remain'd at the same height.
---	--

August 27. The height was one hundred and eighty-two.

August 29. When, neither the fruit, nor the height of the mercury changed any more; I open'd the receiver, and found the apricocks of a more acid, and less grateful taste, than the others, in factitious air; tho' their pulp was of a very good colour, but spungy: they also yielded many bubbles, as did the others.

Hence, 'tis probable, that the artificial air of the cherries, greatly hindered the apricocks from producing air; tho' it promotes the alteration of their colour and firmness; and, also, serves to preserve their taste.

(16.) October 10. 1677. I included an ounce and an half of bruised, unripe grapes, in a receiver, that would hold ten ounces of water; and drew out no air.

Octob. 11. The mercury ascended a little.

12. There was but a small change.

13. The height was half an inch.

17. The height was one inch.

18. The height was one and an half.

19. The height was, almost, four.

20. The height the same; but some mouldiness appear'd on their superficies.

21. The height was four and an half.

22 } The height remain'd the
 23 } same, but the mouldiness
 24 } encreased.

26 } 27 } 30 }	The height was { 5 $\frac{1}{2}$ { 6 { 6 $\frac{1}{2}$ { 7 $\frac{1}{2}$
----------------------	---

Nov. 2

Nov. 6,

PNEUMATICS.

Nov. 6 }
8 }
9 }
12 }
14 }

The height was

9 }
10 }
12 }
15 }
17 }

Nov. 18 }
21 }
Dec. 8 }
12 }
27 }

The height was

23 }
26 }
36 ¹/₂ }
39 }
39 }

Grapes with
spirit of wine.

Jan. 6. 1678. The height was 36. The air broke out.

Octob. 10. 1677. I made the same experiment in another receiver, observing the same circumstances; only here I mixed two drams of spirit of wine with the grapes.

Octob. 11. The mercury was not changed.

12. There was no change.

13. The mercury was not moved.

Oct. 17. It ascended a little.

18. The height of it was not yet a quarter of an inch.

19. It was moved but a very little.

Jan. 6. The grapes, during all this time, had produced no air.

Whence it appears, that spirit of wine hinders fermentation.

A peach in an
exhausted receiver,
with spirit
of wine.

(17.) Octob. 17. 1677. I put a peach into an exhausted receiver, with some quantity of spirit of wine, which could not touch the peach, unless in vapour.

March 27. 1678. I took out the peach, which had kept its colour, but lost its firmness. Though the receiver was small, yet it was not filled with air; for when open'd, the air seemed to rush into it: the peach being softned, was so depressed, that the lower part of it touch'd the spirit of wine; the superior part, also, had contracted the taste of the spirit of wine, as well as that which was immersed in it.

Peaches in air
with spirit of
wine.

(18.) Octob. 17. I included five peaches in an unexhausted receiver; and with them, some spirit of wine, which could not touch the peaches, unless it were elevated in vapour.

Octob. 18. The mercury ascended not at all.

20. The height of the mercury was 3 ¹/₂.

21 }
22 }
23 }
26 }

The height

of it was;

5 ¹/₂ }
7 ¹/₂ }
9 ¹/₂ }
9 ¹/₂ }
12 }

Nov. 6 } The height { 14
12 } of it was { 16
14 } It kept the same
16 } height.

Dec. 8 } The height { 18
16 } of it was { 19 ¹/₂
27 } of it was { 20 ¹/₂

Jan. 6. 1678. it was 23.

March 28. 1678. it was 31 ¹/₂

Peaches in air
without spirit of
wine.

Octob. 17. I included five peaches in a receiver full of common air, without spirit of wine.

Octob. 11. The mercury ascended not at all.

Octob. 20. The height of the mercury was five inches.

Octob.

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PNEUMATICS.

Octob. 21	} The height of it was	8	Nov. 12	} The height of it was	20
22		10	14		20
23		11	16		21
26		12	Decemb. 8		26
Nov. 2	} The height of it was	15	16	} The height of it was	26 $\frac{1}{2}$
6		17 $\frac{1}{2}$	27		28 $\frac{1}{2}$

Jan. 6. 1678. The height was 32.

March 28. 1678. The height was 33 $\frac{1}{2}$.

April 15. The liquor in the lower part of the receiver, had all broke out, and the air followed it ; then I took out the peaches.

Hence we learn, that the very vapour of spirit of wine, somewhat hinders fermentation ; yet much less than the spirit itself.

(19.) April 27. 1678. I included an ounce and a half of paste, mixed with *Paste with leaven, in common air.* leaven, in a receiver full of common air, able to hold twenty-three ounces and a half of water.

April 28. The height of the mercury in the gage was two and a half.

April 30. The height of it was three and a quarter.

May 4. The mercury was depressed, tho' no air broke away, and the paste was mouldy. The height of it was two and a half.

May 6	} The height of it was	2 $\frac{3}{4}$	May 17	} The height of it was	4 $\frac{1}{2}$
8		3	20		5
10		3 $\frac{1}{2}$	24		6
14		4	28		8
June 2	} The height of it was	9	July 5	} The height of it was	13 $\frac{1}{2}$
6		10	19		15
14		10 $\frac{1}{2}$			

April 27. 1678. I included an ounce and a half of unleavened paste, with *Paste, without leaven, in common air.* common air, in a receiver, capable of holding twenty-three ounces and a half of water.

April 29. Hitherto the mercury had not ascended ; but this afternoon it rose a quarter of an inch.

April 30. There was no change.

May 4. The mercury ascended but very slowly, and the paste was mouldy.

May 6. The height of the mercury was four inches.

May 8	} The height of it was	5 $\frac{1}{2}$	May 24	} The height of it was	16
10		7 $\frac{1}{2}$	28		18 $\frac{1}{2}$
14		10 $\frac{1}{2}$	June 2		20 $\frac{1}{2}$
17		12 $\frac{1}{2}$	6		21 $\frac{1}{2}$
20		13 $\frac{1}{2}$	14		25

Hence it seems, that leaven rather hinders than forwards the production of air, if the paste be not made in a hot place.

(20.) May 23. I included an ounce and a half of unleavened paste, in a *Paste with spirit of wine.* receiver capable of holding twenty-five ounces of water, and pour'd spirit of wine upon it.

May

PNEUMATICS.

May 24. The mercury was one inch high.

May 26. It was almost two inches high.

27. It was two and a half.

31. There was no change.

June 2

6

10

July 19

No change.

The height of it was $\left\{ \begin{array}{l} 3\frac{1}{2} \\ 4 \\ 4\frac{1}{2} \end{array} \right.$

Decemb. 14. When the height of the mercury alter'd no more, I open'd the receiver, and found that the paste had an acid smell.

Paste without
spirit of wine.

May 23. I included an ounce and a half of unleavened paste, in a receiver, capable of holding twenty-five ounces of water; but added no spirit of wine.

May 24. The mercury ascended not.

May 26. It was three inches high.

May 27

28

29

31

June 2

4 $\frac{1}{2}$ 5 $\frac{1}{2}$ 7 $\frac{1}{2}$ 9 $\frac{1}{2}$

12

June 6

10

July 4

19

The height of it was $\left\{ \begin{array}{l} 17 \\ 22 \\ 30 \end{array} \right.$

The mercury a little exceeded thirty inches. This day the air broke out, and, therefore, I set the screw.

Decemb. 14. The mercury return'd to the height of fifteen inches; when, I open'd the receiver, and found the paste very acid.

Hence it seems to follow, that spirit of wine greatly obstructs the production of air; and the more, if the paste be fermented; and that unfermented paste will, in tract of time, produce no less air than that which is fermented.

New ale included
in receivers.

(21.) Octob. 11. I exactly fill'd a receiver with new ale, so that no air might be left; and included another quantity of the same in another receiver, wherein some space was allow'd for the air.

Octob. 12. The cover of that receiver, which contain'd some air, was broken; and, therefore, I pour'd the same ale into another receiver, wherein there was room enough left for the air: in the receiver, exactly fill'd, the mercury ascended a little.

Octob. 13. In the receiver, exactly fill'd, the height of the mercury was twelve inches; in the other, thirteen inches; tho' it had been shut up a shorter time, and a much larger space was left, whereinto the air, newly produced, might have been dilated.

Octob. 14. In the full receiver, the height was thirteen; in the other, eighteen. Towards evening, the full receiver work'd the fastest; for the height of the mercury in it was twenty-two; and in the other but twenty.

Octob. 15. In the full receiver, the height of the mercury was forty-two; in the other, twenty-six. And some bubbles of air, which, in the full receiver, had possessed its upper part, wholly vanish'd; and the ale possessed a long space, in the mercurial gage, wherein it was not found before.

Octob.

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PNEUMATICS.

October 16. In the full receiver, the height was 60 inches.

In the other 30.

18. In the full receiver, the height was 90.

In the other 40.

22. In the full receiver, the height was 90.

In the other 42.

23. In the full receiver, the height was 108.

In the other 50.

26. In the full receiver, the height was 108.

In the other 60.

28. In the full receiver, the height was 133.

In the other 63.

The bubbles appear'd again, yet nothing flow'd out.

Nov. 8. The full receiver lost much of its liquor; wherefore, I opened it; when, all the ale seem'd as if it would have vanish'd into froth, unless I had suddenly stopp'd the little hole, that gave it vent. I many times tried, that, if the hole were opened in the gage, the mercury would presently descend; but, if the hole were again stopp'd, it would speedily ascend. The ale had a most pungent taste.

Nov. 9. I opened the other receiver, and observed almost the same things.

Hence it seems to follow, that ale, if the air be wholly excluded from the containing vessel, will ferment more slowly, than if some air be left therein; and that, in time, it makes a greater compression, if no room be left for its dilatation.

(22.) June 27. I put green pease into an exhausted receiver, with spirit of wine. Towards the evening, the receiver seem'd to admit the external air, and the mercury rose to the height of eighteen inches, when I closed the cover with turpentine. Pease, with spirit of wine, in an exhausted receiver.

June 30. I perceived no more change in the height of the mercury.

July 7. No air was produced, even in the most vehement heat.

June 27. I put other pease into an exhausted receiver, without spirit of wine. The receiver, and the quantity of the pease, were the same as in the last experiment. Pease without spirit of wine, in an exhausted receiver.

June 28. The receiver was full of air; tho', I think, it was not exactly shut; and, therefore, I again included the same pease. Towards evening, the height of the mercury was five inches.

June 29 } The height of it was { 10 } July 5 } The height of it was { 26
30 } { 16 } 7 } { 30
July 1 } { 19 }

July 8. The air got out of the receiver.

Hence it appears, that spirit of wine hinders the production of air in pease.

(23.) June 9. 1677. I put cherries into an exhausted receiver, and in six hours time the mercury came to the height of five inches and a half. That the effects of artificial air differ from those of the common, shown in cherries.

PNEUMATICS.

June 20. The mercury ascended three and a half. Towards the evening it was two.

The ascent is here always to be understood, as added to the former.

<i>June 21</i>	$1 \frac{1}{2}$	<i>June 26</i>	3
<i>22</i>	$1 \frac{1}{2}$	<i>27</i>	3
<i>23</i>	2	<i>28</i>	5
<i>24</i>	$1 \frac{1}{2}$	<i>30</i>	$1 \frac{1}{2}$
<i>25</i>	$1 \frac{1}{2}$		
<i>July 1</i>	3	<i>July 4</i>	$2 \frac{1}{2}$
<i>2</i>	4	<i>5</i>	3
<i>3</i>	2		

The height was forty-eight. But, transmitting the air into another receiver, the mercury was depressed to thirty-five inches.

July 6. The ascent of the mercury was four inches, in one night's time.

7. The ascent of it was five and a half in twenty-four hour's time.

8. The ascent of it was $5 \frac{1}{2}$.

9. The ascent of it was $5 \frac{1}{2}$.

10. The ascent of it was 6 .

11. The ascent of it was twelve, in the space of thirty-four hours.

12. The ascent of it was seven.

13. The ascent of the mercury was three; the height about ninety two inches; but the air being transferr'd into another receiver, the mercury rested at fifty.

14. The ascent was 14 .

15. The ascent was 11 .

16. The ascent was 13 .

17. The ascent was 5 .

18. The ascent of the mercury was 9; the height of it 102.

19. The height of the mercury was 92; for 1 transmitted part of

the air into another receiver.

20. The ascent of the mercury was 15.

21. Some air got out, and the height of the mercury was $63 \frac{1}{2}$.

22. The ascent of it was $12 \frac{1}{2}$.

23. The ascent of the mercury was 4; the height of it 79 inches; but the air, being transmitted into another receiver, the mercury rested at 62.

24. The ascent was 8 .

25. The ascent was 9 .

26. The ascent was 4 .

27. The ascent was 4 .

28. The ascent was 5 .

29. The ascent of it was ten, the height ninety-eight. Part of the

air being transmitted into another receiver, it rested at sixty-four.

30. The ascent of the mercury was 6 .

31. The ascent of the mercury was 9 .

Aug. 1. The ascent of the mercury was 4 .

2. I transmitted the air into another receiver, and the mercury remain'd at sixty-eight.

3. I transmitted the air again into another receiver, and the mercury rested at fifty-four.

6. The

6. } The ascent of the mercury was { 7.
7. } { 4.

8. There was no ascent.

9. The ascent was three inches.

The receiver being opened, I found the cherries of a whitish colour, and of very little taste, tho' not ungrateful; their flesh was spongy.

Hence it seems to follow, that cherries contain much air, and that they produce it very irregularly.

(24.) July 13. 1677. I put cherries into an exhausted receiver; and then transmitted into the same, as much air, produced from other cherries, as sufficed to sustain fifty inches of mercury.

July 15. Yesterday, the mercury had not ascended at all; but this day it was two inches higher; that is, twenty-two, above its wonted station.

July 16. The height of the mercury was twenty-three and a half.

July 17. The height of it was twenty-five. More air was forty-five. More air escaped.

26. The height of it was forty-three. Some air got out.

30. The height of it was fifty-two.

27. The height of the mercury

31. The height of it was sixty-one inches.

August 1. The height of the mercury continued nearly the same, tho' the air broke out.

August 27. The air, having been all broke away for some time, I took out the cherries, and found them not to have lost their colour, as in the former experiment: they had contracted no putrefaction, nor mouldiness, but tasted a little more acid than usual; and being open'd, there were many cavities in their flesh, as in fermented paste, or dough, but not quite so thick.

From this experiment, compared with the former, it may, probably, be inferred, that in artificial air, fruits produce less air, and so the better preserve their colour and taste; for the cherries, in the former experiment, remained in the receiver, not much longer than in this.

(25.) Septemb. 10. 1677. I put six ounces of unripe grapes into a receiver, with common air, capable of containing twenty-five ounces of water; and stop'd it firmly by means of a screw. Grapes consumed air.

September 11. The mercury ascended not at all.

Sept. 12. The mercury stop'd a little below one inch.

Sept. 13

14 } The height of { 3 $\frac{1}{2}$
15 } it was { 7
16 } { 10
17 } { 12 $\frac{1}{2}$
 { 14

Sept. 18

19 } The height was { 16
20 } { 18
21 } { 20
22 } { 22
 { 23 $\frac{1}{2}$

September 23. The height of it was 27. The grapes were not alter'd.

September 24. The height was 30.

25. The height was 31. The grapes began to grow yellow.

E e e e 2

Septemb.

PNEUMATICS.

Septemb. 26 } The height { 32 | Sept. 29 } The height { 35
 27 } of it was { 34 $\frac{1}{2}$ | 30 } was { 35
 October 1. The height remained at 35.
 Octob. 2. } The height was 36 | Octob. 10 } The height was { 35
 5 } | 35 } The height was { 32 $\frac{1}{2}$
 6 }

The air got not out, but the cold began, and increased.

Novemb. 9. The height remained the same.

Decemb. 19. Almost all the air escaped.

Decemb. 20. I took out the grapes, and found by their smell and their taste, that they had contracted some mouldiness, tho' not discernable by the eye. They were more firm than before.

Grapes in factitious air.

(26.) Septemb. 10. 1677. I included two ounces of crude grapes in a receiver, capable of holding eight ounces of water; and to the common air added air produced out of pears, till the mercury rested ten inches above its ordinary station.

Sept. 11. The mercury descended, and its height was eight inches.

Sept. 12. The height of it was 11. the ascent 3.

Sept. 13 } The height { 16 | Sept. 15 } The height { 23
 14 } of it was { 20 | 16 } was { 24

Sept. 17. The height was 28. The grapes turned yellow.

Sept. 18 } The height { 29 | Sept. 22 } The height { 35
 19 } of it was { 30 | 23 } of it was { 20
 20 } of it was { 31 | Some air had broke out; and the
 21 } of it was { 33 | grapes were of a yellow colour.

Sept. 24 } The height of the { 25

25 } mercury was { 22

26 The height almost the same.

27 } The height of it was { 22

29 } of it was { 27

30 } of it was { 28

Octob. 1. and 2. The height 28.

Octob. 5 } The height { 30 | Octob. 10 } The height { 31 $\frac{1}{2}$
 6 } was { 31 | 13 } was { 31

Novemb. 9. The height was 13. Some air had got out.

December 19. The height of the mercury was 20 inches.

Decemb. 20. I took out the grapes, and their smell and taste were more grateful than of others; their firmness rather increased, than diminished.

Hence, factitious air seems fit to alter colour, and to preserve taste; but the firmness might be increased here, as in turpentine; the spirits, in time, being exhaled.

Oranges in common and factitious air.

(27.) July 18. I took two pieces of orange, and, by the help of a screw, stopped them close up in a receiver, with common air; when, into the same receiver, I put air, produced out of cherries, as much as sufficed to sustain

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12 inches of mercury. At the same time I put a piece of the same orange PNEUMATICS.
into another receiver, with common air alone, and uncompress'd.

July 20. The orange in the common air began to contract a mouldiness; but the other seemed not at all alter'd.

July 23. The mouldiness of the orange, in the common air, increased; the other piece remained sound.

July 16. The orange, in the common air, did not increase its mouldiness, but seemed wholly rotten: the other also began to putrefy, but remained free from mouldiness.

Aug. 1. Perceiving that the oranges were no longer sensibly changed, I open'd the receivers; and tho' the air, wherewith I had mix'd the artificial air, was so compressed in its receiver, that it could not now sustain twenty-six inches of mercury above its wonted pressure, yet the fruits were far better preserved in it, than in the other; only the superficies seemed to have lost its juice; but all the inner parts, with the rind, were very well-colour'd, well-tasted, and firm: in the other receiver, the whole orange seemed almost rotten, as well as the rind. The orange was more corrupted in the compressed air, because, as it seems, no factitious air had been mixed with it.

It seems worth observing, that the same air, generated from cherries, is apt to produce different effects, upon fruits of a different kind; for here it retarded the alteration of colour and firmness, which, when I included air with apricocks, it accelerated.

(28.) July 20. 1676. I included a small piece of beef in an exhausted receiver, and put as much air, produced from cherries, into it, as sustain'd 27 inches of mercury. Beef in factitious air.

July 21

22

23

25

The mercury remained almost at the same height.

July 26. The beef had removed the receiver from its cover; and because it was very fetid, we threw it away.

July 20. 1676. I put a piece of beef into a receiver full of common air, and carefully stopp'd it in, by means of the screw. Beef in common air.

July 21. The mercury had not at all ascended in the gage.

July 22. The height of the mercury was one inch.

23

25

26

27

The height of
it was

5 $\frac{1}{2}$.

9 $\frac{1}{2}$.

14 $\frac{1}{2}$.

21 $\frac{1}{2}$.

In the evening { 18.
25.

28. The screw, not being tight, suffer'd the air to break out.

Hence it appears, that air produced from cherries, is a great hindrance to the production of air from flesh.

(29.) March 14. 1676. I put two onions into a receiver, full of common air, with a mercurial gage; and fastned the stopple with a screw, to see whether vegetation would increase, or diminish the quantity of the air. Onions in common air.

March

PNEUMATICS.



March 28. The mercury seemed depressed one quarter of an inch; but it afterwards recover'd its former height, and two inches more; and now the air broke out, and the roots grew longer.

April 28. About ten or twelve days since, I perceived the roots to be corrupted; and now they were wholly putrefied.

May 9. The mercury continued at the same height; for the air had broke away: and, therefore, I took out the onions, and found their roots putrefied, but they were not at all mouldy.

Onions in factitious air.

(30.) *March 17. 1676.* I included two onions in an exhausted receiver, and afterwards put air, produced from paste, into it.

March 28. The onions took root, at least, as well as those which I kept in the common air.

April 28. The ends of the roots began to putrefy, yet they were in far better case, than those surrounded with common air. Perhaps, the cause of this difference is, that a greater quantity of water was included with the artificial air. The mercury mounted higher by nine or ten inches.

May 18. Hitherto the onions seemed not at all corrupted; but this day I found one of them to be a little so; tho' different from a mouldiness.

Hence we may gather, that artificial air doth not at all hinder vegetation; and that not only the sensible magnitude of the body, but also the quantity of the air, is increased by vegetation.

Unripe grapes in common air.

(31.) *August 25.* I included six ounces of unripe grapes in a receiver, capable of holding twenty-five ounces of water; but did not exhaust the air.

August 26. The mercury ascended a little.

27. The height of the mercury was 1 inch.

28. The height of it was $1\frac{1}{4}$.

29. The height was $1\frac{1}{4}$.

August 30. The mercury seemed to have descended, rather than ascended. The colour of the grapes was less alter'd here, than in the receiver, containing air produced from pears.

Aug. 31. The receiver broke, and I left the grapes exposed to the free air.

Sept. 7. The grapes being left in the free air, still kept their green colour, and were of a grateful taste, tho' less pungent than before.

Unripe grapes in factitious air.

August 25. I included two ounces of unripe grapes in a receiver, capable of holding eight ounces and a half of water; and having stopp'd it close with a screw, I filled it further with air, produced from pears, till it sustained 15 inches of mercury.

August 26. Some air escaped, and therefore I crowded in new, produced out of the same pears, till the mercury rested 17 inches above its wonted height.

August 27. The mercury was depressed below the 16th inch; yet no air had broke out. Towards evening, the mercury again ascended to 17.

Aug. 28

29 } The height of it was $\left\{ \begin{array}{l} 19 \\ 21 \\ 22 \end{array} \right.$

30 }

Aug. 31

Sept. 1

2

The height of it was $\left\{ \begin{array}{l} 23\frac{1}{2} \\ 24 \\ 24 \\ 24 \end{array} \right.$

Sept.

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Sept. 4. The last height continued, and the grapes had all contracted a yellow colour. PNEUMATION.

Sept. 5. The air broke out.

Sept. 7. The air continuing to get away, by degrees; I took out the grapes, and found them very insipid, and of an ungrateful taste.

This experiment confirms the efficacy of artificial air, to alter the colour of fruits. 'Tis, also, very observable, that here it damaged the taste, and promoted the production of the air, contrary to what had happened in the former experiments. It might be worth while to try, whether the same would happen in all unripe fruits.

(32.) August 2. 1676. I shut up a July-flower in a receiver, with air produced from paste, made with meal, and not mixed. A July-flower in factitious air.

August 4. The flower began to change its colour, and to grow moist.

August 9. The July-flower was a little alter'd.

August 12. The moisture gradually increased, but no mouldiness appear'd.

August 31. The July-flower seem'd little alter'd, tho' it was less fresh than those which were kept in *vacuo*.

August 2. I shut up a July-flower in a receiver, with common air, not mixed. In common air.

August 4. The flower was not changed.

August 9. It grew moist, and had almost lost all its colour.

August 12. A great mouldiness cover'd all the flower.

Aug. 2. I included two July-flowers in *vacuo*, and took special care, that no humidity should be included with them. July-flowers in *vacuo*.

Aug. 4. 1676. One of them began to appear moist.

Aug. 31. 1677. During the whole year, the July-flowers had suffered no change.

Hence it seems probable, that factitious air hastens the change of colour, yet it prevents mouldiness as a *Vacuum*.

(33.) July 24. I put apricocks and some plumbs, several of which were cut in pieces, in a receiver full of common air, and stopped it firmly with a screw. Apricocks and plumbs in common air.

July 25. The mercurial gage was spoiled; so that I could not, by any means, perceive the quantity of the air generated.

July 30. The fruit seemed not at all alter'd, except that one of the cut plumbs had contracted something of mouldiness.

Aug. 2. I opened the receiver, and found all the fruit firm, of a good colour, and a grateful taste.

July 24. I made the same experiment in another receiver, with the same circumstances; only into this last receiver, I intruded air, produced from cherries, till it sustain'd twenty-two inches of mercury. The same in artificial air.

July 25. The mercury descended three inches, and rested at nineteen. Toward the evening, it recover'd its former height, and rested at twenty-two.

July 26 } The height of it was { 28
27 } { 34 } | July 28 } The height of it was { 36
29 } { 40 }
July

PNEUMATICS.

July 30. The height was forty-four. The apricocks which were cut, began to moisten, and dissolve into water.

July 31 } The height was { 51
Aug. 1 } { 60

Aug. 2. The height was sixty-five. Towards evening, when some liquor had escaped out of the receiver, I screwed it tighter; but one of the iron-wires being broken, all the air got away. Wherefore, I took out the fruits, and found them very soft; especially those, whose lower parts were immers'd in the water: the rest were a little more firm, but all of them retain'd a grateful taste.

Hence it seems, that air produced from cherries, promotes the alteration both of colour, and firmness in apricocks.

It appears, also, that some part of such air is destroyed at the first.

Plumbs in common air, in artificial air, and in vacuo.

(34.) July 30. 1676. I put plumbs, cut asunder, into three receivers; one of which was full of artificial air, produced from goosberries; the second, full of common air; and the third exhausted.

Aug. 2. In the artificial air, the plumbs were not changed; in the common air, they began to be mouldy; but in the evacuated receiver, they retain'd their colour, and were soft.

Aug. 5. In the artificial air, the plumbs had contracted a red colour, humidity, and softness; in the common air, they seemed black and mouldy, yet retain'd their firmness; in the evacuated receiver, they were almost dissolved.

Aug. 7. The plumbs, in the common air, began to soften.

Aug. 8. The plumbs, in the common air, seemed to have lost their black colour, and to have contracted a red one; as it happen'd three days before, to the plumbs in the artificial air.

In this experiment, artificial air seems to have promoted an alteration.

Peaches in common and artificial air, mixed.

(35.) Sept. 24. I put five peaches into a receiver, with common air, mixed with some produced from grapes; and included the grapes themselves in the same receiver, that the common air might be the better saturated with the artificial.

Septemb. 25. The height of the mercury was twenty-one inches.

Sept. 26 } The height of it was { 23
27 } { 31
18 } { 39 | Sept. 29 } The height of it was { 42
30 } { 45
Octob. 1 } { 48

Octob. 2. The same height continued.

3. The height of it was 52 and a half.

5. The height the same; but the peaches seem'd moist.

6. The height of it was 58.

7. The height of it was the same.

8. The height of it was 61.

11. The mercury ascended a little.

19. The height of it was 65.

25. The height of it was 61. The cold was sharp.

27. The cold abated, and the mercury ascended.

30. The height was 61, and a little more.

Nov.

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PNEUMATICS.

Nov. 9. The height of the mercury was 59. 'Twas severe cold weather.

6. The height was 61. The frost broke, and it thaw'd.

7. The mercury seem'd somewhat higher.

9. The mercury persisted at the same height.

Dec. 9. In one month's space, the mercury ascended, by degrees, to the height of eighty inches.

April 1. 1678. The mercury came to ninety-six inches above its wonted height. I now opened the receiver, and whilst the air was breaking out, the peaches emitted many bubbles thro' their skins, not without a violent noise; and the skin, in some of them, was broken: they had preserved their taste, and the colour of their pulp; but lost their firmness, as if they had been boil'd: being left in the air for three hours, they were all rotten.

This experiment proves, that common air corrupts bodies, tho' much the less for being mixed with factitious air.

(36.) Aug. 4. The first receiver. I cut five pears, each of them into four parts; and put one part of each into a receiver full of common air, and stopp'd it close with a screw.

Aug. 6. The colour of them was little alter'd, and the mercury ascended not at all.

Aug. 7. The pears were little alter'd; and the mercury was a little higher.

Aug. 8. The pears underwent no great change; the height of the mercury was four inches.

Aug. 9. The height of it was four and a half.

Aug. 10 }	The height	{ 6	Aug. 13 }	The height	{ 16
11 }	of it was	{ 10	14 }	of it was	{ 20

The pears began to be soft.

Aug. 15. The height of it was 21.

16. The height of it was 19. I believe the air had got out.

17. Now I found the air had escaped.

18. The air being almost all got out, since yesterday in the evening, and the fruit looking worse, I took the pieces out, and found them putrefied.

Aug. 4. The second receiver. I took one quarter of each of the afore-said pears, and included them, after the same manner; and, afterwards, added air, produced out of cherries, till the mercury possess'd twenty-three inches extraordinary.

Aug. 6. The fruit was not alter'd, except a little in their colour.

Aug. 7. Almost all the pieces seem'd rotten; the mercury remaining at the same height.

Aug. 8. The pears were not alter'd much; but I could not see the mercury.

Aug. 10. They, gradually, grew softer; and the mercury was forty inches above its wonted height.

Aug. 11 }	The height	{ 51	Aug. 14 }	The height	{ 67
13 }	of it was	{ 61	15 }	of it was	{ 73

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Aug.

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Aug. 16. The mercury descended; yet nothing had got out.

Aug. 17. The mercury exceeded not sixty-seven inches in height; yet the air could by no means escape.

Aug. 18. The mercury remain'd at the same height; but, suffering the air to break out, it had a sharp odour; and the taste of the fruit seem'd very acid, and the pulp exceeding soft.

Pears in an un-
stopp'd receiver.

August 4. 1677. The third receiver. I put a quarter of each of the afore said pears into a receiver, not exactly shut.

Aug. 6. The pears seem'd to change their colour.

Aug. 7. One of the pieces began to lose its firmness; but, in the artificial air, another piece yesterday seem'd wholly rotten.

Aug. 8. One piece was mouldy; the rest were soft.

Aug. 9. The pears gradually grew more rotten.

Aug. 11. They were wholly mucid, and rotten.

This receiver, compared with the first, shews, that corruption begins not in the free air sooner than in included air; but, when begun, that it is much more violent and sudden; because the included air may be fatiated.

Pears in vacuo.

August 4. 1677. The fourth receiver. I included one quarter of each of the said pears in vacuo.

Aug. 6. The height of the mercury was 5.

Aug. 7	} The height of it was	8	} Aug. 13	} The height of it was	20	
8		10			14	23
9		12			15	25
10		14			17	28
11		16				

20. Hitherto the pears had undergone no alteration; but this day they began to be soft. The mercury ascended not.

Aug. 26. Neither the pears, nor the height of the mercury, were at all alter'd.

This production of the air seems very regular.

Hence we find the aptness of artificial air to soften fruits.

And that the production of air was here promoted by artificial air, is very probable; tho' it had succeeded otherwise with apricocks.

Apricocks in
common air.

(37.) August 21. 1677. The first receiver. I divided six apricocks, each into four parts; and put one piece of each into a receiver full of common air, and stopp'd it firmly with a screw.

Aug. 22. The apricocks seemed riper than yesterday; but no air was produced by them.

Aug. 23. One piece, contiguous to the water, began to be mouldy, and the rest inclined to putrefaction. The mercury seemed to have ascended a little.

Aug. 24. A piece next the water, was cover'd with much mouldiness; another piece, more remote from the water, was somewhat mouldy also; but all were rotten.

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Aug. 25. The fruit contracted no more mouldiness; but the putrefaction increased. The height of the mercury was seven inches.

Aug. 26. The height of the mercury was 15.

28. The height of it was 30.

29. The same height continued.

30. The height of it was 33. The fruits were almost all dissolved.

31. The height of the mercury was 38.

Septemb. 1. The height of it the same.

2. The same height still.

3. The mercury ascended a little.

Septemb. 4 } The height { 41 | Septemb. 7 } The height { 45
5 } of it was { 43 | 8 } of it was { 46

Septemb. 9. The same height continued.

Sept. 22. Little or no change appear'd in the height of the mercury; but the fruit was almost dissolved into water.

Octob. 1. When the mercury continued at the same height, and the fruit seem'd almost vanish'd, I open'd the receiver, and found the apricocks very much impaired and soft; yet they retained a taste not unpleasant, but tending to acid.

Aug. 21. 1677. The second receiver. I cover'd one quarter of each of the aforesaid apricocks, with a receiver, not defended against the external air. *Apricocks in an open receiver.*

Aug. 22. They were flaccid, as if they had been dry, or wither'd.

Aug. 23. Many of them appear'd rotten and mouldy.

Aug. 24. The apricocks were wholly putrefied, and mouldy.

Aug. 21. The third receiver. I included firmly, by the help of a screw, one quarter of each of the aforesaid apricocks, in an unexhausted receiver; to which I, afterwards, added air produced from pears, till it sustain'd 20 inches of mercury. *The same in one unexhausted, with an addition of facitious air.*

Aug. 22. The mercury ascended not at all; but the fruit seemed to have acquired a greater degree of maturity, than that included in common air.

Aug. 23. These seemed less alter'd, than those which were in common air.

Aug. 24. They remain'd unalter'd.

Aug. 25. The fruits began to produce air, but I could not discern the quantity.

Aug. 26. Little alteration in the fruit.

Aug. 28. It began to moisten, yet was far less alter'd than that which remain'd in common air.

Aug. 30. The mercury emerg'd above the bodies, by which it was hid. Its height above the wonted station, was thirty inches.

Aug. 31. The height of the mercury was forty inches.

Sept. 1. The height of it was the same.

2. The same height continues.

3. The height 45.

8. The height was little changed.

9. The height 40; yet no air got out.

11. The height was 38.

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11. The



12. The mercury continued to descend.

13. The height of it was 33.

Sept. 14. The mercury was so depressed, as to appear no more.

Sept. 22. The mercury emerged again; its height was 33. The fruit was cover'd with a kind of mucor.

Octob. 1. When neither the apricocks, nor the height of the mercury, were any more alter'd, and the mucor vanished, I open'd the receiver, and found the apricocks not impaired, but of a good colour, their pulp spongy and soft, and of a taste inclining to acid.

Apricocks in an unexhausted receiver, whose air was afterwards condensed.

Aug. 21. The fourth receiver. I took a quarter of each of the said apricocks, and shut them up firmly, with a screw, in an unexhausted receiver; into which, afterwards, I intruded air, till the mercury rose 90 inches above its standard height.

Aug. 22. Our receiver broke into an hundred pieces, by the force of the air compressed within it; whereupon, I put the fruit into another, and added only such a quantity of air as was able to sustain sixty inches of mercury.

Aug. 25. The apricocks had contracted much mouldiness; and I added new air.

Aug. 26. They were wholly infected with mouldiness and rottenness.

This receiver, if compared with the former, shews, that the quantity of corruption depends on the quantity of the air.

Hence we have it confirm'd, that alterations are made more suddenly in factitious air; and that, in time, the corruption is far greater in common air.

That the effects of compressed air differ from those of the common, shewn by onions in condensed air.

(38.) March 21. 1677. I put two onions into a receiver, which was to be stopp'd close with a screw; and intruded so much common air thereinto, as raised the mercury sixty inches above its usual station.

March 28. The onions took root as well as other onions which I included in common air at the same time.

April 28. The onions included in common air, eight days ago, were cover'd with mouldiness, though, in the beginning, they had shot numerous roots: the onions in the other receiver began to corrupt at the ends of their roots; but the compressed air, ten days before, had found a gradual passage out, and now was almost wholly escaped. I, therefore, put in new air, till the mercury had attain'd to the height of sixty inches above its usual standard.

April 29. The onions in the compressed air, were cover'd all over with mouldiness.

Hence it seems to follow, that a little compreffure doth not prejudice bodies to be expanded by vegetation.

And the new air, which was intruded, seems to have promoted the mouldiness, though, probably, in the beginning, the compreffure of the air retarded both the mouldiness, and the corruption.

Tulips and lark-spurs in common and compressed air.

(39.) May 9. I put two equal quantities of tulips and lark-spurs, into two receivers of an equal bigness, and stopp'd them up firmly with screws:

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I left one of them with common air only, but compressed the other by the intrusion of new air, till the mercury exceeded its wonted height by seventy inches. PNEUMATICS.

May 11. Two tulips, in the common air, contracted mouldiness; but all things remained unalter'd in the compressed air.

May 12. A third tulip, in common air, began to be finewed; but nothing like it happen'd in the compress'd air.

May 14. One tulip, in the compress'd air, was finew'd; but those in the common air, were all very mucid; and one of the lark-spurs, in the common air, had also contracted a mucor.

May 17. Three of the tulips in compress'd air, had contracted a finew; but not half so much as those in the common air. Two of the lark-spurs, in the common air, appear'd finewed also; but those shut up in compress'd air, were preserved fresh, and wholly free from mouldiness, or finew.

May 21. The flowers in the common air, were all rotten and putrefied; but those in the compressed air, received no further alteration: and the tulips, which had contracted some finew, seemed rather to lose it, than to acquire new.

May 30. When the flowers, in the common air, being wholly putrefied, were dissolved into water, I took them out, and kept the liquor in the vessel, to try whether any insects would breed therein. In the compressed air, the flowers suffer'd no more sensible alteration; I, therefore, took them out, and found them moist, and of an acid odour.

Hence, it seems that compressed air hinders putrefaction and mouldiness, in some plants.

(39.) *May 21. 1677.* I cut an orange into two equal parts, and inclosed one of the halves in a receiver, with air so compressed, that it would sustain an hundred inches of mercury above its wonted height: I left the other half in another close receiver, only with common air. *Orange in compressed and common air.*

May 25. Each half of the orange had contracted mouldiness; but that in the common air was much more mucid than the other.

May 26. The compressed air had entirely got out, and therefore I put in new.

May 30. I every day perceiv'd some air had escaped, and, therefore, daily supplied fresh. And the orange, by receiving new air so often, contracted a mucor, notwithstanding the compressure, much more than the other piece that was left in the same air without pressure.

June 1. I took out the two half oranges; and that which lay in the compressed air, seemed to have contracted a corruption, at least, three times greater than that which had continued in the common air.

Hereby the disposition of compressed air, to retard corruption, is confirmed; yet, in time, 'tis very probable, that the quantity of corruption may depend upon the quantity of the air.

(40.) *May 31. 1677.* I included two equal quantities of roses, in two receivers, stopp'd by the help of screws; into one of which I intruded as much *Roses in common and compressed air.*

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much air as would sustain ninety inches of mercury, besides its accustomed pressure; but I left the other with common air only.

June 11. The roses in the common air were free from mouldiness, only they seemed to have lost something of their colour: but those shut up in the compressed air, had almost all contracted a yellow colour, as if they had wither'd in the open air; yet they were not mucid, or finewed.

June 18. This last week, the flowers, in the common air, suffer'd not the least change; but those in the compressed air, grew yellower. I open'd both receivers, and found the roses to have retain'd their scent, yet it was somewhat alter'd; neither were they dry, or wither'd. I kept them apart in the open air, and found that those taken from the compressed air, were not so soon alter'd by the contact of new air, as those which had remained in uncompressed air.

Hence it seems to follow, that compressed air is sometimes fitter to alter colour than common air. And, perhaps, it may be worth our notice, that roses so included, contract not a mouldiness, but only a yellow colour; tho' in tulips and lark-spurs, 'tis otherwise.

(41.) June 1. 1677. I put the two halves of the same orange into two receivers; in the one I increased the quantity of air till it sustained the mercury an hundred inches above its wonted height; but left the other uncompressed, only exactly shut.

Orange in compressed air, and common.

June 6. Each half of the orange grew mouldy; especially that, whose ambient air was compressed. But new air was every day supplied; for the compressed air, in 24 hours time, had almost all got out. But in the former, it had remain'd very well shut in, for six whole days.

June 11. The orange, in the common air, contracted no more mouldiness; but, in the compressed air, the mouldiness gradually increased.

June 18. Finding the mouldiness of the orange, in the common air, to diminish, rather than increase, I took it out; and perceiving further, that, in compressed air, the orange was not more mucid, after I had ceased to intrude new air, I was willing to try, whether the new air supplied new strength to the orange, to exert and thrust out its mouldiness; and therefore, made the mercury in the gage, by means of the air intruded, to exceed its wonted height 80 inches.

June 20. Two days after I had intruded new air into the receiver, the mouldiness of the orange appeared to be manifestly greater.

Hence we may gather, that the quantity of the mouldiness depends on the quantity of the air.

Shrew-mice in common and compressed air.

(42.) June 17. 1677. I put two shrew-mice into two receivers, of equal bigness, and stopped them up carefully; in one of them I left only common air; into the other, I intruded air, till the mercury was higher, by 30 inches, than usual: the mouse, in the common air, was included about 52 minutes past 5 of the clock; and 6 minutes after the other.

The mouse, in the compressed air, seemed to lose his strength much sooner than the other, the motion of his breast being less frequent: yet, about 18 minutes after 6 o'clock, the mouse in the common air, which seemed the stronger

stronger, fell into convulsive fits, and died; but that in the compressed air, PNEUMATIC. seemed then, and some time after, to be as well, as he was an hour and half before.

About eleven of the clock, the mouse in the compressed air, still breathed; but, about four in the morning, he was found dead, in the same posture wherein he was seven hours before: whence we may conjecture, that he was free from convulsive fits.

I must not omit, that the mouse, in the common air, had consumed something of that air; so that the mercury stood at 29 inches, and, when the receiver was opened, presently ascended to 30.

Hence we learn, that compressed air seems fitter than the common, to prolong life; since the one mouse lived so much longer, tho' only a double quantity of air was included in the receiver.

(43.) *June 13. 1677.* I put four flies into a receiver, and afterwards intruded air, till the mercury rose sixty inches above its wonted height; and at the same time, included three other flies, in another receiver, with common air not compressed. Flies in common and compressed air.

June 14. In the morning, all the flies were well. In the afternoon, I found two of them dead in the compressed air; but in the common, they were all alive. About five a-clock, one of the flies, in the compressed air, was alive, and three in the common air.

June 15. This morning I found all the flies in the common air dead; but that single one which remain'd alive in the compressed air, seem'd still to be very well; and, being taken out of the receiver, flew briskly away.

Hence it seems, that flies are not very sensible of the air's compressure; and that they die more for hunger, than want of air: for the fly which remain'd so long well, fed upon the carcases of those which were dead; so that she seem'd not to be distemper'd.

(44.) *June 15.* I repeated the preceding experiment, only including four flies in each receiver, and compressing the air somewhat more.

June 16. This morning I found two of the flies, in the common air, dead; and but one in the compressed air.

About two in the afternoon, the four flies, in the common air, seem'd to be dead; but, in the compressed air, the three were alive.

Hence, the compressure of the air seems of small consequence to flies; and, indeed, they are not prejudiced by the rarification of it, without great difficulty, and unless there be almost a compleat vacuum.

(45.) *June 18.* I included two frogs in two receivers, and stopped them by the help of screws; the one only contain'd common air, the other, air compressed, till it sustain'd seventy inches of mercury. Frogs in common and compressed air.

June 19. Both the frogs were alive; and the height of the mercury, in both receivers, remained the same.

June 20. Neither of the frogs were dead; and they seem'd rather to

dimi-

PNEUMATICS. diminish, than increase the air: but the difference was so small, that I dare not be positive therein.

June 21. In the morning, both the frogs were alive; but, towards evening, that in the common air was found dead.

June 22. At evening the frog, in the compressed air, was alive.

June 23. In the morning I found it dead.

Oranges in common and compressed air.

(46.) June 18. 1677. I shut the two halves of the same orange, in two receivers, and stopped them by the help of screws; the one with common air, the other, with air compressed to sustain ninety inches of mercury.

June 22. This morning I found the orange, in the common air, mouldy; but the other was sound.

At three in the afternoon, the orange, in the compressed air, seemed, also, to have contracted some mucus.

June 23. The orange, in the common air, was far more mucid than the other.

June 24. The orange, in the common air, did not increase its mouldiness; but the other was cover'd all over with it.

June 28. The mouldiness, produced in the common air, was now wholly vanished: in the other receiver, I perceiv'd no further alteration in the fruit.

June 30. Both remaining in the same state, I took them out. The part which was kept in common air, seemed half rotten; but the other, besides its sinew, appear'd wholly putrefied.

Hence 'tis confirm'd, that the quantity of the mouldiness depends on the quantity of the air.

It seems also worth observing, that the mouldiness appear'd a little later in the compressed air, than in the common, tho', afterwards, it increased much more.

Roses in common and compressed air.

(47.) June 29. 1677. I included roses in two receivers, stopp'd by the help of screws; I left one with common air only, but filled the other with so much, that the mercury ascended ninety inches above its usual height.

July 14. Four or five days ago, I found the roses, in the compress'd air, wither'd, and degenerated to a yellow colour. There was not the least alteration in the other receiver.

July 17. When I perceived, that this experiment proceeded after the same manner as that above-mention'd, I took out the roses. Those kept in the compressed air, were very much corrupted, and of a very ungrateful smell; but the others were little alter'd, and their scent not unpleasant.

Hence we have a further confirmation, that the quantity of corruption depends on the quantity of the air.

Lemons in common and compressed air.

(48.) July 4. I cut a lemon asunder, and put the halves into two receivers, to be stopp'd by screws; the one I left with common air only, but the other was fill'd with so much compressed air, that it sustain'd ninety inches of mercury above its usual standard.

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July 7. This day, both parts of the lemmon seem'd to grow mouldy at the same time. PNEUMATICA

July 17. That in the compressed air had contracted much more hoar than the other; and, perceiving no farther alteration, I took them out, and found the lemmon, in the compressed air, far more putrid than the other.

Hereby it is confirm'd, that the quantity of corruption depends on the quantity of the air.

It seems also, that a triple compression of the air, in respect of a lemmon, is too weak, sensibly to retard the production of finew.

(49.) July 18. 1677. I included two parcels of July-flowers, equal in number, in two equal receivers, and stopp'd them close with screws; I fill'd the one with compressed air, till it sustain'd an hundred inches of mercury, extraordinary; but the other was left with common air alone. *July-flowers in common and compressed air.*

July 23. In the compressed air, the July-flowers shew'd some hoariness; the others appear'd only moist; but the mercury exceeded its wonted height only seventy inches; for some of the air had got out.

July 25. In the compressed air, the July-flowers proceeded to corrupt much faster than the others. They had wholly lost their colour.

July 26. In the compressed air, the July-flowers were wholly putrefied, and cover'd with a hoary finew; the others were moist only in some places.

August 1. Perceiving no farther alteration, I took the flowers out of their receivers; those which were kept in compressed air, were rotten, and fetid; the other kept their colour, and their smell was not offensive; but they were moist.

And this is a farther confirmation, that the quantity of the air increases corruption.

We may, also, observe, that the mouldiness is not produced, but in compressed air; nor is it probable, that this happen'd by chance; since, in each receiver, there were three or four July-flowers included.

(50.) July 21. 1677. I included a shrew-mouse in a receiver, with common air, and shut him in firmly with a screw, to try whether he would produce, or consume air. *A shrew-mouse confined in common air.*

After two hours, the mouse died, and some air was consumed; but a less quantity than in the former experiment of this kind.

July 24. Hitherto I found no change in the height of the mercury. Towards evening, it seem'd a little higher.

July 25. This morning much air was produced *de novo*.

July 26. The quantity of the produced air increased.

Hence we have a confirmation, that living animals consume air; but dead ones produce new.

(51.) August 31. I put pears into a receiver; whereto, after it was well stopp'd I added as much air, as sufficed to sustain thirty inches of mercury, extraordinary. *Pears in compressed air.*

September 1. The mercury was depressed.

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Sept. 2. The height of the mercury decreased; it exceeded not twenty-five inches.

Sept. 3. The mercury rose one inch higher; and staid at 26.

Sept. 4. The height thereof was 28.

Sept. 8. The receiver leaking, I put in new air; and, this day, opening the receiver, to compare the taste of this fruit with that of the other; I found, that five of the pears had lost their firmness, but two retain'd it.

Pears in common air.

August 31. I included pears, of the same kind, in another receiver, with common air only, not compress'd.

September 1. The mercury was a little depress'd, as if it had been in compress'd air; the cause whereof might be only the cold.

Sept. 2. The mercury varied not.

Sept 3. The height of the mercury was one inch, above the usual standard.

Sept. 4 } The height
5 } of it was

{ 4
6 $\frac{1}{4}$

Sept. 6 }
7 }

The height
of it was

{ 6 $\frac{1}{4}$
12

Sept. 8. The height of the mercury was twenty. The pears, being taken out of the receiver, had preserv'd their taste much better than those included *in vacuo*. They, also, retain'd their firmness.

Pears in vacuo.

August 31. I included pears of the same sort *in vacuo*; but some external air brake in, and the height of the mercury was one inch.

Sept. 1 }
2 }
3 }
4 }

The height
of it was

{ 4
8
12
16

Sept. 5 }
6 }
7 }
8 }

The height
of it was

{ 19
23
27
30

The pears, being taken out, had kept their firmness, but lost much of their taste.

Hence it seems to follow, that, in a greater compressure, a less quantity of air is produced.

A small bird in compressed air.

(52.) *December 7.* I shut up a small bird in a receiver, capable of holding twenty ounces of water. The bird began to be ill, before I had set the screw; but, after I had intruded so much air, as to sustain thirty inches of mercury, above its wonted height, she seem'd to recover; but, soon after, began again to be sick: and, therefore, I intruded air the second time, till the mercury rested forty-five inches above its usual height; whereby the bird was again restored: but, in a little time, she began to gasp again; then, opening the receiver, after she had staid in it twenty-eight minutes, she flew out, and was very well.

A shrew-mouse in compressed air.

(53.) *January 20. 1678.* I put a shrew-mouse into the receiver of the wind-gun, above-described; and, immediately, so far condens'd the air, till it was reduced to about the twentieth part of its space: then I presently discharged that air, and the elliptic hole being open'd, I suspected that the mouse had been only a little convulsed; but, when he was taken out, there were no signs of life in him. Whether the cause of his death were to be ascribed to the narrowness of the receiver, or to the compressure of the air, is a question.

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I put another mouse into the same receiver; and the air, being reduced to a third or fourth part of its natural space, I open'd the receiver, but not so carefully as in the former experiment; yet the mouse, taken out therefrom, was found to be very well.

I, afterwards, repeated the experiment; the air being about seven or eight times condens'd; and the mouse seem'd to suffer no inconvenience thereby.

I made the same experiment again, in air compressed seven times, and left the mouse included for twenty-four minutes; which time being elapsed, I discharged the air, and, opening the hole, perceiv'd the mouse to fetch many deep groans, as it were: I took him out, but he could not recover.

Hence it is manifest, that a great compression of air is pernicious and destructive to animals.

(54.) *January 28. 1678.* I put a shrew-mouse into a glass, to whose neck we tied a bladder, that stopp'd the orifice. These were put into a receiver for compressing of the air. Soon after, when the mouse began to be sick, I compressed the air, and the bladder was straitned; so that the mouse was in condensed air, whilst no new air could pass to him: then he seem'd to be much better, and his heart beat less frequent; when, opening the receiver, he was, in a short time, as well as ever.

I repeated the experiment, and left the mouse so long, that he could hardly breathe, whilst I began to compress the air; and the compressure seem'd again to abate his respiration: the receiver being open'd, and the mouse exposed to the air, he could not breathe much more freely; but, if I blew the air on him with bellows, he seem'd to be something reliev'd. Being, again, committed to the compress'd air, he breathed less frequently, and, at last, died.

March 25. Because, in the preceding experiment, it was not clear, whether the air enter'd thro' the ligature of the bladder, I used the instrument described, *Fig. 79.* And when I perceiv'd the mouse was sick, and breathed seldom, I intruded water into the receiver, so that the air was reduced to half of its space; and, then, the mouse breath'd more rarely. But if, extracting the water, I left the whole space entire for the air, his respiration seem'd more vivid; and the air being thus, many times, contracted and dilated, the sick mouse seem'd to respire more freely in the common air than in the compressed. Whence, I conjectur'd, that air is to animals, like food; the quantity whereof ought to bear some proportion to their strength. And, that I might more certainly know this, I put the same mouse into my pneumatic engine, and rarify'd the air, so that it possess'd more than double the usual space. Whilst the air was rarifying, the mouse presently began to be better; yet, a little while after, he seem'd sick: and, when the air was restor'd, it in no wise affected him. I, thus, repeated the rarification three times, with the same success; but, at last, the mouse died.

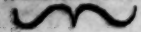
(55.) *May 5. 1677.* I put a bee, with distill'd vinegar, and pulveriz'd coral, into a receiver; and the air, being wholly exhausted, I order'd the

Gggg 2

matter

The effects of artificial air upon animals; and, first, upon a bee included with distilled vinegar, and powder'd coral.

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Flies in artificial air of goosberries.

matter so, that the coral fell into the glass of vinegar: but the air, produced from thence, did not restore any power of motion to the bee; but, when she was exposed to the open air, she soon began to move herself.

Hence a suspicion arises, that artificial air is unfit to preserve the life of animals.

(56.) August 12. 1676. I put two flies into a receiver, and, exhausting the common air, substituted, in its stead, air produced from goosberries, till it sustain'd twenty-six inches of mercury.

Afterwards, I put two other flies, also, *in vacuo*; but with this difference, that I let so much common air into them, as could sustain twenty-three inches of mercury.

Within a quarter of an hour, the latter flies, upon the restitution of the air, recover'd that power of motion, which they had lost *in vacuo*, and flew about in the rarified air; but the former lay without any motion, tho' they had receiv'd a greater quantity of air.

Aug. 13. The flies, in the artificial air, seem'd still dead; but the others were lusty.

The flies, taken out of the artificial air, and exposed to the common air, remain'd so, all this whole day, without recovering life.

Aug. 18. I repeated the experiment, with the same success; tho' I had restored a greater quantity of artificial air.

Hence we have an high confirmation, that artificial air is noxious to animals.

Flies included with fire in the artificial air of paste.

(57.) June 22. 1677. I put paste into three receivers, out of which, I, afterwards, exhausted the air.

June 23. When the three receivers did, this day, regurgitate with air produced from the paste, I kindled a perfumed cone, and put it into one of the receivers; which, being presently stopp'd, the fire, within one minute, went out: then, by blowing, I expell'd the artificial air from the receiver, and put fire into it, as before, and it burn'd bright for a considerable time; tho' I had shut the receiver as speedily, and as accurately, as before.

I made another experiment, after the same manner, with a fly; and, in the artificial air, she presently seem'd to be dead; but, afterwards, being exposed to the sun, she, in a short time, grew well again. Then, I blow'd common air into the receiver; but the fly, included as before, suffer'd no inconvenience thereby.

I repeated the same experiment, with the same fly, in the third receiver, fill'd with artificial air, with the same success; only this fly, when taken from the artificial air, could not be restored, without longer time, because she was left longer therein.

Hence it appears, that factitious air is prejudicial to fire, as well as to the life of animals.

Flies and frogs in artificial air.

(58.) June 25. 1677. I put paste into four receivers; and totally exhausting the air from two of them, I pump'd out only half the air from the other two.

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June 26. I found the two receivers, which I had left half full of common air, to be quite fill'd with air newly produced; but know not, whether they had, for some time, regurgitated or no, so that the quantity of common air was much diminish'd. However, I put two flies, at once, into one of the receivers, after the manner before described; and, soon after they touch'd the bottom of the receiver, they remain'd without motion. I put a third fly into the receiver, after the same manner, and found she liv'd a little longer there, than the former. A fourth fly, being put in, maintain'd her life longest of all; yet, at last, suffering some convulsion, she lay moveless on her back. All the flies, after some stay in the artificial air, being taken out, and expos'd to the common air, grew well in a short time.

I made the same experiments in another receiver, half full of artificial air, and, in a manner, with the same success; but the flies in that receiver, into which only common air was admitted, soon recover'd their strength and motion.

June 27. I found one of the receivers, which was wholly evacuated of common air, to be full of artificial air; but, it being casually thrown down upon the ground, entrance was, thereby, given to the external air; yet I put a frog into it, which seem'd not to be very sick therein.

June 30. The fourth receiver, by the power of the produced air, seem'd, at length, forc'd away from its cover. I put a frog into it, and she fell into high convulsions, for five minutes space; and then lay without motion. In four minutes after, I open'd the receiver, and, taking out the frog, she remain'd, for forty-six minutes, without motion; but, afterwards, in four or five minutes more, she grew very well.

Hence it is evident, that artificial air is very hurtful to the life of animals; but that, if mixed with common air, it doth not so readily produce its effects.

(59.) *June 28. 1677.* I put paste into four receivers, three of which I caus'd to be wholly exhausted of common air; but the fourth was left half full of air.

June 29. One of the receivers, that were wholly exhausted, was found full of air, newly produced; and a frog, being put into it for four or five minutes, had strong convulsions: then, for one minute, she lay without motion; whereupon, I took her out, and, in five minutes, she began to move, and, a while after, became well again.

I took another receiver, fill'd with artificial air, and, putting a frog into it, in seven minutes she ceas'd to be convulsive. And, afterward, when she had lain one minute there, without motion, I open'd the receiver, and, taking her out, found that she began to struggle, and move; tho' I judg'd those motions to be the remains of her convulsions: for, after that, she continu'd moveless for half an hour, and more; yet, at last, she grew well again.

The receiver, from which I exhausted only half its air, had so long regurgitated with produced air, that, very probably, much common air had

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had got out together with it. A frog, being put into it, seem'd to be vehemently mov'd, and convulsed, for ten minutes, as the rest did, and, then, she seem'd quite dead; but, after a full minute, I open'd the receiver, when, the frog, being expos'd to the open air, within a quarter of an hour, began to move.

I put a frog into a receiver, full of common air, to try whether, the paste being now taken out, she would live any longer there.

July 1. In the afternoon, I found her dead, tho' she breath'd in the morning; so that she liv'd about forty-eight hours.

June 30. I put a frog into the fourth receiver, which was wholly fill'd with artificial air; for seven minutes and an half, she was vehemently convulsive, and, at last, died; then, after two minutes, she was taken out, but recover'd no motion at all.

July 1. Perceiving the frog to remain in the same posture, I threw her away.

Hence we have a confirmation, that artificial air is the more hurtful to animals, the freer it is from common air.

*A shrew-mouse,
 snail, and flies,
 in artificial air
 of paste.*

(60.) June 30. I included paste in two receivers, and then exhausted the air.

July 4. I put a shrew-mouse into one of the receivers, filled with artificial air, where he suffered vehement convulsions, and in one minute, died. I presently took him out, and expos'd him to the common air; but no power of motion could be thereby recover'd.

Then I took the other receiver, and, putting a snail into it, with some wonder observed, that he continued to move very strongly, for a whole quarter of an hour; but, afterwards, his motion was slower, till in about another quarter of an hour, he lay still, as if he were dead; but then, being taken out of the receiver, and expos'd to the air, he soon grew well.

I put flies into the same receiver; but now it had admitted too great a quantity of external air, for the flies received no hurt.

Hence we gather, that artificial air kills animals by some venomous quality, and not only by the defect of common air; for the snails liv'd longer *in vacuo*.

*A frog in air
 produced from
 cherries.*

(61.) July 5. 1677. I took a receiver filled with air produced from cherries, and transmitted it out of that, into another receiver, full of common air, in which a frog was kept: matters were so order'd, that the water gave place only to the artificial air entring in; and the water itself flowed out. And thus the frog, being included in pure artificial air, for a quarter of an hour, and more, suffer'd convulsions, and, at last, lay still without motion; yet, being afterwards taken out, and expos'd to the open air, she grew quickly well.

Hence it seems probable, that air produced from cherries, is less hurtful to frogs, than that produced from paste.

*Flies, and a
 shrew-mouse, in
 artificial air of
 goosberries.*

(62.) July 9. 1677. I put goosberries into three exhausted recei-

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July 20. I found one of them sever'd from its cover, by the force of the produced air: I cast a fly into it, which died instantly; a second fly being, likewise, cast into the receiver, presently died; a third put into the same, seemed, for a little while, to be convulsive; but less than a fourth fly, that I included therein; which yet, in one quarter of a minute, lay moveless. Afterwards, I dispelled the artificial air out of the receiver, by blowing, and, in a little time, the flies grew well.

July 24. I took another receiver, filled with air produced from goof-berries; and putting a shrew-mouse into it, found that he died there in half a minute.

Probably, therefore, the air produced from fruit, is less hurtful to animals, than that produced from minerals: for, on the 20th day of July, I found that a mouse liv'd not above a quarter of a minute in air produced from gun-powder.

(63.) July 5. 1677. I included paste in four exhausted receivers.

July 6. One of them, being filled with factitious air, was forced from its cover, which I again stopped; yet not so soon, but some common air might mix with the artificial: I put a shrew-mouse into it, which was presently highly convulsed, and, after one minute and an half, remained moveless; and, being presently taken out, he seemed to have some convulsive motions, and died.

A shrew-mouse, a bird, and an adder, in artificial air of paste.

July 7. I took a second receiver, filled with artificial air; and having included a little bird therein, suddenly stopped it: she presently fell into convulsions, and, within about a quarter of a minute, died: I took her out, but 'twas too late, for she never stirred after.

I blew out the artificial air from the receiver, and then, another bird of the same kind, being put in, was very well, though she staid there four minutes.

July 9. I took a third receiver full of artificial air, and put that bird into it, which, in the last experiment, had continued well, and yet seemed to be lively and sound; before she had been there a full quarter of a minute, she lay without motion, and being presently taken out, there appeared no sign of life in her.

In the afternoon I put an adder into my fourth receiver, and, within two minutes, he began to be sick, to gape and pant; yet he was not wholly deprived of motion, till after twenty-four minutes. Then, in six minutes more, I took him out of the receiver, motionless as he was, and exposed him to the free air, but he did not recover.

July 10. The adder remained in the same state, and gave no signs of recovery.

(64.) July 12. 1678. I put a bird into a receiver full of air produced from raisins of the sun; she died in a quarter of a minute; and tho' I took her out presently, yet she never stirred more.

A bird and a shrew-mouse, in artificial air of raisins.

July 18. I, likewise, put a shrew-mouse into a receiver full of air, produced from raisins of the sun; but a thread, left on the edge of the receiver, hinder'd me from stopping it close; yet the mouse presently began

Barometrical

Shrew-mice included in common air.

gan to be very ill, and, after two minutes, he lay, as it were, without any motion; yet, being taken out, he was well again in two or three minutes time.

(65.) *October 1. 1678.* About ten in the morning, I included a shrew-mouse with common air, in a receiver, fortified against the external air; about eleven, the mouse could hardly breathe: I threw another strong lusty mouse into the same receiver, and presently put on the cover; but the first mouse, having consumed some of the air, the external air was forcibly impelled into the receiver, and so dispelled a great part of the air stagnant there: upon which, the first mouse seemed to be much better; neither did he die much sooner than the other, but both of them died about noon. About four in the afternoon, I put another strong mouse into the same receiver; and, lest the external air might again expel the included air, I put him in very leisurely: this third mouse lived not three minutes entire.

Whence we may conjecture, that the portion of air which hath once served for the respiration of animals, as much as it could, is no longer useful for the respiration of another animal, at least, of the same kind.

Snails in factitious air of paste.

(66.) *April 28.* In the morning, I put so great a quantity of paste into an exhausted receiver, that, in the afternoon, I found the receiver full of factitious air; whereupon I put a snail into it, which presently frothed very much, and often expanded and contracted itself; but, in four minutes, he ceas'd to move: yet I took him not out, till he had staid in the receiver for a quarter of an hour; and then, being releas'd, he seem'd as if quite dead: for, tho' he were prick'd with a pin, yet he discover'd no sign of life; tho', after another quarter of an hour, being prick'd, in the same manner, he mov'd a little.

I blew out the factitious air, and put in another snail: he remain'd very well in the receiver, and did not froth at all.

Hence we have a confirmation, that factitious air is a greater enemy to animals, than a Vacuum.

Snails in the factitious air of Pease.

(67.) *June 22. 1678.* In the morning, I put green pease into an exhausted receiver; and, towards evening, the mercury had almost attain'd the height of ten inches.

June 23. The height of the mercury was almost thirty inches.

June 24. The mercury did not yet exceed thirty inches in height. The cover no longer stuck to the receiver; yet nothing, hitherto, had escaped.

June 26. I included the same pease in the same evacuated receiver.

July 29. When I now found that the receiver was fill'd with factitious air, I thrust a snail into it, which froth'd much, and very often shot out and contracted his horns; but, in six minutes time, he lay still, as if he had been dead; and continued thus for two or three minutes; then the receiver being open'd, and the snail taken out, mov'd himself a little, if he were prick'd. Whence it seems to follow, that air produced from pease is less prejudicial to snails, than air from paste. I blew new air into the receiver, and a snail then put into it, did very well.

In

In this experiment it seems observable, that pease quickly produce air *in vacuo*; but, that in the usual compressure of air, they generate little. PNEUMATICS.

(68.) June 22. 1676. I put a butter-fly into an exhausted receiver, and it was almost three hours before he was wholly deprived of motion; at length, perceiving him to lie unmoved, I let the air into the receiver, and, presently, the butter-fly moved. Then I bound him, by one of his horns, with a thread, and suspended him in the receiver; when, he was carried very freely from one part of it to the other, by the motion of his wings; but, after the air was extracted, the clapping of his wings was in vain, for he could not, in the least, move the thread from its perpendicular posture. Animals in vacuo, and first a butter-fly.

(69.) July 12. 1676. Yesterday I put two flies into a receiver, in which I left $\frac{1}{4}$ of air, (*i. e.*) as much as would sustain ten inches of mercury; the biggest of the flies seemed to die presently; but the other, which was a small-bodied one, lived almost twenty-four hours. Flies in a receiver partially evacuated.

When both the flies lay, as if they were dead, I suffer'd some air to enter in; the mercury was fifteen inches high, when the lesser fly began to move her feet, but the other continued still without motion.

Hence it appears, that air, highly rarified, may serve for insects to breathe in; and that it doth not kill them so soon as artificial air.

(70.) May 1. I put two snails into an exhausted receiver, and, for a whole hour, they seemed to be well enough, and crept up to the top of the vessel; but, in two hours time, they fell down from thence, and lay without motion. Snails in vacuo.

After they had remained *in vacuo* for six hours, I took them out, and, within half an hour, they began to move a little. During the time they were included, they produced near as much air as sustain'd the mercury at the height of a quarter of an inch.

These snails liv'd longer *in vacuo*, than did others included in artificial air.

(71.) August 12. 1676. I put the eggs of flies into an exhausted receiver, to try if they would there produce worms. Flies eggs in vacuo.

Aug. 14. Worms were formed, but the air had crept into the receiver, so as to sustain fifteen inches of mercury.

Hence it appears, that insects may be produced, and live, if not *in vacuo*, yet, at least, in air very highly rarified.

August 16. 1677. I put flies eggs into an exhausted receiver.

No worms being produced, I admitted the air into the receiver, and left all things in the same posture, to try whether the eggs had lost their faculty of producing worms.

Sept. 9. The eggs produced nothing.

This experiment seems to shew, that insects may be generated, and live in air highly rarified; but not at all *in vacuo*.

(72.) March 17. 1677. I put two equal quantities of frog-spawn, into two glass vessels of equal bigness; I left the one in an exhausted receiver, exposed to the sun; but the other, being in a receiver full of common air, Frog-spawn included in vacuo, and in common air.

PNEUMATICS. I fortified against the access of the external air. The frog-spawn *in vacuo*, all swell'd into bubbles.

May 2. No frogs were produced in either receiver; and the spawn, kept *in vacuo*, remain'd still full of bubbles: but, about three days ago, all the bubbles vanish'd, and the spawn was changed to a green liquor.

July 2. The receivers remained in a window, exposed to the noon-day sun; so that some water, mix'd with the frog-spawn *in vacuo*, and the very spawn itself was elevated into vapours; and afterwards, sticking to the sides of the receiver, out of its own vessel, was there condensed: but the vessel, kept in the common air, still contained all its water, together with the spawn.

A frog in vacuo. (73.) June 15. I shut a frog in an exhausted receiver, at about seven of the clock in the evening, and about nine the frog died.

June 16. I repeated the experiment, and again perceived, that the dead frog, in two hours space, had produced some air, rather than consumed it.

June 18. The frog, hitherto left *in vacuo*, was swollen very much; but the air, now entering, made her far more flaccid and lank than before.

Hence it appears, that a receiver, void of artificial air, is less hurtful to such kind of animals.

Fly-blowings in vacuo. (74.) August 3. 1678. I put fly-blowings, upon flesh, into an exhausted receiver.

Aug. 12. No worms were generated.

Aug. 15. Perceiving no change in the eggs, I open'd the receiver, to try whether they would generate in the free air.

Sept. 15. Nothing was produced from them.

Hence we see, that animals, which may be generated, and live in highly rarified air, are killed *in vacuo*.

Vinegar-eels in vacuo. (75.) Aug. 22. 1678. I included vinegar full of small eels, or vinegar worms, in an exhausted receiver.

Aug. 29. The worms still moved, but were fewer than at first.

September 6. Yesterday some of the worms still moved, but now I could not see one; and using a microscope, I found them all dead: but, in the vinegar, which I had left in the open air, the eels had as brisk a motion, as at the beginning.

Hence it appears, that very minute animals are also affected by the presence and absence of the air.

Fire in compressed air, and first perfumed cones, included, and fired, in condensed and common air. (76.) May 14. I took a perfumed cone, which being once kindled in the free air, will, by degrees, wholly consume; and put it into a receiver firmly stopp'd with a screw: then I intruded air into it, till the mercury rose one hundred and twenty inches above its wonted height; when, by a burning-glass, I kindled the cone, which presently darkned all its receiver with smoke, and, after some time, seven eighths of an inch thereof, in length, were reduced to ashes; but taking out the cone, and blowing away the ashes, I found only the superficies thereof consumed; the inner parts remaining entire.

I included another cone of the same sort in a much greater receiver, but did not compress the air therein: the cone, fired by the same burning-glass, was not taken out, till all the fumes abated and fell down; yet, much less of this cone was burnt, than of the former.

(77.) *May 19.* I weigh'd a perfum'd cone exactly, and then firmly included it in a receiver with common air, and kindled it by means of a burning-glass; when the fumes were condensed, I took the cone out of the receiver, and found, it had lost of its weight, almost one grain.

Afterwards, the same cone, observing the same circumstances, was again included and kindled; but first I had intruded as much air into its receiver as sustain'd ninety inches of mercury; and, by means of a pair of scales, found the loss of weight, now, to be four times more than before.

Hence it seems to follow, that the consumption of matter by fire is greater in proportion to the quantity of air contain'd in the receiver.

(78.) *May 17. 1677.* I included a perfum'd cone in a receiver firmly stopp'd by the help of a screw; and the air compressed to sustain sixty inches of mercury above its usual height, I fir'd it with a burning-glass; the cone being afterwards taken out, had lost three grains and an half in weight.

I repeated the same experiment in air, so compressed, that the mercury reached one hundred and twenty inches higher than usual; then the cone was seven grains and three quarters lighter; and so, tho' the quantity of the air was not double, yet the consumption of the matter by fire, was more than twice as much as in the former experiment.

May 17. I made the same experiment in air, compressed to sustain ninety-seven inches of mercury; and, then, the loss of weight seem'd to be six grains.

Hereby we are taught, that the matter is the more consumed by fire, as the compressure of the air in the receiver is the greater; or rather, that the consumption is made in a proportion greater than that of the compressure.

May 18. 1677. I intruded a perfum'd cone, as before, in a receiver seven times larger than that used in the former experiments; and crowded no air at all into it. The cone, kindled there, lost three grains and a quarter of its weight; whereas, in the same quantity of air, if reduced to a fifth part of its space, it would have lost ten grains.

Hence it seems to follow, that the same quantity of air, reduced to less than its accusom'd space, causeth a greater consumption, than in its natural state.

(79.) *May 19. 1677.* I repeated the last experiment, in the same receiver, closely stopp'd with a screw, that nothing might get out or in. The cone lost but one grain and a quarter of its weight; whence I suspect, that it was not well kindled.

May 21. I made the same experiment, in the same manner; and this day the cone was lighter by four grains; whence I more certainly collected, that it was not well set on fire in the preceding experiment.

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May 23. I repeated the same experiment twice, but suspect, that the cone was not well kindled; since, at one time, it lost, only three quarters, and at another, one grain of its weight.

May 24. I made the same experiment again; and this day also the loss of weight was found only one grain and a quarter. Then I open'd the receiver, and having wiped and cleans'd away the soot, repeated the experiment; when the cone took fire very well; for the loss of its weight amounted to six grains and an half.

I tried the same experiment again, in an unclean's'd receiver, and the cone lost only three grains.

May 25. I made the same experiment in a receiver well wash'd, and the cone was lighter by six grains and an half.

I made the same experiment, in the like manner, in a well cleans'd receiver, and the cone lost seven grains and an half.

I made the same experiment again, in an unwash'd receiver, and then I could not sufficiently kindle the cone.

May 26. I made the same experiment in an unwash'd receiver, about the middle of the day; the sun being clear, and bright; and remov'd not the burning-glass for a long time, so that it took fire very well, and became eight grains lighter.

Hence it is manifest, that the quantity of a cone to be consumed in the same quantity of air is not fix'd and certain, but sometimes greater, sometimes less, as the cone shall be more or less kindled. Besides, the imperfect mixture of the matter may cause some difference; yet it seems certain, that fire is more easily kindled in compressed air, than in common; and the consumption will be the greater in a certain quantity of air, if that air be reduc'd into a narrower space, than if it possess'd no more than usual.

(80.) *May 22.* I put a perfumed cone into a receiver made for compressing the air; and intruding the air till the mercury rested thirty inches above its usual height, I kindled the cone, and found its weight to be abated one grain and three quaters.

May 23. I made the same experiment again, after the same manner, and with the same success.

I repeated the same experiment, but the cone did not kindle well. Whence we have a confirmation, that fire is more easily kindled in air much compressed, than in common air, or that which is but a little condensed.

I repeated the same experiment, and after I had remov'd the burning-glass, whilst I was intent to see, whether the cone would proceed to be consumed, the receiver brake into an hundred pieces, some of which, struck my head and wounded it: which I mention, that no man may be confident his glass will not break, whilst he is about such experiments, because he has found, that at other times it resisted a greater pressure. For this very glass, had contain'd air four times more compress'd. It had also resisted air, sustaining one hundred and ninety-eight inches of mercury above its wonted height; yet, now it was broken by a pressure, more than six

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fix times less; and, therefore, while a man looks into such receivers, his ^{PNEUMATICS.} head should be guarded.

(81.) *June 4. 1676.* I burnt paper, besmeared with sulphur *in vacuo*, ^{Fire made use of to produce air, and first paper, besprinkled with sulphur burnt in vacuo.} and found, that it prodced some air; which was not at all diminish'd for two days.

This air must be ascribed to the paper, for none is produc'd out of sulphur alone.

(82.) *June 15.* I burnt harts-horn *in vacuo*, and found, that the fumes, ^{Harts-horn burnt in vacuo.} issuing therefrom, contain'd some air.

June 17. On these two last days, I repeated the same experiment, and always observ'd, that air produced from harts-horn, was soon, in part, destroy'd; but that, which preserved its elasticity for an hour after the burning-glass was remov'd, seem'd, afterwards, not to lose it at all.

June 19. I took the harts-horn out of the receiver, and found no volatile salt, but only a fetid oil to be produc'd therefrom.

(83.) *June 21.* I burnt amber *in vacuo*, and, at first, could not find that the fumes ascended above the height of one inch; yet, in a receiver full of air, they would be carried up to the top, and from thence return downwards; yet, afterwards, even in the *vacuum* itself, the fumes reached almost to the top of the receiver, but the mercury varied not at all in its gage. ^{Amber burnt in vacuo.}

June 22. This night, a great deal of that water, in which I had immersed the receiver, found a passage into it, tho' the cover was so well fitted to the aperture, that I never perceiv'd any water to get in betwixt them, before. Hence a suspicion arose, that some volatile salt had, probably attracted the aqueous parts, by reason of the congruity betwixt them.

July 8. I still kept the receiver immersed in water, but no more water entered in; as if, the salts being washed away, the external water, destitute of assistance, could no longer insinuate.

(84.) *Jan. 18. 1677.* I put two drams of camphire into an exhausted receiver; and the juncture of the cover, with the receiver, being fortified against external air, I put the camphire on a digesting furnace. ^{Camphire sublimed in vacuo.}

Jan. 19. The camphire sublimed into flowers, but no air was produced.

(85.) *May 24. 1676.* I included *Sulphur vivum* in an exhausted receiver, ^{Sulphur vivum fused in vacuo.} and melted it by the help of a burning-glass; but found, that the fumes, produced therefrom, contain'd no air, because the mercury ascended to the aperture of its gage, as is usual, while the receiver is evacuating; yet, when that was cool'd, the mercury return'd to its former height: and, therefore, that change was, probably, owing to the air included in the seal'd leg of the gage, being rarified, and driving the mercury into the other part.

(86.) *July 19.* Having included paste, nine days ago, *in vacuo*, and perceiving that it now contain'd no more air, I endeavour'd to fire it with a burning-glass. The subsiding fumes had tinged the superficies of the paste, of a curious yellow; and, I conjectur'd, that some air was produced, because the receiver, which, before, was closely join'd to its cover, might now, with ease, be pluck'd therefrom. ^{Paste expos'd to the rays of a burning-glass in vacuo.}

(87.)

PNEUMATICS.

The production
of air from
grapes in vacuo.

(87.) September 9. 1676. I exhausted the air out of a receiver, half full of dried grapes, and fortified it against the external air.

Sept. 10. In twenty-four hours time, the height of the mercury was $\frac{1}{2}$.

Sept. 12. In two days time, the ascent of it was $\frac{1}{2}$.

14.

17.

22.

The ascent of the mercury was $\left\{ \begin{array}{l} \frac{3}{8} \\ \frac{3}{8} \\ \frac{5}{8} \end{array} \right.$

27. The ascent was $\frac{1}{2}$. The height three inches.

From figs.

October 11. The height of the mercury was now about six inches.

September 9. 1676. I put dried figs into a receiver, and fill'd about half of it with them; then I extracted the air, till the mercury rested at the height of three inches.

Sept. 10. No air was produced.

Sept. 17. Perceiving no air to issue out of the figs, I open'd the receiver.

Hence we learn, that dried fruits, put into an exhausted receiver, produce very little air with regularity.

From pears and
apricocks.

(88.) August 5. 1676. I included pears and apricocks in vacuo.

Aug. 6. In eighteen hours time, the mercury rose two inches; in ten hours more, it reach'd to three.

Aug. 7. } The height of it was $\left\{ \begin{array}{l} 5 \\ 6 \frac{1}{2} \end{array} \right.$

9. In fourteen hours space, the mercury mounted three quarters. Its height was seven and a quarter.

Aug. 10	} The height of it was	8 $\frac{1}{2}$	Aug. 18	} The height of it was	25
11		10 $\frac{3}{4}$	19		29
12		12 $\frac{1}{4}$	20		31 $\frac{1}{2}$
13		14 $\frac{1}{4}$	21		32 $\frac{1}{2}$
14		16	22		34
15		18	23		35
16		20	26		38 $\frac{1}{2}$

Aug. 29. The height of the mercury was forty-one.

Sept. 1. The height of it was forty-two and a half.

4. The height of it was forty-four.

7. The three last days, being hotter than the foregoing, the ascent of the mercury was two and a quarter; its height, forty-six and a quarter.

Sept. 10. The height of the mercury was forty-seven and a half.

13. The mercury was depressed; its height only forty-four inches.

23. The mercury, by degrees, mounted again to forty-eight inches.

27. The height of the mercury was fifty and a half.

Nov. 5. The mercury ascended, gradually, to fifty-two and a half.

Nov. 28. The apricocks were reduced to water; the skin being sever'd from the pulp, yet no more air produced.

Jan. 10. 1677. Whilst it froze very hard, the mercury rose to fifty-seven inches; but, when it thaw'd, it sunk to twenty-three. Whether the strength of the frost open'd some way for the air to get out, I know not.

(88)

March

March 3. The mercury could ascend no higher, because the air was got out. This day I found the receiver tumbled on the ground; and the apri-cocks, when the frost broke, were putrefied, and had lost their colour. PNEUMATICS.

Hence it seems to follow, that apri-cocks produce air almost as easily in their wonted pressure, as *in vacuo*.

(89.) June 22. 1676. I put four cherries into two exhausted receivers, and, proceeded with both alike, except that in the one, the cherries were whole, in the other, cut asunder; in two hours, the whole cherries had impell'd the mercury into the gage, to the height of ten lines, and the cut ones to about twenty. From cherries.

June 21. In twenty-four hours, the mercury, in the receiver containing the whole cherries, rose to the height of three inches; but, in the other, the gage was spoil'd.

June 26. The whole cherries had not yet produced so much air, as to sustain fifteen inches of mercury; but the cut cherries had wholly fill'd their receiver with air.

July 9. The receiver of the whole cherries was removed from its cover; I eat one of them, which tasted pleasant enough. I included the rest again *in vacuo*; many of them were broke, and, in one hour's time, they impell'd the mercury to the height of about two inches.

July 10. During these last twenty-four hours, the mercury ascended not: whether the gage was damaged, I am not certain.

July 15. I found the cover sever'd from the receiver; whence it was clear, that the gage was hurt.

Hence it appears, that some cut fruit, sooner produce their air, than what is whole.

(90.) June 9. 1676. I put some cherries, that were not acid, into an exhausted receiver; and, within an hour, found as much air produced from them, as sustain'd a quarter of an inch of mercury.

June 10. In eighteen hours, the mercury rose to eleven inches.

June 11. The fruits produced less air, gradually; so that, this day, towards the evening, the mercury came not up to fifteen inches.

June 12. The mercury was a little higher than fifteen inches.

13. The height of the mercury was twenty-two inches.

16. The mercury, yet, came not up to thirty.

18. Perceiving no more air to be produced, I open'd the receiver.

Such a small production of air seems very remarkable, because I had found fruit, of the same kind, in *France*, to fill their receivers in two days time. Probably, fruits of the same kind, in several countries, differ much amongst themselves.

(91.) June 12. 1676. I put cabbages, cut in pieces, into an exhausted receiver, with a mercurial gage; and, in an hour's time, the mercury ascended one line. From cabbages.

June 13. The mercury was now come almost to the height of ten inches.

17. It was come almost to the top of its gage; and, the receiver being open'd, I found the cabbages little alter'd.

June

PNEUMATICS.

June 19. Being left for two days in the open air, they were wholly corrupted, and blackish. I put them again *in vacuo*, to try whether the putrefaction begun, would promote, or retard the production of air.

June 19. The mercury, in half an hour, ran up half an inch.

22. For three whole days, the mercury got higher, only by ten lines. Its height was one, and a third of an inch.

June 23. Finding the cabbages produce no more air, I took them out of the receiver; their smell was very bad.

Hence a suspicion arose, that bodies, when they putrefy, have produced almost all their air.

From oranges.

(92.) May 29. 1676. I took pieces of orange, weighing four ounces, and put them into a receiver, capable of holding ten ounces of water; and exhausted the air.

June 10. The receiver was remov'd from its cover, by the force of the air produced; so that I took out the oranges, and presently put them into another exhausted receiver, capable of containing eight ounces of water; when, the mercury, within half an hour, was elevated to the height of half an inch.

June 13. This sudden ascent of the mercury was not durable; for it, yet came not to the height of two inches.

June 16. The mercury, during the last twenty-four hours, ascended about three lines.

June 21. The mercury, the last twenty-four hours, did not ascend the space of one line.

July 18. I perceiv'd no more alteration in the height of the mercury, but some mouldiness appear'd; tho', I am certain, that no air, from without, had enter'd the receiver.

From a tulip.

(93.) April 27. 1676. I put a tulip into an exhausted receiver, with a mercurial gage; but, before it was fortified against the external air, enough got in to sustain two inches of mercury.

May 2. The tulip which, at first, appear'd striped with various colours, was now wholly changed into a dark red, become moist, and produced very little air.

Half a lemon.

(94.) April 22. 1676. I put half of a lemon into an exhausted receiver, with a mercurial gage, so short, that the mercury could not ascend three inches.

April 24. In two days space, the mercury came to the height of an inch and a half.

April 25. The mercury was now two inches high.

April 27. Yesterday the mercury ascended four lines; but, this day, only one.

April 29. During the two last days, the mercury mounted higher by one line.

May 3. In four days space, the mercury ascended one line, and a little more.

May 3. 1677. The mercury came to the top of the gage, yet no air got out; but the lemon was a little alter'd.

Jan.

Physico-mechanical Experiments.

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PNEUMATICS.


Jan. 1. 1678. Hitherto no air escaped out of the receiver; but the lemon had contracted a yellow colour, and a moisture.

(95.) March 16. 1677. I put two apples, of the same sort, into two exhausted receivers; one of the apples having begun to putrefy, but the other was only bruised by a few blows.

May 15. 1677. Hitherto they seem'd in very good case; but now the apple, which was bruised, appear'd wholly rotten, and the receiver was forc'd from its cover: the other apple remain'd without change.

August 20. 1677. The apple, which before began to be rotten, suffer'd no farther alteration; but, finding that the receiver was now parted from its cover, and fearing lest the apple would be speedily putrefied, I took it out: its taste was grateful, inclining to acid, as if it had been fermented; but the pulp somewhat resembled meal in consistence.

Hereby it seems confirm'd, that fruits have produced the greatest part of their air, when putrefaction begins in them; since the putrid apple did not fill its receiver, but in a much longer time than the other.

(96.) May 17. 1676. I pour'd two equal quantities of milk into two glass-receivers, of equal bigness; the one I left in the free air, and the other I included in an exhausted vessel, with a mercurial gage.

May 18. The cream floated on the top of the milk, left in the free air; but that *in vacuo*, was only cover'd with bubbles, and the gage not alter'd at all.

May 19. The bubbles gradually swell'd; and the mercury, in the gage, was a little higher.

May 20. The bubbles, *in vacuo*, swell'd yet more, and that milk seem'd curdled; but the other, in the free air, was, manifestly, curdled. The mercury, *in vacuo*, came almost to the top of its gage.

May 22. The milk, *in vacuo*, proceeded to generate more air; and now it evidently appear'd to be curdled. Whence, it is manifest, that the coagulation of milk, when the air is taken away, is retarded. Almost all the bubbles were now broke.

June 20. The milk, *in vacuo*, was no longer cover'd with bubbles, and remain'd still coagulated in the same state. But the milk, in the free air, became very fetid, and was full of worms. When it was put on the engine, and the air extracted, it emitted many very large bubbles, for a long time; and the worms mov'd very vehemently, but not one of them died in four hours time.

May 19. 1677. Three or four months ago, some whey, *in vacuo*, was pour'd out of a vessel into a receiver, and it seem'd clear and limpid, like water; yet there was whey enough left in the vessel, to separate the butyrous from the caseous part, at a sufficient distance.

This day the milk, stagnant in the receiver, seem'd to have got out of it; so that it is clear, the air, in the receiver, had a greater force than the external air; for the cover, also, was separated from the receiver. Towards night, I took that milk out of the receiver, and found it to be acid, both in smell and taste, yet it was not ungrateful to the palate; but, after

PNEUMATIC. a short time, the whey, which hitherto had remained limpid between the caseous and butyrous part, began to disappear, and to be blended with the rest.

May 24. The butyrous part wholly vanish'd; tho', as yet, it had suffer'd no sensible mutation: but the milk began to smell ill.

June 1. Our milk had not, yet, contracted the worst of scents; neither had it produced any worms: but it grew dry by degrees, and, this night, the mice eat it up, as, perhaps, they had the butyrous part before.

Here we see, that the coagulation of milk, when air is extracted therefrom, is somewhat retarded; that the weight of the butter, of whey, and of cheese, is not the same in the air, as *in vacuo*; for, in the air, they are confusedly mixed; but, *in vacuo*, one swims on the top of the other; that the putrefaction of milk, when air is extracted, is hinder'd, or very much retarded; and, lastly, that milk, by continuing long *in vacuo*, is made unfit to generate worms, even in common air.

Urine.

(97.) *September 5. 1677.* I took the same receiver, and the same vessel, used before to preserve milk *in vacuo*, and included urine therein, as I had done milk before. The quantity of urine was about three ounces, and three drams, and the receiver capable of holding ten ounces of water.

Sept. 7. The mercury reach'd to the height of almost two inches.

Sept. 8. The mercury was somewhat higher than yesterday.

December 5. The mercury ascended not above three inches; and, for the whole month past, was not changed. The urine seem'd not to be at all alter'd.

Decemb. 6. I set other urine under a receiver, not defended against the external air.

Decemb. 16. The urine, *in vacuo*, still kept unalter'd; but the other, in ten days time, seem'd turbid, and to have contracted some mouldiness on its superficies.

This experiment, compar'd with the former, makes it probable, that urine contains less air than milk.

But the power of the air to corrupt urine, seems very observable.

Paste.

(98.) *May 19.* I took paste, very much diluted, and without leaven, and putting it into a glass-vessel, included it in an exhausted receiver: and tho' the vessel which contain'd it, were not half full, before all the air was exhausted; yet the paste had swollen above the brim of the vessel.

May 20. The paste continued to swell, and was interspers'd with many cavities.

May 22. The paste was much more tumid than before, and much air was generated therefrom.

May 23. In the morning I found the cover sever'd from the receiver, by the force of the produced air, and some of the paste spread above the edges of the receiver; yet its swelling was somewhat abated. In the afternoon, its swelling was much more abated, yet it took up twice more space than before it was put into the receiver. The taste of it was not acid; and, therefore, I think, that bread, thus made, is very light.

(99.)

Physico-mechanical Experiments.

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(99.) July 20. 1676. I put a quantity of beef into an exhausted receiver, defended against the external air; and another equal quantity into a receiver, neither exhausted, nor closely stopp'd. PNEUMATICS.
Beef.

July 21. In thirty hours time, the exhausted receiver was fill'd with air, so that I suspected some air had got in: and, therefore, included the same beef again, and so closed it; there was no fear any external air should enter.

July 22. In fourteen hours time, the mercury rose to the height of fifteen inches.

July 25. For three whole days and more, the beef did not produce air enough to fill one half of the receiver.

July 26. The receiver was sever'd from the cover; and in one hour's time, I perceiv'd the beef, being again included *in vacuo*, had produced air enough to sustain ten inches of mercury.

July 28. I found the receiver again fill'd with air, and re-exhausting it, much air was in a short time again produc'd from the beef.

July 30. The receiver being again fill'd, I again included the beef *in vacuo*, and found, that the air produced from it in one hour, sustain'd ten inches of mercury.

August 1. The receiver being this day fill'd again, the beef stunk abominably, and we threw it away.

Hence it appears, that flesh, whilst it putrefies, produces much more air, than before it putrefies; but 'tis otherwise in fruit.

(100.) July 18. 1676. I put some goosberries, which I had long kept in receivers, to produce air, into one that was exhausted. Goosberries.

Within half an hour the mercury ascended to the height of one inch.

In an hour and a half, the mercury mounted another inch.

July 19. In twenty-four hours time, the receiver was almost all filled with air.

July 20. The cover was forced from the receiver, and much juice run out.

July 29. I left the same goosberries in a receiver, not defended against the external air; but this day I included them again *in vacuo*, to try, whether they could produce any more air.

July 30. In sixteen hours time, the goosberries drove up the mercury an inch and a half into the gage.

July 30. 1677. The goosberries could not wholly fill the receiver; and they always remain'd in the same state; but a while since they had almost lost their red colour, and inclined to white.

From hence it seems to follow, that this fruit, after it has produced all its air, suffers very little alteration; as if that air itself were the cause of corruption.

(101.) August 23. I put pears into an exhausted receiver with a mercurial gage; and before the receiver could be well defended against the external air, the mercury was risen one inch and a half. Pears.

In two hours time it ascended four inches; its height being almost six.

Iiii 2

August

Physico-mechanical Experiments.

PNEUMATICS.

August 24. The height of the mercury was twelve inches.

August 25. The height thereof was sixteen.

Aug. 26 } The height { 18	Aug. 28 } The height { 23
27 } of it was { 21	31 } of it was { 30

Sept. 1 } The height { 32	Sept. 4 } The height { 44 $\frac{1}{2}$
2 } of it was { 35	5 } of it was { 45 $\frac{1}{2}$
3 } of it was { 38 $\frac{1}{2}$	6 } of it was { 50

Sept. 7. The height of it was the same, because some air had escaped; but I prevented that for the future.

Sept. 8 }
9 } The height of the mercury was { 53 $\frac{1}{2}$
10 } { 54 $\frac{1}{2}$
 { 58

Sept. 12. Yesterday the mercury remain'd at the same height; but now it seem'd to be depressed: whence I conjecture, that some air had got out. The height of it was fifty-three and a half.

Sept. 13. I transmitted the air into another receiver: the height of it was thirty-two and a half.

Sept. 16. I perceiv'd that the air had got out; and opening the receiver, found the pears very rotten.

These pears produced their air irregularly, sometimes quicker, sometimes slower.

Dried plumbs.

102.) Sept. 17. I put dried plumbs into an evacuated receiver.

Sept. 19. The mercury seem'd to have ascended a little.

Sept. 22. I perceiv'd not, that the height of the mercury was alter'd.

Novemb. 9. When the plumbs produc'd no more air, I open'd the receiver.

By this experiment, 'tis confirm'd, that dried fruit is very unfit to produce air.

And nut-kernels.

(103.) Sept. 28. I put fresh nut-kernels, cut to pieces, into an evacuated receiver, with a mercurial gage.

Sept. 29. The mercury ascended a little.

Sept. 30. The height of it was two inches.

Octob. 5. The mercury continu'd to ascend by degrees: the height of it exceeded six inches.

Octob. 15. The height thereof was ten inches.

Octob. 22. The height of it was fifteen.

Nov. 28. The mercury was come to twenty inches, or more; but now the receiver was thrown down and broken, and the nut-kernels scatter'd: they were preserv'd very well, both as to colour and taste.

Hence we may conjecture, that air, without sensible putrefaction, is producible from fruits, even of a hard consistence.

The production
of air above its
usual pressure, in
pease, raisins,
and water, in
common air.

(104.) June 22. I included new pease in a receiver, with a glass full of raisins of the sun bruised, and mixed with water; and did not exhaust the air.

Towards evening, the mercury had mounted to twelve inches; but a great part of that air was produced from the raisins, not from the pease.

June 23. The height of the mercury was forty-nine.

June

Physico-mechanical Experiments.

613

PNEUMATICS.

June 24 } The height { 75 | June 26 } The height { 90
25 } of it was { 90 | 28 } of it was { 100

The pease sweat, as it were, and grew yellow.

June 30. The height of the mercury was one hundred and ten.

July 1. The mercury ascended not, yet no air escaped.

July 4. The height of the mercury was one hundred and twenty-four.

July 7. The height of it was one hundred and forty.

July 10. The height remain'd the same, but the liquor, which distill'd from the pease, got out.

July 12. New liquor was produc'd from the pease; but the mercury continu'd at the same height.

July 13. The liquor got out of the receiver, and some air besides; whereupon I set the screw, and new liquor, being in a short time collected, fortify'd the cover within.

July 15. The receiver was broken in pieces; but the pease, being softer than ordinary, were easily stript of their husks, as if they had been par-boil'd; but they kept their ordinary taste.

(105.) Sept. 15. 1676. I put unripe plumbs into an exhausted receiver; ^{In plumbs in vacuo.} and before the receiver could be guarded against the external air, the mercury ascended an inch.

Sept. 16. In twenty-four hours time, the mercury ran up five inches, and its height was six.

Sept. 17. The height of the mercury was eight.

Sept. 18 } The height { 10 | Sept. 23 } The height { 18
19 } of it was { 12 | 24 } of it was { 19
20 } { 14 | 26 } { 23
22 } { 18 | 28 } { 26

Octob. 1. The height of the mercury was thirty.

Octob. 4. The height of it was thirty-one, the weather somewhat cold.

Octob. 5 } The height { 32 | Octob. 9 } The height was 33½
7 } of it was { 33 | 11 }

Octob. 15. For these two last days, the cold being abated, the mercury ascended swifter; its height was thirty-seven.

Octob. 17 } The height { 38 | Octob. 29 } The height { 45
19 } of it was { 39½ | Nov. 2 } The height { 46
22 } { 41 | 5 } of it was { 47
26 } { 43 | 20 } { 53

In this experiment, the air seems to be produc'd sometimes regularly, and at others irregularly.

(106.) July 6. 1676. I put goosberries into an exhausted receiver; but ^{In Goosberries in vacuo.} before we could prevent the entrance of the external air, it had impell'd the mercury half an inch; and, afterwards, in half an hour, the air, produced from the goosberries, impell'd it another half inch.

In seven hours time, the mercury ascended four inches higher; and rested at five.

July 7. In fourteen hours, the ascent of the mercury was two inches and a half. In

Physico-mechanical Experiments.

In 10 hours, the ascent of it was $2\frac{1}{2}$.

July 8. In 14 hours, the ascent of the mercury was $1\frac{1}{2}$.

In 10 hours, the ascent was 2.

July 9. In 14 hours the ascent of the mercury was $2\frac{1}{2}$.

In 10 hours its ascent was $1\frac{1}{2}$.

July 10. In 14 hours the ascent of it was $1\frac{1}{2}$.

In 10 hours the ascent of it was 3.

July 11. In 24 hours, the ascent of the mercury was 4.

July 12. In 24 hours, the ascent was 4.

Now the mercury was brought to its wonted pressure.

July 13. In the morning, I found the cover broken; and because it was fastened by a screw, to prevent its being sever'd from the receiver, I suspected this happen'd from the internal air. I substituted another cover in its stead.

July 14, 15, 16, 17, 18. I perceived no change in the height of the mercury, because the cover was not exactly closed; and therefore I took out the fruit, and put part into another evacuated receiver, and the rest I stop'd up closely with common air, that nothing might get out.

In 4 hours, the mercury ascended 4 inches.

July 19. In 14 hours, the ascent of the mercury was $1\frac{1}{2}$; but, suspecting the air to have escaped, I set the screw.

In 9 hours, the ascent of the mercury was 11 inches.

The cover broke, and the air escaped.

This experiment seems to prove, that goosberries contain much air, which, as soon as it is freed from the wonted pressure of the air, more readily breaks out, than when restrained by some ambient air, till the goosberries begin to ferment; for then air is produced in a far larger quantity, tho' the compressure be greater.

In paste in va.
cuo.

(117.) July 8. 1676. I included paste in an exhausted receiver; and, before it was guarded against the external air, the mercury was come to the height of three inches; the air making an irruption from without: whence the paste, which was much swollen, lost about the third part of its tumidity.

A little while after, it swell'd again; and, within half an hour, the mercury mounted higher by two inches.

In one hour's time, the ascent of the mercury was two and a half; and the paste continued to swell.

In another hour, the ascent of the mercury was three inches and a half.

In an hour more, the ascent of it was four inches and a half; and it rested at sixteen.

July 9. In fourteen hours space, the ascent of it was twenty-one inches, and the height of the mercury thirty-seven. I suspected that some air had got out. When I set the screw, the cover broke; and, upon admission of the external air, the paste, which always rose, now abated about two inches of its tumidity; though it was less compressed than before.

In

In five hours, the ascent of the mercury was fifteen inches.

But, when I again endeavour'd to set the screw, the cover broke, so that the air escaped; and the paste was presently somewhat depressed.

In four hours, the ascent of the mercury was ten inches: the paste again swell'd, as before; but, being willing to substitute a better screw, I permitted the air to enter; yet the paste did not now subside, as before.

July 10. This night the paste rose again; yet it seemed to have produced no air.

In four hours there was no ascent of the mercury.

In seven hours, the ascent of it was four inches.

July 12. I perceived no ascent of the mercury.

13. It seemed to have ascended a little.

17. Seeing no more air produced, I took out the paste, and found it to have a sourish smell.

This experiment seems to prove, that air may be produced out of paste, in compressed air, as well as *in vacuo*.

But the paste was twice depressed, because the compressed air, suddenly finding a way to escape, was dilated; as happens in springs, when carried beyond their point of rest: but, when that air was immediately repell'd by the external air, the paste pitch'd, and was depress'd.

(108.) July 13. 1677 I included some horse-beans *in vacuo*, with water; ^{In beans in *vacuo*.} when, those which were bruised, seemed to swell much; but those which were left whole, suffer'd no sensible alteration.

In two hours space, I saw no air produced, tho' the beans continued to swell.

July 14. In twenty-four hours, the ascent of the mercury was seven inches.

July 15. In sixteen hours, the ascent of the mercury was three inches and a half.

In eight hours, the ascent of it was one and a half; the height of it twelve.

July 16. In fourteen hours, the ascent of it was three.

17. In twenty-six hours, the ascent of it was six.

18. In twenty-four hours, the ascent of the mercury was almost nine.

19. I stopp'd the receiver firmly with a screw, because the air had got out. In nine hours the ascent was one inch.

20. In twenty-four hours, the ascent was three and a half.

21. In twenty-four hours, the ascent was five and a half.

22. In fourteen hours, the ascent of the mercury was two.

23. In twenty-four hours the ascent of the mercury was eighteen.

24. In fourteen hours, the ascent of the mercury was almost five. The height of it thirty-five above the wonted pressure.

25. The receiver could not sustain a greater pressure. I found the beans of a fetid smell, not much unlike that of putrefied flesh.

Hence it seems to follow, that beans contain much air, and that it is produced in a moderate pressure, as well as *in vacuo*; sometimes more suddenly,

PNEUMATICS.

In goosberries
in vacuo.

ly, sometimes more slowly. But, especially, that great inequality, which happen'd July 23, is observable.

(109.) July 23. I included goosberries in an exhausted receiver, and guarded them very well against the external air.

In two hours, the mercury ascended one inch.

July 24. The height of the mercury was seven inches and a half.

July 25	} The height {	12		July 27	} The height {	20
26		of it was {		17		28

July 29. The height of it was almost 30.

30. The height of it was almost 31. I transmitted some air out of this receiver, into another evacuated receiver; and the height of the mercury was 26.

31. The height of the mercury was 35.

August 1. The height of the mercury was 39. But some air had escaped; and going to stop the receiver close, I suffer'd more air to get out.

The height of the mercury was 30.

Aug. 2. The height of the mercury was 39. I transmitted some air into another receiver.

The height of the mercury was 31.

Aug. 3. The height of the mercury was 39.

4. The height of the mercury was 41.

5. The height of the mercury was 43. I transmitted the air into another receiver.

The height of the mercury was thirty inches.

6. The height of the mercury was 43.

7. The height thereof was 47.

8. The height thereof was 48. But the air being transmitted into another receiver, the height of it was 36.

9. The height of the mercury was 41, in fourteen hours.

Aug. 10. The height of the mercury was 47; the air being transmitted into another receiver, the height of it was 35, in twenty-four hours.

11. The height of the mercury was 38 and a half, in fourteen hours.

12. The height of the mercury was 42, in twenty-four hours. I extracted the air, and the height of the mercury was 26.

13. The height of the mercury was 33, in twenty-four hours.

14	} The height of it was {	36	} 24		17	} The height was {	44	} 24	
15		39					18		47
16		41 $\frac{1}{2}$					19		50

I transmitted the air into another receiver; and the mercurial gage was spoiled. I took out the goosberries, and found they had lost their colour, and almost all their acidity.

From hence we may infer, that goosberries produce their air regularly, unless something be extracted out of the receiver; for then they acquire a power to produce new air more speedily.

(110.)

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(110.) Sept. 12. I put crude grapes into an exhausted receiver; but before they could be fenced from the external air, as much had got in as ^{PHNEUMATICS.} *In grapes in vacuo.* sustain'd three inches of mercury.

Sept. 13	} The height of it was	$\left\{ \begin{array}{l} 5 \\ 10 \\ 17 \end{array} \right.$	Sept. 17	} The height was	$\left\{ \begin{array}{l} 19 \\ 23 \\ 25 \end{array} \right.$
14			19		
16			20		

Sept. 22. The height of the mercury was 30. I stopped the receiver with a screw.

23. The height of the mercury was about 30 and a half.

24. The height thereof was 32.

Sept. 26	} The height of it was	$\left\{ \begin{array}{l} 34 \frac{1}{2} \\ 36 \frac{1}{4} \\ 36 \frac{1}{4} \\ 37 \frac{1}{4} \\ 37 \frac{1}{2} \end{array} \right.$	Octob. 2	} The height of it was	$\left\{ \begin{array}{l} 39 \frac{1}{2} \\ 39 \frac{1}{2} \\ 40 \frac{1}{2} \\ 41 \frac{1}{2} \\ 41 \frac{1}{2} \end{array} \right.$
27			4		
28			5		
29			7		
30			9		

Octob. 15. The height of the mercury was 46. It ascended chiefly on these two last days, when the frost was dissolved.

Nov. 2. The height of the mercury was 54.

5. The height was 58.

Jan. 10. 1677. The mercury was come to the height of 70 inches; yet I perceived no sensible change in the mercurial gage, even when the cold was sharpest; tho' the grapes and their juice were turn'd to ice.

September 21. Hitherto the grapes seemed not alter'd; but the mercury had ascended a little, because the air found a passage out. I open'd the receiver, and when the air broke forth, many of the grapes seemed to be wrinkled. The grapes had kept their taste, but it was much more pungent: the juice continued tinged of a curious red colour.

This experiment seems to inform us, that grapes produce not all their air, but in a long tract of time.

(111.) August 10. 1677. I put pears, cut asunder into an exhausted receiver. Towards evening the mercury was risen ten inches. ^{In pears in vacuo.}

Aug. 11	} The height of it was	$\left\{ \begin{array}{l} 20 \\ 38 \\ 48 \end{array} \right.$	Aug. 15	} The height was	$\left\{ \begin{array}{l} 55 \\ 60 \\ 68 \end{array} \right.$
13			16		
14			17		

The air being transmitted into another receiver, the height of the mercury remained at 53 and a half.

Aug. 18	} The height of it was	$\left\{ \begin{array}{l} 61 \\ 64 \end{array} \right.$	Aug. 20	} The height of it was	$\left\{ \begin{array}{l} 70 \\ 72 \end{array} \right.$
19			21		

The air being transmitted into another receiver, the mercury remained at 61.

Aug. 22	} The height of it was	$\left\{ \begin{array}{l} 68 \\ 74 \end{array} \right.$	Aug. 24	} The height of it was	$\left\{ \begin{array}{l} 79 \\ 81 \end{array} \right.$
23			25		

The air being transmitted into another receiver, the height of the mercury was 61.

PNEUMATICS.

Aug. 26. The height of the mercury was 56. Some air having got out, I transmitted the rest into another receiver, and the mercury remain'd at 52.

Aug. 27	} The height of it was {	60		Aug. 30	} The height was {	83
28		68		31		88
29		75		Sept. 1		93

Septemb. 2. The height of it was 100.

Sept. 3. The height of it was 89 ; some air having escaped, which made me cautious to prevent the like for the future.

Sept. 4. The height of the mercury was 100.

5. The same height continued.

7. The same height still continued, tho' no air escaped.

9. The height of the mercury was 107.

10. The height of the mercury was the same.

The air being transmitted into another receiver, the mercury rested at 99.

Sept. 11. The mercury moved not.

13. The height of the mercury was 105.

October 8. I found that the air had got out.

This experiment seems to inform us, that pears produce their air by fits.

Miscellaneous experiments, and first, melted lead and tin cooled in vacuo.

(112.) March 16. I melted down lead, with a fire, in a brass vessel, whose diameter was an inch and half ; but before the lead was concreted by cold, I put it into a receiver, out of which I suddenly exhausted the air ; whence the figure of the lead was concave, and the parts of it the more depressed, nearer the center : but lead, congealed in common air, exhibits a convex figure, except in the middle, where there is a little cavity.

I made the same experiment on tin, with the same success ; and tho' both metals being fluid, and very hot, had remained long in vacuo, yet no bubbles seemed to rise from either ; but all other hot liquors seem to yield numerous bubbles in vacuo.

Salt and water in vacuo.

(113.) Sept. 2. I put water, saturated with salt, in vacuo ; to try whether it would be there converted into crystals, and the salt be carried above the superficies of the water, as happens in the free air.

Sept. 15. The water, with the dissolved salt, abiding in the same state, I open'd the receiver ; and, as no vapours could escape, 'tis reasonable to judge, that the salt could not there be converted into crystals.

The air of goosberries in vacuo.

(114.) August 8. 1676. I put air produced from goosberries, into an evacuated receiver, furnished with a mercurial gage.

March 1. 1676. I perceived no change in the height of the mercury, and therefore, open'd the receiver.

The weight of air to that of water.

(115.) August 8. I took a vial, able to hold seven ounces, five drams, and three grains of water, and exhausted the air out of it ; and, when, in a balance, it was suspended in equilibrium, with another weight, I pierced

pierced the bladder which cover'd the orifice, with a needle; and then, the vial being fill'd with air, appear'd heavier by four grains and a half; which latter weight to the former, is as 1 to 814: whence it follows, that water is about, at least, 800 times more ponderous than air of an equal bulk. This day was hot and clear; and some air is always left in the receivers after exhaustion.

(116) Jan. 16. 1677. I put *Aqua fortis*, with fixed nitre, into a receiver; and, having exhausted the air as much as I could, poured one of them on the other, and found much air produced. I marked the height of the mercury in the gage. *Aqua fortis, and fixed nitre in vacuo.*

March 5. Finding the produced air was not destroy'd, and that the mercury persisted at the same height, I open'd the receiver, and found nitre produced *in vacuo* from the mixture.

(117.) May 12. 1677. I fill'd a long and very narrow-neck'd vial, with oil, up to the middle of the neck, and put it into a receiver, firmly stopp'd by the help of a screw; into which, I afterwards intruded air, till it sustain'd 120 inches of mercury above its wonted height. The oil, in the neck of the vial, appear'd depressed about a quarter of an inch; the cause whereof I judg'd to be the compressure of the air: but, having eased the screw, and thereby suffer'd the air to break in, and be dilated, the oil did not ascend at all; so that, I suppose, it was condensed only by cold. *Oil, water, and spirit of wine, in compress'd air.*

August 5. I made the same experiment, after the same manner, using water instead of oil; yet could perceive no change of the height of the water in the neck of the glass; tho' the heat, being moderate, might have produced a sensible effect.

Jan. 14. 1678. Finding, by some experiments, that compressed air enters into the pores of the water, and pierces even to the bottom, a suspicion might arise, that the water was not condensed by the compressed air, because the air entering into the pores, made the pressure within equal to that from without; I, therefore, filled the abovesaid glass with spirit of wine; leaving only the length of three inches in the top of the neck thereof, which was filled with air only. Then applying my hands to the glass, the spirit of wine, being heated, soon filled the whole neck to the top. The glass being now inverted into a vessel of mercury, I removed my hands, when the spirit of wine being soon cooled, suffer'd the mercury to possess three inches in height. I put the vessel, and the glass, in that posture into a receiver, and afterwards compressed the air therein, till the mercury exceeded its wonted height 90 inches; yet there was no sensible condensation of the spirit of wine, nor any ascent of the mercury: however, it is certain, that no air had crept in, because the mercury hinder'd it; and the receiver being open'd, when the air, that compressed from without, was dilated, no bubbles appear'd in the spirit of wine.

Here it seems worth enquiring, how the spirit of wine was so sensibly condensed by a moderate cold, and not at all by a great compressure of the air.

PNEUMATICS.

Spirit of wine,
and oil of tur-
pentine in vacuo.

(118.) May 12. 1676. I poured spirit of wine into a glass vessel, and added some drops of oil of turpentine thereto, which swimming upon the spirit of wine, began to be there whirl'd about. I put the glass vessel on the pneumatic engine, and cover'd it with a receiver; yet the bubbles did not at all cease to move up and down. Then I pump'd out some of the air; when, the bubbles, emerging from the spirit of wine, adhered to the drops of oil, and carried them to the sides of the vessel, and there detained them; yet two drops, free from such bubbles, proceeded to have a further motion. Afterwards, I wholly exhausted the receiver, and some drops rose to the top thereof, by the force of the bubbling spirit of wine; but the remaining drops continued to be moved a little, and soon after rested. The air being admitted, the drops began again to renew their motion, but it was slow, and quickly ceased.

I repeated the experiment with spirit of wine, and oil of turpentine, purged from air; and no ebullition was then made, nor did any bubble appear: but the drops of the oil of turpentine were moved *in vacuo*, as in the open air.

Hence, it seems to follow, that the cause of the motion of the drops, is not owing to the dissolution; for all dissolutions *in vacuo*, have, hitherto, seemed to me, to produce bubbles.

Radishes and
claret in vacuo.

(119.) May 19. 1676. Yesterday I left two radishes *in vacuo*, one of them hanging with the root downwards, the other in a contrary posture; and both cut transversely, rested over a vessel, which contain'd red wine. These remaining for a whole night *in vacuo*, seem'd well purged of their air. Opening the receiver, I added two other radishes to the former, cut after the same manner, having first taken off their thick skin. Then exhausting the receiver, I immerg'd the cut part of all the radishes, at once, into the subjacent wine; upon which, many bubbles seem'd to arise from them: and more bubbles proceeded from those radishes which were purged of air, for a whole night, than from those which had not remained above half an hour *in vacuo*, with their skins off.

Hence bubbles seem to be formed of particles of air, swimming in water; and because, in the skin there are some canals, fit to retain parts of air, the peeled radishes afforded no opportunity for the formation of so many bubbles.

The liquor ascended no less into those radishes which hung with their roots upwards, than into the others.

A small glass
tube, plunged in
water, the insu-
sion of naphritic
wood, and spirit
of wine, in va-
cuo,

(120.) May 4. 1676. I immersed one end of a small open glass tube, into water stagnant *in vacuo*, and presently the water ascended up into it, as usual in common air, and to the same height; but, soon after, many bubbles being formed there, raised the water higher, and kept it suspended in three different places, interecepted by many bubbles; and several other bubbles seemed to pass out from the end immersed in water.

Then sealing the other end of the tube hermetically, and making the experiment in common air, the water ascended not up into the tube at the open end. But, *in vacuo*, it ascended therein, as if it had been open at

both ends ; and many bubbles suddenly formed, separated the water, contained in the tube, to a great distance, as before : in the mean time, many other bubbles seemed incessantly to pass out from the end of the tube immersed, tho' they afterwards appear'd less frequent.

But the water being suspended higher in the tube, seemed to contain no bubbles, whilst the end only emitted so many.

Then I took out that end from the water, and no more bubbles appear'd, tho' it was wholly fill'd with a cylinder of water.

May 5. I repeated the experiment ; but before I had immers'd the end of the tube in water, a drop, which ran over from the upper aperture of the receiver, flowed down to the open end of the tube, and penetrated into it to the height of two lines ; and no bubble was formed there in a full half hour. I, afterwards, plunged the end of the tube into the water of the vessel, and bubbles soon began to be formed as before ; some of which succeeded others within half a minute : but, afterwards, they were less frequent. Repeating this experiment, many times, I perceived, that when the water was extracted from the tube, no bubbles appear'd ; but if it were immersed in water, some would adhere to the end of it, either sooner or later.

May 6. I made the same experiment, with the infusion of nephritic wood, with a like success ; excepting that the bubbles emerged, and penetrated the liquor, before they had acquir'd any considerable bigness : whence we may conjecture, that this liquor is very thin, and hath no viscosity to resist a pervading body.

May 10. I repeated the same experiment with spirit of wine, mixed with a certain oil, made *per deliquium*, but found nothing new ; only the liquor ascended not so high into the tube.

Hence the bubbles seem to be formed, at the extremity of the tube, of aerial particles, swimming in the water ; which finding some impediment at that end, cannot pass by, and so, new ones coming upon them, they swell into bubbles.

(121.) July 18. 1676. Two days ago, I took some horse-beans, and included them in an iron tube, closely stopped ; first pouring water on the compressed beans, till the tube seemed wholly full ; to try whether the expansive force of the beans would break the tube. This day the tube seem'd not to be alter'd ; but, the stopple being loosen'd, some air broke out, and much water, which was not imbibed by the beans, fell upon the ground : then we heard a noise, as it were, of bubbling water, for above an hour.

Horse-beans and water included in an iron tube.

July 25. The tube remain'd in the same posture ; but now one of the ends of it being unstopp'd, and some beans taken out, the murmur of the bubbling water was heard as before.

From hence it seems to follow, that beans contain air, which, in a great compressure, cannot escape ; but breaks out, if freed from the compressing force.

PNEUMATICS.

(122.) March 4. 1677. I put a glass, half full of spirit of sal armoniac, and copper filings, into a well exhausted receiver, and stopp'd it up: in 15. minutes, the liquor had contracted a blue colour, very much diluted; but, the air being admitted, in three minutes the blue colour appeared vivid and thick. I put the liquor, so tinged, again *in vacuo*, to try whether that colour would, in time, vanish.

A certain oil,
and spirit of
wine in vacuo.

April 4. The blue colour almost quite disappear'd, but quickly return'd, upon admission of the air.

(123.) May 8. I put a certain oil, made *per deliquium*, and spirit of wine, into an exhausted receiver: the spirit always swam on the top; and, lest the spirit should bubble over the edges of the vessel, I extracted the air, by degrees; when, at first, great bubbles arose from the spirit, and but very small ones from the oil; after one hour, the oil afforded large bubbles, which, from being small at the bottom, fill'd, in their ascent, the whole breadth of their vessel: and, after another hour, some bubbles broke out with so great force, that they hit against the top of the receiver.

May 9. I repeated the experiment in a glass somewhat long and narrow, that I might the better perceive the motion of the bubbles; and I saw the bubbles passing out of the oil into the spirit of wine, without any great increase of their quantity: but being distant only one quarter of an inch from the superficies, they were suddenly expanded.

Aqua fortis,
spirit of wine,
and iron in va-
cuo, and com-
mon air.

(124.) May 3. 1676. I mixed a quantity of *Aqua fortis*, with a larger of spirit of wine; then distributed the mixture equally into three glass vessels, and put three equal pieces of iron into them, to each vessel one. This done, I included one of the three vessels *in vacuo*; and there many great ebullitions were made. In a quarter of an hour I took out the vessel, and found the liquor black and turbid; whilst the other two vessels had their liquor not alter'd in colour; only some black powder appear'd at the bottom.

One of these two vessels I put *in vacuo*, and there arose ebullitions, great indeed, but much less than the former: in one quarter of an hour, I took out the vessel, and found the liquor black, yet less so than the former; but that which was left always in the air, remain'd, in a manner, unchanged.

May 4. In the morning, the liquors in the two vessels, put *in vacuo*, appear'd clear and green.

But that in the open air bubbled more strongly, than it did yesterday, and was of a red colour. I put the three vessels together *in vacuo*, and perceiv'd no remarkable ebullition; only some bubbles appear'd larger in the red liquor, than in the other two.

From hence it seems to follow, that spirit of wine accelerates ebullition *in vacuo*.

Spirit of sal-ar-
moniac and
copper filings in
artificial air of
paste.

(125.) Jan. 21. 1678. I had a glass half full of spirit of sal armoniac, and filings of copper, the mouth whereof was so exactly stopp'd, that the blue colour, induced by the external air, now wholly disappear'd. The stopple was made of leather, prepar'd after a particular manner.

This glass I set *in vacuo*, with unfermented paste, that the receiver being full of air, from the paste, I might perforate the leather that stopp'd the glass;

glass; and try, whether the contact of the air, generated from the paste, would also communicate a colour to the liquor.

Jan. 22. There was no need to perforate the leather; for I found the liquor already tinged: whence it is probable, that air produc'd from paste, is endu'd with such minute particles, as to penetrate leather, which is impervious to common air.

Jan. 25. The liquor became almost colourless; whence it appears, that common air is too thick to penetrate all passages, which are pervious to air, produc'd from paste.

Feb. 2. I put the same vial *in vacuo*, but did not cement the receiver to the cover; so that the air, gradually entering, in twenty-four hours, fill'd the receiver, as it was leisurely fill'd with the air produc'd from paste; yet the liquor still remain'd colourless.

Feb. 15. I put the same glass again *in vacuo*, with some quantity of paste; but, this time, the air produc'd from thence, did not pervade the leather, as it had done before, and the liquor was not at all tinged.

(126.) April 2. 1678. I put a shrew-mouse into the filtering engine; and, when I perceiv'd him reduc'd to extremity, I began to stir the pump, that the air, might be, as it were, filter'd thro' the water. The mouse, a while after, seem'd to be better, yet not wholly restor'd; and having been long kept fasting, I am uncertain, whether he died for want of aliment, or of new air.

A shrew-mouse in an engine that filters air thro' water.

April 12. I repeated the experiment with a small weakly mouse, that had been kept a long time without food. And finding the same success as before, I took out the mouse before he was dead, but he recover'd not: so that more experiments are requir'd, to shew the effect of this filtration.

(127.) May 2. 1678. Six weeks ago, I included frog-spawn in three receivers, the first of which was exhausted; the second contain'd common air; and into the third, I intruded so much air, that the mercury rested sixty inches above its usual height.

Frog-spawn in vacuo, common air, and compress'd air.

In fifteen days, the mercury in the evacuated receiver rose an inch. The spawn in the common air seem'd corrupted, and of a blackish colour; but that in the compressed air, remain'd unalter'd in colour; tho' no frogs were generated.

In a month's time, the sperm *in vacuo* had not changed its colour, excepting the black round spots; but seem'd reduc'd into water: the colour of that in the common air was very black, but in the compress'd air the spawn began to be reddish.

As yet, no change was perceiv'd, either in the spawn *in vacuo*, or that in the common air; but in the compress'd air it appear'd redder.

May 22. The sperm *in vacuo* was not chang'd; in the compress'd air it remain'd red; but in the common air it again became colourless.

June 23. The sperm *in vacuo*, and in common air was not tinged, but in the compress'd air it inclin'd to green.

Octob. 15. I took the spawn from all the vessels; that kept *in vacuo* was almost exhaled out of its vessel, and appear'd stagnant in the receiver, like clear water: that in the common air remain'd colourless; but that in the compress'd air still kept its red colour.

PNEUMATICS.

Oranges in receivers, with and without water.

(128.) May 9. 1678. Six days ago, I included two pieces of the same orange in two receivers, not quite of equal bigness; in the greater, there was left some quantity of water, so that the same space remain'd for the air in that, as in the less. The orange included with water, tho' it were not touch'd by it, was four times more mouldy, than that kept without water.

And, therefore, in repeating this experiment, I put two pieces of the same orange into two receivers; but fill'd the third part of one of them with water, yet so, that it did not reach the orange.

June 15. Neither of the pieces had contracted any mouldiness.

May 16. I repeated the experiment with the same success; only, neither orange had acquired any mouldiness in the space of more than a month; tho', in former experiments, all such oranges grew mouldy.

The cause of the difference, seems to be some particular disposition of the air.

Turpentine included in a wind-gun.

(129.) June 1. 1678. I put a small glass tube, half full of Venice turpentine, into our wind-gun; and had scarce reduc'd the air to the tenth part of its wonted space, but the leather, spread over the elliptic valve, was driven out; so that, the air having escap'd, I drew the glass-tube out of the engine, and found many bubbles formed in the superficies of the turpentine. I, therefore, suspected, that the air had pervaded the turpentine; and that it would have penetrated deeper into it, if they had remain'd longer thus inclos'd together. I plac'd the same tube in the same gun, and there left it in air reduc'd to about the fifteenth part of its natural space.

June 3. I open'd the engine, and, taking out the tube, found the turpentine almost free from bubbles; yet, by degrees, many were formed therein, in the parts remote from the superficies.

June 4. I put new turpentine into the same tube, and included it *in vacuo*, that it might be the better purged of air; then I pour'd the water upon it, and shut up all in the wind-gun.

June 8. I open'd the engine, and, at first sight, both the water and the turpentine in the tube, seem'd to be very free from bubbles; but soon after I perceiv'd, that bubbles were form'd in the turpentine, and that they ascended by degrees: some of them seem'd to be made, almost at the very bottom, about half an inch below the superficies of the turpentine. Whence we may conjecture, that all the water, and so great an height of the turpentine, were pervaded by the air, which formed those bubbles.

Spirit of sal-armoniac, and copper-filings in vacuo.

(130.) August 11. 1678. I included spirit of sal-armoniac, with a mercurial gage, *in vacuo*; and after the spirit ceas'd to emit any bubbles, I mix'd copper-filings therewith, which caus'd many bubbles to rise again; but they were so far from producing any air, that they consum'd what was there before. But the liquor became greenish and turbid.

Decemb. 5. The spirit was almost all exhaled out of the containing vessel, and, being condens'd in the receiver, remain'd still turbid, by reason of much filth, which was included there: but that which was not exhaled out of the vessel, appear'd clear like water. The mercury, also, was wholly expell'd

expell'd out of the gage. Whence I conjecture, that the air in the receiver, PNEUMATICS.
was gradually more consumed.

(131.) Sept. 2. 1678. I put two cylinders, one of tin, the other of lead, Cylinders of tin
and lead immer-
sed in mercury,
in vacuo, and
in common air.
in vacuo; their lowest parts were immerfed in mercury; and, at the same time, I immerfed two other cylinders, like the former, after the same manner in mercury: but these latter were left in the free air.

Sept. 6. I open'd the exhausted receiver, and the mercury in the tin cylinder was risen four inches and a half, above the superficies of the stagnant mercury; and cutting the cylinder transversly, in the middle of that height, the amalgam seem'd to have penetrated into the cylinder, about half a line. And cutting the cylinder transversly again, in that part, which was distant only one inch, from the superficies of the stagnant mercury, I found the thickness of the amalgam equal to one line.

In the lead-cylinder, the mercury rose two inches and a half; but, only as far as the superficies; and the very part, immerfed in the mercury, was not penetrated by it, to any sensible thickness.

Sept. 7. I took the tin-cylinder left in the air, out of the mercury, in which it was immerfed, and found the mercury to have ascended to the height of five inches.

Sept. 10. The same cylinder being left in the mercury, seem'd to be be-
fmeared therewith to the very top, six inches, and more, above the super-
ficies of the stagnant mercury. When the cylinder was transversly cut in
several places, the mercury appear'd to have pierc'd the deeper into the tin,
the nearer it came to the stagnant mercury; so that in the part adjacent to
the mercury, almost the whole diameter of the cylinder, three lines broad,
was penetrated thereby.

In the lead-cylinder, the mercury exceeded not the height of three inches
and a half; neither had it penetrated to any sensible thickness. Whence
it appears, that the weight of the air, contributes little or nothing to the
ascent of mercury into metals.

(132.) Decemb. 12. 1678. I took a small whiting, and having cut off his
head, divided him transversly into five pieces; the first whereof, I included
in vacuo. The second in common air. The third in air so compress'd, as
to sustain mercury fifty inches above its wonted height. These three re-
ceivers were closed with screws. The fourth piece was put into a receiver,
full of air, produc'd from paste, which was presently stopp'd. The fifth
was left in the free air. A whiting inclu-
ded in vacuo,
in common air,
in air com-
press'd, in arti-
ficial air, and
left in the open
air.

Decemb. 15. In the morning, that part of the whiting, which was left in
the free air, began to shine; and, towards evening, it gave a more vivid
light.

Decemb. 16. In the morning, the whiting left in the free air, ceas'd to
shine; but towards evening shone again.

Decemb. 17. This morning, the same part of the whiting shone a little,
yet less than yesterday in the evening.

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Decemb. 18. In the morning, there appear'd no light, tho' I long fix'd my eyes upon the receiver in a dark place; but the night coming on, the light appear'd again.

Decemb. 20. Hitherto the same part of the whiting left in the air, continued to shine; but all the other parts did not yet begin to do so.

Decemb. 22. Yesterday, the light of the whiting, left in the air, had not quite ceas'd, but this day it appear'd no more.

Decemb. 24. The part of the whiting in the free air, entirely gave over shining; that included in common air, did, yesterday, yield a faint light; but this day it shone no more.

Decemb. 26. No more light appear'd in that in the common air: but the three other pieces did not begin to shine.

Jan. 26. 1679. I perceiv'd no more shining in any one of the receivers.

Artificial air
destroy'd; and
first that of
cherries trans-
mitted into a
receiver full of
common air.

(133.) Aug. 3. 1677. I transmitted air, produc'd from cherries, into a receiver full of common air, but so stopp'd with a screw, that the mercury ascended to twenty-five inches above its usual height.

Aug. 4. The mercury was depress'd about two inches. The height of it, this day, was only twenty-three.

Aug. 6. The height thereof was reduced to twenty.

Aug. 7. The height thereof the same.

Aug. 8. The mercury was somewhat depress'd.

Aug. 10. The height of it was nineteen and a half, above its usual standard: and perceiving little or no alteration, I open'd the receiver.

Hence we have a confirmation, that air, produc'd from fruits, at the beginning, is in part destroy'd; but, that the rest can very long retain the form of air.

That of sal-ar-
moniac, and oil
of vitriol, in va-
cuo.

(134.) May 26. 1676. I put six grains of sal-armoniac into a receiver, with a sufficient quantity of oil of vitriol: then, the air being exhausted, I forc'd down the salt into the oil; whereupon, a great ebullition presently follow'd, and the mercury ascended in the gage, almost to its wonted height; but presently after it sunk again, and return'd to its former state.

May 27. I repeated the experiment; the salt remaining ten hours *in vacuo*, before it was put into the oil; but the ebullition proceeded as before; yet, the air was produced much more slowly, nor could it wholly be destroy'd, in seven or eight hours time; yet at last the mercury descended to the very bottom.

May 29. I made the same experiment again; leaving the materials for twenty-four hours *in vacuo*: the ebullition seem'd much less, and the air was produc'd, both in a less quantity, and more slowly than before. I observ'd also, that whilst the materials remain'd *in vacuo*, before their mixture, the mercury came nearer to the open end of the gage, as if some air had been either extracted or destroy'd.

And of oil of
vitriol with a
fifth part of
common air.

June 8. I put oil of vitriol, alone, into a receiver, in which, I left only a fifth part of common air; to try, whether this oil, without sal-armoniac, would diminish the elastic force of the air: but the force of the air was increas'd, and the mercury in one hour's time seem'd to have ascended a little

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little into the gage; tho', afterwards, for twenty-four hours no change happen'd.

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This experiment shews, that some artificial air may be destroy'd; but why this destruction happens, sometimes sooner, sometimes later, deserves a further enquiry.

(135.) *July 10. 1676.* I put paste, made two days before, and now grown sourish, into a receiver, and stopp'd it firmly with a screw.

In one hour, the height of the mercury was one inch.

In seven hours, the height of it was six.

July 11. The height of it was eleven.

July 12. The height of the mercury was twenty-four.

July 13. The height thereof was thirty.

July 14. The height of the mercury was sensibly greater.

July 15. The mercury ascended a little. Measuring its height exactly, I found it thirty-eight inches.

July 19. No more air was produc'd from the paste.

July 10. 1676. I put another quantity of the same paste, much less than the former, into an exhausted receiver.

Tho' the quantity of the paste was less, yet, in one hour's time, the height of the mercury was two inches.

In seven hours, the mercury came almost to the top of the gage; but it was a short one.

July 19. The paste was not able to move the receiver from its cover; tho', at the beginning, it had produc'd a greater quantity of air, than the paste in common air. I endeavour'd to fire it with a burning-glass, and the fumes, elevated therefrom, afterwards falling upon the paste, tinged the superficies thereof, with a pleasant yellow colour: and that air was thus produced, I conjectur'd, because the cover was afterwards easily sever'd from its receiver.

Hence we learn, that air is sometimes generated much more easily in *vacuo*, than in common air.

(136.) *August 20. 1676.* I put paste, kept for 24 hours, in a receiver full of common air; to which I added new air, so that the mercury exceeded its wonted height, four inches, and a half.

In six hours, the mercury gained almost 4 inches; and its height was 8.

Aug. 21. The ascent of the mercury was 4 and $\frac{2}{3}$.

Aug. 22. The ascent of it was about 1.

23. The ascent of it was half an inch.

26. For three whole days, the ascent of the mercury was only half an inch.

27. There was no ascent of it at all.

29. The paste, taken out of the receiver, smelt acid.

August 20. I put another quantity of the same paste into an exhausted receiver, and observ'd the same proportion between the quantity of the paste, and the capacity of the vessel, as in the former experiment.

The mercury presently seem'd to have ascended. Its height was two inches.

LIII 2

Aug.

The different celerity wherewith air is produc'd in vacuo, and in common air, shewn from paste in common air.

Paste in vacuo.

PNEUMATICA.

Aug. 21. The ascent of the mercury was 5.

22. The ascent of it was 3.

23. The ascent of the mercury was 1.

26. For three whole days, the ascent of it was 2.

27. There was no ascent of the mercury.

29. I took out the paste, exhausted of its air, from the receiver.

This experiment farther confirms, that air is, sometimes, more easily produced *in vacuo*, than in common air.

Filberd-kernels
in *vacuo*.

(137.) Sept. 4. 1677. I put the kernels of filberds into an exhausted receiver.

Sept 5. The height of the mercury was 5 inches.

Sept. 6

7

8

9

The height
of it was

10

10

12

15

Sept. 11

12

13

14

The height
of it was

18

23

27

29

Sept. 15. The height of it was almost the same.

17. The height of it was 30.

18. This day the air began to get out of the receiver; for some bubbles appear'd in the turpentine, which closed the juncture of the receiver, and cover.

And filberd-kernels
in common
air.

(138.) September 4. I put kernels of filberds into a receiver with common air.

In the afternoon, the quantity of air seemed to be lessen'd.

Sept. 5. The height of the mercury was less than half an inch.

9. The height of it was the same.

7. The height of it was 1 inch.

8. The same height continued.

18. The same height continued.

This experiment confirms, that sometimes air is produced much more easily *in vacuo*, than in common air.

Raisins with
water in *vacuo*.

(139.) September 15. 1677. I included 8 ounces of raisins of the sun, bruised and diluted with a little water, in an exhausted receiver, able to hold 22 ounces of that fluid.

Sept. 16. The height of the mercury was six inches.

Sept. 17

18

The height
of it was

10

15

Sept. 29

20

The height
of it was

29

29

Sept. 21. This day I found the receiver forced from its cover.

Sept. 24. I took out some of the raisins; but those that remain'd, I enclosed in the same evacuated receiver.

Sept. 25. The raisins forced the receiver, now full of air, from its cover.

And raisins with
water in
common air.

September 15. 1677. I put 8 ounces of raisins of the sun, bruised and diluted with a little water, into a receiver, able to hold 22 ounces of water; but did not exhaust the air at all.

Sept. 16. The mercury was three quarters of an inch above its usual height.

Sept. 17. The height of the mercury was 1 and a half.

18. The height of it was 3.

Sept.

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Sept. 19 }
20 } The height of it was { 5
21 } 7
9

Sept. 22 }
23 } The height was { 11
24 } 12
15

PNEUMATICA

Permitting the air to break out, many bubbles emerged from the raisins. This experiment further teaches, that air is sometimes much more easily produced in *vacuo*, than in common air.

(140.) February 17. 1677. I put three onions into an exhausted receiver. *Onions in vacuo*

Feb. 19. The height of the mercury was one inch.

21. The ascent thereof was again 1. The onions were not alter'd.

25. The whole ascent of the mercury was 9. The onions not alter'd.

May 4. The onions had yet suffer'd no alteration.

18. Neither were they yet alter'd.

June 19. I found the receiver forced from its cover, and the onions rotten.

Feb. 17. I inclosed 3 onions in air, so rarified, that it could sustain only ten inches of mercury. *Onions in rarified air.*

Feb. 19. There was no ascent of the mercury.

21. There was yet no ascent thereof. The onions did not sprout, but contracted a mouldiness.

25. The ascent of the mercury was about 7 inches.

The onions received no further alteration.

May 4. The onions were not alter'd.

18. The onions were not yet alter'd; but the receiver, by the force of the produced air, was removed from its cover.

February 17. I put 3 onions in a receiver not exactly shut.

And onions in common air.

21. They contracted no mouldiness, but sprouted.

25. They gradually took root.

May 4. The onions began to be mouldy.

This experiment makes it probable, that some bodies produce their air not much more easily in *vacuo*, than in rarified air.

It hence also appears, that vegetation is hinder'd, not only by the evacuation, but also by the rarification of the air.

It likewise deserves our observation, that the onions, as long as their roots sprouted, contracted no mouldiness.

(141.) August 23. 1677. I put bruised pears into an exhausted receiver, with a mercurial gage.

The difference betwixt whole, and bruised fruits, shewn in bruised pears in vacuo.

August 25. The height of the mercury was five inches.

Aug. 26 }
27 } The height of it was { 10
28 } 14
18

Aug. 29 }
30 } The height was { 21
31 } 25
28

Sept. 1. The height of it was 30.

2. The receiver was forced from the cover.

August 23. I put whole pears into an exhausted receiver; the quantity of the pears, and the capacity of the receiver, being the same with those just mention'd. *And whole pears in vacuo.*

Aug. 25. The height of the mercury was 11.

Aug.

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Aug. 26 } The height { 17 | Aug. 28 } The height { 28
27 } of it was { 25 | 29 } of it was { 30

Aug. 30. The mercury ascended no higher, the receiver being forced from the cover.

This experiment seems to prove, that bruised fruits do not produce air so soon as entire ones.

In whole apples
in vacuo.

(142.) August 24. I enclosed whole apples in *vacuo*, with a mercurial gage.

August 25. The height of the mercury was 5 inches.

Aug. 26 } The height of it was { 9 | Aug. 29 } The height was { 19
27 } { 12 | 30 } { 25
28 } { 15 | 31 } { 28

September 1. The height of it was 29.

2. The height of it was 30.

3. The receiver was forced from the cover.

And bruised ap-
ples in vacuo.

August 24. I put an equal quantity of bruised apples into an evacuated receiver, of the same capacity with the former.

Aug. 25. The height of the mercury was 1 inch.

26. The height of it was 3.

27. The height of it was 4.

Sept. 3. The mercury continued at the same height.

25. The mercury ascended not.

This experiment seems to inform us, that bruised fruits produce air, slower than whole ones.

In bruised grapes
in vacuo.

(143.) August 25. 1677. I put unripe grapes, bruised, into an evacuated receiver.

Aug. 26. The height of the mercury was one inch.

27. The height of it was two inches.

28. The height of it was 2 and a half.

29. The height of the mercury was the same.

Sept. 15. The mercury did not ascend, but its height remained at 2 $\frac{1}{2}$.

And whole
grapes in vacuo.

August 25. 1677. I put whole unripe grapes into an evacuated receiver.

Aug. 26. The height of the mercury was three inches.

27. The height of the mercury was five.

Aug. 28 } The height { 7 | Aug. 30 } The height { 12
29 } of it was { 10 | 31 } of it was { 13

Sept. 1. The height of the mercury was 15.

2. The height of it was 16.

3. The height of it was 18.

4. The height of it was the same.

Sept. 5. The height of the mercury continued the same; but almost all the grapes had contracted a yellow colour.

Sept. 7. The mercury rested at the same height; and all the grapes were yellow.

Sept. 15. The height of the mercury was twenty.

This

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This experiment shews, that whole fruits produce air more readily than bruised. Pneumatica

(144.) Sept. 10. 1677. I put two ounces of grapes, not bruised, into a receiver able to hold ten ounces of water. In whole grapes.

Sept. 11. The height of the mercury was six inches.

Sept. 12 } 13 } 14 }	The height of it was	{ 9 12 15 }	Sept. 15 } 16 } 17 }	The height of it was	{ 20 25 28 }
----------------------------	-------------------------	-------------------	----------------------------	-------------------------	--------------------

Sept. 18. The height of the mercury was thirty. The grapes were not at all altered,

Sept. 19. The height of the mercury was the same.

Sept. 20. The receiver was not yet forced from the cover. The grapes were not altered, but appeared only a little riper.

Sept. 21. The receiver was forced from the cover, tho' nothing had escaped.

Sept. 22. In the morning, the grapes began to rot; I, therefore, included them again *in vacuo*.

Sept. 23. The height of the mercury was five inches.

Sept. 24 } 25 } 26 }	The height of it was	{ 9 14 17 }	Sept. 27 } 29 } 30 }	The height of it was	{ 20 27 28 }
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Octob. 10. The receiver was not forced from the cover, till to-day: the grapes, by their colour, seem'd rotten, yet kept their firmness.

Sept. 10. 1677. I included two ounces of ripe, bruised grapes in a receiver capable of holding ten ounces of water. And bruised grapes.

Sept. 11 } 12 } 13 } 14 }	The height of the mercury was	{ 4 7 10 12 }	Sept. 15 } 16 } 17 } 18 }	The height of it was	{ 15 18 20 25 }
------------------------------------	----------------------------------	------------------------	------------------------------------	-------------------------	--------------------------

Sept. 19. The grapes had severed the receiver from the cover, and much juice was spilt.

Sept. 20. I again put the same grapes into the same receiver; but, because they had spilt their juice by ebullition, I did not exhaust all the air: the mercury rested at the height of five inches.

Sept. 21. In the morning, the receiver, being now full of air, no longer adher'd to the cover; so that I took out the grapes, and transmitted them into another receiver, which I stopp'd close with a screw, but extracted no air from it.

Sept. 22. The height of the mercury was eleven inches; tho' the receiver was able to hold twenty-six ounces of water.

Sept. 23. The height of the mercury was nineteen.

Sept. 24. The height of it was the same.

Sept. 30. The height of it was twenty.

Octob. 3. When the grapes produc'd no more air, I took them out, and found them of a bitter taste; being not yet perfectly ripe.

This

PNEUMATICS.

And in found
and bruised ap-
ples, in vacuo.

This experiment, compar'd with that before related, of unripe grapes, seems to intimate, that unripe grapes produce less air when they are bruised, than when whole; but that ripe grapes do the contrary.

(145.) Nov. 19 1678. I put apples into three evacuated receivers. In the first was a sound apple; in the second an apple bruised, and laid loose in the open vessel; in the third, was also a bruised apple: and the cover of this so fitted the including vessel, that it straitly compressed the parts of the apple; but in exhausting the receiver, the air, formed between the parts of the apple, expelled all the juice.

Nov. 21. In the first receiver, the height of the mercury was five inches; in the second, three; in the third, none.

Nov. 23. In the first receiver, the height of the mercury was seven; in the two others there was no change.

Decemb. 7. In the first receiver, the height of the mercury was eleven. There was no alteration in the other two.

Jan. 23. The first receiver was now severed from its cover, by the force of the air produced a-new. In the two others there was no air generated.

May 20. 1679. The third receiver was forced from its cover; but the second had produced no air.

This experiment informs us, that bruised fruits produce less air *in vacuo*, than sound ones; contrary to what happens in common air. The reason whereof, may, perhaps, be this, that fruits bruised are very much rarified *in vacuo*; whence the several principles, of which they consist, cannot act upon one another: but unbruised fruits, by reason of the entireness of their ambient skin, suffer less rarification.

That air is
sometimes unfit
to produce mould-
iness, shewn by
roses in common
and compressed
air.

(146.) July 12. 1678. I put roses into two receivers, to be stoppt with screws. One of them contain'd common air uncompress'd; but I intruded so much air into the other, as sustain'd the mercury sixty inches above its wonted height.

Aug. 2. The roses in the common air, were, four days ago, turn'd yellow, as if they had been wither'd; but those in the compressed air, kept their colour very well.

Feb. 10. 1679. Those in the compressed air, retain'd their fresh colour.

This experiment, compar'd with that made, last year, with roses, informs us, that the air, at different times, is differently affected; so that sometimes it hath a power to hinder corruption, and sometimes to promote it.

And by tulips
and lark-spurs.

(147.) May 22. Fifteen days ago, I included two equal quantities of flowers, in two receivers: into one of them, I thrust so much air as sustain'd the mercury sixty inches above its wonted height; but in the other, I left common air uncompress'd. The flowers were tulips and lark-spurs.

Since that time no mouldiness appear'd, except, only, that ten days ago, one half of the tulip, in the common air, being cut asunder, seem'd somewhat mouldy; and now the other half of the same tulip in compressed air, seem'd also a little mouldy.

Some of the flowers seem'd as fresh, as when first put in; especially those in the common air; but in the compressed air, they seem'd moister.

June

June 22. No more mouldiness appear'd : whence we have it confirm'd, that the air is, sometimes, unfit to produce mouldiness ; since, last year, all this kind of flowers, contracted a great mouldiness.

(148.) Sept. 4. 1678. I exposed one dram of minium, in an open glass, to the sun-beams, concentrated by a burning-glass ; and found that it lost $\frac{1}{4}$ grain of its weight, though much of the minium had not been touch'd by the rays.

(149.) Sept. 6. I took calcined coral, and endeavour'd to calcine it further, by the rays of the sun, in a sealed glass ; and the whiteness of the calx was somewhat increased hereby.

Sept. 10. I exposed the same coral again to the sun-beams, in the same glass hermetically sealed, for two whole hours ; and, then weighing the glass, found it had lost about $\frac{1}{4}$ part of a grain, since it was first sealed.

(150.) May 23. I put calx of tin in a light glass vial, hermetically sealed, and weigh'd it exactly : afterwards I exposed it to the beams of the sun, for a long time, by the help of a large lens ; then the glass, being again weigh'd, seem'd to have lost $\frac{1}{4}$ part of a grain of its weight.

May 29. I repeated the experiment with minium, instead of calx of tin, and the loss of weight came to $\frac{1}{4}$ part of a grain.

May 30. I endeavour'd to calcine the same minium again, but such plenty of air was produced, that the glass broke, with a great noise, into an hundred pieces.

June 6. I made the same experiment again with minium ; and then $\frac{1}{4}$ part of a grain was wanting of the weight.

Attempting again to burn minium, the glass also broke.

July 15. I used wood-coals for the same experiment, but the sun did not at all affect them.

July 20. I exposed Sulphur vivum, to the beams of the sun, in the same manner ; and tho' it was easily melted, and emitted many fumes, yet I found no change at all in the weight.

Aug. 1. I kept the same vial still, with the flower of sulphur ; and often exposed it to the fire of the burning-glass, without danger of being broken ; because sulphur produces no air : the fumes rose, and, at first, the sulphur bubbled ; but the weight remain'd the same.

(151.) Nov. 6. A piece of roasted rabbit, being exactly closed up, in an exhausted receiver, was two months, and some few days after, taken out, without appearing to be corrupted, or sensibly alter'd, in colour, taste, or smell.

(152.) March 11. A small glass receiver, being half fill'd with pieces of white bread, was exhausted, and secured.

April 1. The receiver being open'd, part of the bread was taken out, and appear'd not to have been impair'd in that time ; only the outside, of some pieces of crumb, seem'd to be a very little less soft and white, than before. There appear'd no drops, or the least dew, on the inside of the glass. The remaining bread was, again, secured soon after.

PNEUMATICS.

April 18. The bread was taken out again, and tasted much as it did the last time; the crust being, also, soft, and no drops of water appearing on the inside of the glass.

Milk.

(153.) *March 9.* I open'd a small exhausted, and secured receiver, wherein, about three months ago, we had included some milk, which was well-colour'd, and turn'd, partly, into a kind of whey, and, partly, into a kind of soft curd. The taste was not offensive, only a little sourish, like whey; nor the scent fetid, but somewhat like that of sourish milk.

Violets.

(154.) *March 5.* Violet-leaves, put up, freed and secured from air, being open'd, *April 7.* appear'd not to have chang'd their shape, colour, or consistence; but their odour could not be well judg'd of; because he who included them, had crush'd many of them together, in thrusting them down; since, by such a violation of their texture, 'tis natural for violets to lose their fragrancy, and acquire an earthy smell.

(155.) Having carefully placed some violets in an exhausted receiver, of a convenient size, and bigness, and secur'd it from immediate commerce with the external air; after seven months, we look'd upon them again, and found they were not putrefied, or resolved into any mucilaginous substance, but kept their shape entire; some of them retaining their colour, but more of them having so lost it, as to look like white violets.

Sheep's blood.

(156.) *Nov. 5.* We convey'd into a conveniently shaped receiver, some ounces of sheep's blood, taken from the animal, kill'd that afternoon. After the exhaustion of the air, during which, numerous bubbles were generated, that made the liquor swell considerably; the included blood was kept in a warm place for twenty days; and, during one or two of the first, the blood seem'd to continue fluid, and of a florid colour; but afterwards, degenerated into one, that tended more to blackness.

Nov. 25. We let in the external air; and the glass, containing the blood, being held in a light place, the greatest part of the bottom of it seem'd to be thinly overlaid with a coagulated substance, of a higher colour than what swam above it; which, though it appear'd dark, and almost blackish in the glass, whilst view'd in the bulk, yet, if it was shook, those parts of it that fell down along the inside of the glass, appear'd of a deep fair colour. But, whilst the blood continu'd in the glass, it was suppos'd not to stink; since, even when it was pour'd out, tho' its scent seem'd, to me, somewhat offensive, yet, to others, it seem'd to smell like the blood of a dog, newly kill'd.

Cream.

(157.) *March 17.* Some cream being put up, and secur'd in an exhausted receiver, appear'd, a year after, to be more thick, and almost like butter, at the top, than in other parts; and afterwards, by being well shaken together, in the glass, it was easily enough reduced to butter, whose butter-milk, by the judgment of those who were more used hereto than I, appear'd not different from ordinary butter-milk; and, I found it had, like that, a grateful sourness. The butter was judg'd to be a little sourer than ordinary, but was not, as they speak, made.

(158.) *Feb. 18.* We look'd upon three vials, that had been exhausted, and secured *Sept. 15* last; the one of these had in it some slices of roasted beef, the other some shivers of white bread, and the last some thin pieces of cheese; all which, seem'd to be free from putrefaction, and look'd, much, as when they were first put in; we, therefore, let not the air into the receiver, but left them, as they were, to prolong the experiment. PNEUMATICS.
Beef &c.

(159.) *Feb. 18.* There was a fourth vial, wherein, about six months before, had been inclos'd, and secured some july-flowers, and a rose; yet, these being kept in the same place with the rest, tho' they seem'd a little moist, retain'd their shape and colour, especially the rose, which look'd, as if it had been lately gather'd. We observ'd, in none of these four receivers any great drops, or so much as dew in the parts situated above the included matter. Flowers.

(160.) *June. 4.* We left some strawberries in an exhausted receiver, and coming to look upon them after the beginning of *November*, we found them to be discolour'd, but not alter'd in shape, nor mouldy; we, therefore, left them still in the receiver for further trial. Straw-berries.

(161.) *May 2. 1669.* A piece of roast-beef, secur'd *September 15.* last, appear'd to be not at all alter'd; no more did a piece of cheese, secured in another receiver, and some pieces of a *French* roll, secured, on the same day, in a third. Cheese, &c.

Flowers, seal'd up *August 12. 1668*, being this day look'd upon, appear'd fresh.

(162.) *June 17.* A pint of small beer, being put into a conveniently shaped glass, afterwards exhausted, and secured from the air; the most part of *August* proving extraordinarily hot; towards the latter end, there was, at several times, great thunder, which turn'd the beer in our cellar, and in most of those of the neighbourhood, sour. *Sept. 1.* The beer was open'd, but did not seem to be sour. Small-beer.

(163.) To try, whether the thunder would have such an effect upon ale, exactly stopt in glass vessels, as it often has on it in ordinary casks; I caus'd some ale, moderately strong, to be put into a conveniently shaped receiver, and having exhausted the air, and secur'd the glass vessel, 'twas put into a quiet, but not a cool place. About six weeks after the liquor had been inclos'd, there happen'd some very loud thunder; and our beer, upon this, tho' the cask was kept in a good cellar, being generally noted to have been turn'd sour; I stay'd yet a day or two longer, that the operation upon our included liquor might be the more certain and manifest; and then taking out the ale, found it good, and not at all sour'd. Ale.

(164.) Some black-berries, included in an exhausted receiver, *Sept. 21. 1670.* were open'd *June 20. 1673.* and found free from all mouldiness, and ill scent; only there was some sour liquor, which being taken out, the berries were secur'd again. At the same time, another parcel of the same berries was exactly clos'd up in a receiver, whence the air was not pump'd; but coming, *Octob 11. 1673.* to look upon the glass, we found it crack'd, and the fruit all cover'd with a thick mould. Nor was this the

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only vessel, wherein, trials made to preserve fruits without any exhaustion of the air, miscarried.

Octob. 11. 1674. The former berries *in vacuo*, being look'd upon, appear'd much less black than before; but did not seem putrefy'd, either by loss of shape, or by any stinking smell; nor was the least mouldiness observ'd upon them, tho' they had been kept in the same receiver for above four years.

Ale.

(165.) *June 14.* We put a convenient quantity of ale into a bolt-head, and seal'd it up hermetically; the next year, *July 5.* we broke off the seal, and found the liquor very good, and without any sensible sourness. The next day, it was seal'd up again, and set by for thirteen months; when, the neck of the glass being broken, the ale was found sour. We see, however, that a small quantity of ale was preserv'd good, at least, above a year; which is much longer, than that liquor usually keeps.

Claret.

(166.) *June 14. 1670.* In a large bolt-head, we hermetically seal'd up above a pint of *French claret*, which, when we came to look upon *July 5. 1671.* appear'd very clear and high colour'd, and had deposited a large sediment at the bottom of the glass, but fasten'd no tartar, that we could perceive, to the sides. Upon breaking the seal'd end of the glass, we thought there was an eruption of included air, or steams; and, high above the surface of the wine, there appear'd a certain white smoke, almost like a mist, and then gradually vanish'd: the wine continu'd well tasted, and was a little rough upon the tongue, but not at all sour.

The bolt-head was seal'd up again *July 6. 1671.* and set by, till *August 5. 1672.* at which time it was open'd again, and the wine still tasted very well.

June 26. 1673. The bolt-head, with the same claret, being open'd, was found very good, and seal'd up again. *Octob. 11. 1674.* the same wine was open'd again, and appear'd of a good colour; it was not sour, but seem'd somewhat less spirituous, than other good claret; perhaps, because of the cold weather.

Bodies preserv'd in compress'd liquors, and first apricocks with raisins and water.

(167.) *Aug. 3. 1678.* I included two apricocks in two receivers, one of which was exactly fill'd with bruis'd raisins of the sun, and with water; but in the other, there were only lodg'd a few raisins, so that the apricock was not touch'd, by them, or their moisture.

Sept. 10. I took out the apricock, inclos'd with the water; and, whilst the air broke out, the fruit bubbled very much: the raisins had lost, almost all their taste, but the apricock preserv'd a pleasant relish; and seem'd more pleasant than such fruit usually is at that season of the year.

Feb. 10. 1678. The apricock, inclos'd without water, kept its colour and figure, only seem'd to have lost its firmness.

This experiment informs us, that the taste of some fruits may be preserv'd in an infusion of raisins of the sun; at least in vessels able to resist a great compressure of the air.

Peaches in an infusion of raisins.

(168.) *Sept. 17. 1678.* I included peaches, with an infusion of raisins, in two receivers, shut with a screw.

Sept.

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Sept. 21. Too great a quantity of air produced in one of the receivers, expell'd some part of the liquor. The other receiver retain'd its liquor.

Sept. 25. The receiver, out of which the liquor was expell'd, lost some more of it; so that a fifth, or sixth part, now seem'd empty: but, setting the screw, the liquor was then preserv'd. The other receiver remain'd unalter'd.

Sept. 26. The same receiver began, again, to leak, and run over: I set the screw again.

Nov. 27. Our receiver, hitherto, seem'd to be exactly shut; but now I open'd it; and, whilst the air was getting out, the peaches bubbled very much: one of them, which was of that sort whereto the stone usually adheres, preserv'd its firmness, and a pleasant taste; but the other, being of the yellow-colour'd kind, was very soft; yet the taste thereof seem'd to be more pleasant than of the other. The liquor was very grateful.

Decemb. 28. The other receiver seem'd unalter'd; but, when I open'd it, innumerable bubbles immerg'd from the liquor, and from the peach. The peach, on one side, had preserv'd its firmness; on the other, it had lost it: but the whole was grateful to the palate, tho' somewhat sharp.

This experiment seems to teach, that liquors may grow sour, tho' no spirits have evaporated from them.

(169.) *September 20.* I included peaches, with unripe grapes, in two receivers, and filled them exactly; the one with apples bruised to the consistence of a pultice; and the other, with an infusion of raisins of the sun. *Peaches with grapes, apples, and an infusion of raisins.*

Sept. 25. The receiver, fill'd with pulp of apples, hitherto seem'd unalter'd; but, in the other, the air, which was generated, had thrust out half of the contain'd liquor, and impel'd the mercury into the gage, to the height of 100 inches; wherefore, I open'd the receiver, and the peach, whilst the air got out, was almost reduced to the consistence of a pultice: the taste of it was pleasant.

I put another peach into the same receiver, and substituted a new infusion of raisins of the sun, instead of that which was lost.

Sept. 26. The mercury rose to 30 inches above its usual height.

Sept. 27. The height of the mercury was 72.

28. The height of it was 90. The liquor work'd out.

30. The same height remain'd; but the liquor was all escaped.

October 1. All the air had, also, escaped; wherefore, opening the receiver, I found the peaches very soft, but of a pleasant taste.

Octob. 3. The receiver, filled with the pulp of apples, had lost nothing; but now I perceiv'd, that almost all the juice of the apples had run out: I open'd the receiver, and found its contents very much fermented. The peach was very soft, but not unpleasant in taste.

This experiment informs us, that fruits cannot be long kept in pulp of apples, because of the great production of air; tho' that happens a little later in the infusion of raisins.

(170.) *Sept. 23. 1678.* I included peaches, with crude grapes, in two receivers; one of which was exactly fill'd with pulp of apples, the other with unripe grapes, bruised. *Peaches with grapes, and the pulp of apples.*
Octob.

PNEUMATICS.

Octob. 1. The receiver, fill'd with pulp of apples, seem'd to have suffer'd no alteration; but the other, was empty of liquor: this, therefore, I open'd, and found one of the peaches to have retain'd its firmness and taste; but the other had lost its firmness, yet retained a grateful taste.

Feb. 5. 1679. The receiver, containing the pulp of apples, seem'd unalter'd: I open'd it, and the great ebullition which arose thereupon, manifested, that a great compression of the air was made. The pulp of apples, and the peach, retain'd a grateful taste, but somewhat more pungent than ordinary.

This experiment shews, that juice of crude grapes cannot, conveniently, be used for the preservation of fruits, by reason of the too great production of air.

Pears included with the pulp of apples.

(171.) *Sept. 25. 1678.* I included two pears, called buttter-pears, in a receiver, exactly fill'd with pulp of apples.

Sept. 28. I perceiv'd no alteration in the height of the mercury.

Octob. 5. The mercury was now risen 15 inches.

Octob. 6. The height of the mercury was above 16.

Octob. 12. The mercury was not changed.

Octob. 20. Three days ago, the mercury was depressed, though nothing had escaped.

Octob. 26. This day the receiver was crack'd; though I did not find that the air was compressed within it; but, perhaps, the screw was set too high. The pulp of the apples was of a very grateful taste; so were the pears, tho soft, and one of them inclined to rottenness.

Perhaps, the crack in the receiver, was the cause of so little air being produced in this experiment.

Peaches inclosed with the pulp of apples, and unripe grapes.

(172.) *Octob. 1. 1678.* I inclosed peaches in two receivers; one of which was filled with pulp of apples, and the other with unripe grapes, bruised.

Octob. 5. Much air was produced in the second receiver, and some of the juice ran out. The height of the mercury was 64 inches.

Octob. 6. The juice continu'd to run out: the height of the mercury was 70.

Octob. 8. Now the juice seem'd to be all run out of the receiver; and the height of the mercury was 86.

Octob. 12. The mercury remain'd at 86.

Octob. 18. The receiver, emptied of its juice, held the air very well; and the mercury in it rested at 86. The other receiver, filled with pulp of apples, had, for these five last days, suffer'd some juice to flow out.

Decemb. 4. I open'd the receiver, fill'd with pulp of apples; and tho' all the juice was gone, yet it still retain'd the air, very much compressed; and many bubbles broke out, not without noise, after the receiver was quite open'd. The peach was very soft, and of a pungent taste, like to that of strong wine.

Jan. 22. 1679. After the effusion of the juice in the other receiver, the mercury rested at the same height. I open'd the receiver; the peaches emitted many bubbles, and were wrinkled, but their colour was little changed: their taste was most pungent, and inclining to acid.

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This experiment confirms the conclusions drawn from the former.
(173.) Octob. 4. 1678. I put peaches into three receivers; the first of which was filled with ale; the second, with hopp'd beer; the third, with wine.

Peaches included with ale, beer, and wine.

Octob. 5. The height of the mercury, in the first receiver, was 15; in the second, 10; in the third, 9.

Octob. 6. The height of it, in the first receiver, was 25; in the second, 15; in the third, 20.

Octob. 8. The height of the mercury, in the first receiver, was 35; in the second, 15; in the third, 20.

Octob. 12. The height in the first receiver, was 63; in the second, 15; in the third, 28.

15. The height of the mercury, in the first receiver, was 81; in the second, 15; in the third, 30.

16. There was no more change perceived in any of the three receivers.

18. The mercury rather descended, than ascended in all the three.
22. In the wine, only, the mercury ascended, or descended, according to the degrees of heat and cold.

24. The height of the mercury, in the first receiver, was 96; in the second, 15; in the third, 30.

30. The height, in the first receiver, was 115; in the second, 20; in the third, 30.

Nov. 3. The height, in the first receiver, was 117; in the second, 20; in the third, 30.

6. The height, in the first receiver, was 120; in the second, 31; in the third, 31.

11. The height of the mercury, in the first receiver, was 105; in the second, 31; in the third, 28.

The weather was cold.

Nov. 16. The height of the mercury was the same. The peach, which hitherto lay at the bottom, now mounted to the upper part of the liquor, in the second receiver; the rest staid at the bottom.

Nov. 25. The height, in the first receiver, was 140 inches; in the second, 47; in the third, 32.

Nov. 28. The height, in the first receiver, was 96; in the second, 36; in the third, 28. It was very cold weather.

Decemb. 13. The height, in the first receiver, was 96; in the second, 47; in the third, 33. I open'd the third receiver, and found the peach firm, and of a laudable colour; but it had contracted much of its taste from the wine, and might yet be improved by sugar. The wine, also, was grateful to the palate.

Decemb. 30. The height of the mercury, in the first receiver, was 96 inches; in the second, 47. I open'd the first receiver; when, the peaches, which had lain, till then, at the bottom of the liquor, presently emerg'd to the upper part, and emitted many bubbles: the taste of the ale, of which they had greatly partook, became pleasant, with sugar. Hence

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Hence fermented liquors may be useful for the preservation of fruits, as being unfit to produce air.

(174.) *Sept. 5. 1678.* I included one whole peach, with another cut to pieces, in a receiver; into which, I afterwards poured old wine, till it was exactly fill'd, and then shut it with a screw.

Nov. 20. Nothing, hitherto, seem'd to be alter'd; but, this day, I perceiv'd some of the wine run out.

Nov. 30. A third part of the wine was lost.

Decemb. 8. The wine beginning again to run out, and there being but little of it left, I open'd the receiver, and found the peaches very much fermented, yet of a grateful, but most pungent taste. The wine, also, was pleasant.

From this experiment, compared with the third receiver, in the former, we may conjecture, that wine hinders the fermentation of peaches, if used in a sufficient quantity; but here the quantity was not sufficient, because the pieces of the cut peach fill'd the whole receiver, so that no room was left for the wine, but in the interstices.

(175.) *Octob. 11. 1678.* I put two unripe peaches, one whole, the other cut to pieces, into a receiver fill'd with hopp'd and fermented beer.

Octob. 12. In one night's time, the mercury ascended three inches.

Octob. 15. The height of the mercury was 15.

16. The height of it was 15.

18. The height of it 12. It was very cold.

20. The height of it remained at 12.

22. The mercury ascended again. The cold abated.

Nov. 2. The height of the mercury was 20.

3. The mercury descended a little. It was cold weather.

6. The height of the mercury was 28. The weather grew hotter.

8. The height of it was 33.

11. The height of the mercury was 40.

12. The height remained at 40. Some of the beer work'd out.

16. The height of it was 46.

19. The height of it was 43. But much of the beer was lost.

21. The mercury ascended not, but the beer continued to work out.

23. When the beer was almost all work'd out, I open'd the receiver, and found the peaches very soft, yet of a grateful taste; tho' they were kept for 9 hours in the free air, after the receiver was open'd.

From this experiment, compared with the second receiver, we may infer, that beer hinders the fermentation of peaches, and the production of air, if used in a sufficient quantity: but here there was only a little beer, contain'd in the interstices, which was unable to hinder the fermentation of the peaches.

(176.) *Octob. 19. 1678.* I included raw beef in three receivers; the first of which was exactly fill'd with stale beer, forcibly intruded; so that the mer-

Raw beef included with stale beer, and common air.

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mercury exceeded its wonted height by sixty inches. The second was, also, exactly fill'd with stale beer, but here there was no compressure made. The third was fill'd, partly with the beef, and partly with common air.

Octob. 20. In the first receiver, the mercury was depress'd to twenty inches below its usual height; tho' nothing at all had escaped out. In the second, also, it descended; but in the third, it ascended a little.

Octob. 26. In the first receiver, the mercury sometimes ascended, and then descended, very irregularly; in the second, it began to ascend slowly, two days ago; in the third, it was not mov'd at all.

Octob. 27. A piece of the same beef, which was left in the air, began to smell ill; and the mercury in the third receiver, began to ascend; in the second, it continu'd to ascend gradually; but in the first, it seem'd rather to descend.

Nov. 3. The mercury in the first receiver ascended not; in the second, the height of it was twenty inches; in the third, ten.

Nov. 5. I open'd all the receivers, and the two first had no offensive smell, only contracted a scent from the beer. The flesh boil'd in the same beer was very tender, but its taste was bitter; perhaps, by reason of the too great quantity of beer. The beef included with common air, presently smelt fetid, upon being open'd; yet, when taken out, and applied to the nose, it scarce seem'd to stink. I included the same flesh in the same receiver, to try whether new air being admitted, would promote corruption.

Nov. 6. The height of the mercury was three inches.

Nov. 11. The height of it was nine.

Nov. 25. The height of it was twenty.

I open'd the receiver, and found the flesh so fetid, that I was forc'd to throw it away.

From hence it seems to follow, that beer may help to preserve flesh, especially if it be forcibly intruded into the receiver; but this compressure is soon abated, because the air, compress'd in the same receiver, is apt to enter into, and gradually pervade the pores of the beer.

(177.) *Nov. 12.* I included beef, press'd together as close as I was able, in three receivers: into the first of them I pour'd water, mix'd with one fortieth part of salt, which fill'd up all the interstices, left betwixt the parts of the flesh; the second, in like manner, contain'd some salt water; but it was so forcibly intruded, that the mercury in the gage ascended fifteen inches, above its wonted height: into the third receiver, I pour'd no water, and therefore those few interstices, which could not be possess'd by the flesh, were left for the air.

Beef included with salt-water, and common air.

Nov. 13. The mercury descended in all the receivers, especially in the second, wherein was the compress'd liquor.

Nov. 18. The two receivers, which were uncompress'd, did not drive the depress'd mercury upward: but that, whose mercury had been impell'd to fifteen inches, and afterwards had descended most, now return'd almost to its former height. A piece of the same beef, being left in the air, began to smell ill.

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Nov. 23. In all three receivers, air was produced a-new; but to-day the mercury, in the second, descended three inches, and the height of it was twenty; in the other two 'twas about sixteen. I open'd the first receiver, and the flesh was not at all corrupted.

Nov. 30. I took that flesh out of the receiver, which was put in without salt, and it did not stink at all; but, being boil'd, was very tender, and of a pleasant taste.

Decemb. 6. I open'd the receiver, into which I had forcibly introduced salt water. The mercury exceeded its wonted height by twenty-five inches. The flesh smelt strong, yet did not stink: that *in vacuo* yielded many bubbles, which ceas'd not, till a pretty while after the receiver, in which it was included, was taken from the pneumatic engine; then mercury, in one hour's time, came to the height of three or four inches. I, afterwards, immers'd the same receiver so exhausted, in hot water; and the liquor, contain'd therein, bubbled very much, tho' the water, from which it borrow'd all its heat, did not boil; but so great a quantity of air was produc'd, or had enter'd from without, that the receiver was quickly full. The liquor, contain'd therein, did not, afterwards, bubble, or boil, tho' it were immerg'd in boiling water. I took out the flesh, and found it pleasant and tender, yet less so, than I expected; perhaps, because it was not boil'd enough.

Hence, water, as well as beer, may conduce to the preservation of flesh.

Oysters with
their shells, and
without, inclu-
ded in salt-wa-
ter, common air,
and in *vacuo*.

(178.) Nov. 29. 1678. I inclos'd oysters in four receivers: in the first, the oysters were without their shells, and exactly fill'd the whole space; in the second, the oysters, with their shells, were included with common air; in the third, the oysters also were included in their shells; the remaining space of the receiver, being exactly fill'd with salt-water. These three vessels were firmly clos'd with screws. The fourth receiver was exhausted of air, and contain'd three oysters in their shells, and eight taken out of their shells. When the air was pump'd out of this receiver, the oysters freed from their shells, emitted many large bubbles; but the three others suffer'd no sensible change, only one of them gaped.

Nov. 30. In the three receivers, stopp'd with screws, air seem'd to be consumed, rather than produced; but the mercury *in vacuo* ascended a little.

Decemb. 4. Whilst the weather was cold, the mercury ascended not; but now, when the cold began to abate, the height of it in the first receiver was seven inches; in the second, none; in the third, three; and in the fourth, three.

Decemb. 5. The height of the mercury in the first receiver was twenty inches; in the second, one; in the third, three; in the fourth, five.

Decemb. 7. The height of the mercury in the first receiver was thirty inches; in the second, one; in the third, three; in the fourth, eight. Other oysters, left, at the same time, in the air, smelt ill.

Decemb. 9. In the first receiver, the height was thirty; in the fourth, eleven. The rest were not chang'd.

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Decemb. 13. There was no change in the three first receivers ; but in the fourth, the height was fourteen inches.

Decemb. 20. In the first receiver, the height was forty-six ; in the fourth, twenty-four ; the rest were not chang'd.

Decemb. 21. In the first receiver, the height was fifty-two ; in the fourth, twenty-five ; in the rest, no change.

Decemb. 22. The height of the mercury in the first receiver was sixty ; in the fourth, twenty-seven ; no change in the rest.

Decemb. 27. In the fourth receiver, the height was twenty-nine ; the rest were not chang'd.

Jan. 1. 1679. The oysters in the third receiver, had ting'd the water black.

Jan. 25. The mercury *in vacuo* seem'd still to remain, almost, at the same height. But this day, some bubbles were form'd in the turpentine, by the internal air, about the juncture of the cover with the receiver. I, therefore, open'd the receiver, and found the oysters very fetid. I, likewise, open'd the other receivers, and found the oysters of an ill scent, and turn'd to a kind of viscid gelly.

This experiment seems to inform us, that fish produce less air than flesh ; yet will be corrupted, tho' defended against the air.

(179.) *Nov. 29. 1678.* I exactly fill'd a glass vessel, with fresh and unsalted butter ; then stop'd it with a screw. A mercurial gage was included in the same vessel. Butter included in a receiver.

Nov. 30. In the night, the cold being very sharp, the butter was condens'd ; for the mercury approach'd nearer to the aperture of its gage.

Decemb. 2. The mercury came still nearer to the aperture of its gage ; perhaps, because the cold daily increas'd.

Decemb. 5. The cold being abated, the mercury return'd almost to its former height.

Part of the same butter, being left in the air, began to have a very bad smell.

Decemb. 7. The cold returning, the mercury, again, came to the top of its gage. The butter left in the air, smelt worse than before, tho' it was still edible.

Decemb. 24. The butter had produced no air ; being taken out of the receiver, it was of a grateful taste, except, only, a little of the superficies, which lay contiguous to the leather spread over the cover.

It follows, that butter may be kept a great while, if it be defended from the external air.

(181.) *Nov. 30. 1678.* I fill'd two receivers with whittings ; and that no air might be left in the vacant spaces, into the one I pour'd wine ; and into the other, oysters, with their juice ; so that both receivers were exactly fill'd. When I had afterwards clos'd their covers with screws, the air in the mercurial gages was compress'd ; but in three hours space the mercury again return'd to its former mark. Whittings and wine, and whittings and oysters, included in receivers.

Decemb. 2. The cold increasing, the mercury came nearer to the aperture of its gage in both receivers.

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Decemb. 4. The cold ceasing, the mercury ascended very much in that receiver wherein the oysters were; but, in the other, it moved not.

Decemb. 5. In the receiver, containing the oysters, the height of the mercury was 20 inches; but, in the other, it was not yet return'd to its usual height.

Decemb. 7. In the receiver with oysters, the height of the mercury was 40; in the other, it continued still below its standard.

Decemb. 9. The mercury, in both receivers, was changed little or nothing.

Decemb. 20. When the mercury alter'd no more, I open'd the receivers, and both of them were very fetid. It here seem'd new to me, that the receiver, in which the wine was, had admitted of corruption, without producing air; for, hitherto, all bodies, whilst they were corrupting, had produced some.

Beef with spice,
included in re-
ceivers.

(181.) Decemb. 3. 1678. I put raw beef into two large receivers, with pepper and cloves; and that no air might be left in the interfices, I pour'd beer upon them; and, in no long time after, found the pressure of the air, in the receivers, to be abated; the mercury, in the gages, coming to the open ends.

Decemb. 8. The mercury ascended not in either of the receivers. I open'd the one, that I might boil the flesh; which had contracted a sweet scent from the cloves; and the liquor, contain'd in the same receiver, before it was boil'd, smell'd like hippocras.

Jan. 2. 1679. I open'd the other receiver, and found no air produced therein: the flesh was not at all corrupted; and, when I boil'd it *in vacuo*, I observ'd, that if a more intense fire were made, the air, or some spirits, broke thro the stop-cock, which was fasten'd to the top of the receiver. The receiver, being cooled, all the night, was, the day after, found, almost, quite empty of air. The flesh was very tender, and well tasted, only it was a little over-boil'd; for it had been kept on the fire full six hours.

Hence we have a confirmation, that beer may be useful to preserve flesh, especially if the bitter taste thereof be corrected by aromatics.

Larks, with
beef and ale, in-
cluded in a re-
ceiver.

(182.) Decemb. 4. 1678. I included two larks, with some beef, in a receiver, and fill'd all the spaces, unpossess'd by the flesh, with ale; at the same time, I fill'd another receiver, with the same sort of beef, adding beer, also, but no larks.

Decemb. 9. Some pieces, cut off from the larks, and exposed to the air, began to smell ill; but those included in the receiver, had produced little air; for the mercury was not yet come five inches above its wonted height. In the other receiver it was not moved.

Decemb. 19. In the receiver, which contained the larks, the mercury ascended no higher; for the cover being broken, suffer'd the liquor to run out. Wherefore, I open'd the receiver, and boil'd both the beef and the larks, which were not at all corrupted, but very grateful to the palate. The beef had contracted a pleasant taste; partly from the larks, and partly from the beer.

Decemb,

Decemb. 23. I open'd the other receiver, and the flesh being boiled, seem'd pleasant; yet not so pleasant as that which received a venison-like taste from the larks. PNEUMATICA.

Hence birds may be long preserv'd by the help of beer, or ale.

(183.) *December 14.* I included apples in four receivers: in the first was a whole apple, and all the interstices were fill'd with powder'd sugar: in the second, was an apple cut in pieces, and the spaces fill'd with sugar, as before: in the third, was, also, an apple, cut; but the rest of the receiver was fill'd with water, wherewith a tenth part of sugar had been mixed: in the fourth, the apple was also cut, and the spaces fill'd with a solution of one part sugar, and five of water. Apples included in receivers.

Decemb. 21. In the first receiver, the mercury began to ascend a little, yet the sugar did not dissolve; in the second receiver, all the sugar was melted, and the pieces of apple were shrivel'd: they produced much air, when first put into the receiver. In the two other receivers, the mercury began, also, to ascend; but, in the third, the pieces of apple were very much corrupted, their skin being taken off.

Decemb. 22. Air was produced in all the receivers; but the quantities did not bear the same proportion amongst themselves, as the quantities of the sugar: for, in the second receiver, much air was produced; but, in the fourth, the mercury ascended less than in the third. Some air was, also, generated in the first.

Decemb. 27. In the three first receivers, the height of the mercury was ten inches; but in the fourth, only six.

Decemb. 31. In the first and second receiver, the height of the mercury was 13; in the third, 15; in the fourth, only 9.

Jan. 2. 1679. In the first and second receiver, the height of the mercury was almost 14; in the third, 17; in the fourth, 11.

Jan. 7. In the second, the height of the mercury was 16; in the third, 36; in the fourth, 15; but, in the first, the mercury had not ascended, and something had escaped out of the receiver: I, therefore, eas'd the screw, that I might dispose it the better, and then the air made an escape.

Jan. 9. In the first receiver, the height was six inches; in the second 16; in the third, 39; in the fourth, 15.

Jan. 17. In the first receiver, the height was 13; in the second, 19; in the third, 56; in the fourth, 17.

Jan. 30. In the third receiver, the height of the mercury was 76 inches, and the liquor got out; I, therefore, open'd it, and found the fruit to have lost much of its taste; but the water had contracted it, and was pleasant to the palate. In the second receiver, the mercury ascended no more. I open'd this, also, and found the fruit much more pleasant than the other; yet much of its taste was imparted to the sugar, which was turn'd into a very good syrup.

Feb. 16. The height of the mercury, in the first receiver, was 22 inches; but, in the fourth, 33. This I open'd, and found the fruit to have

PNEUMATICS have lost much of its taste; and that the ambient water had got it, and was thereby turn'd into a pleasant drink.

Feb. 27. In the first receiver, the height of the mercury was thirty nches.

March 15. In the first receiver, the height of the mercury was not changed; but, now, something escaped out of the receiver: I open'd it, and found the apple of a laudable colour; but the pulp was spongy, and had lost much of its taste.

This experiment seems to teach, that sugar is not so fit to preserve fruits, as fermented liquors.

A lark included
with milk.

(184.) *December 23.* I fill'd a glass vessel with milk, then stopp'd it with a screw; and, into another receiver, I put a lark with milk, and stopp'd it close.

Decemb. 24. This evening I perceiv'd, that the caseous part was separated from the butyrous, in the closed receivers; as well as in the milk, which, at the same time, I left exposed to the air.

Decemb. 27. I found no air produced in the receiver which held the lark; but, in the other, the mercurial gage was spoiled.

Decemb. 31. The mercury ascended in that receiver which contain'd the lark; but the milk left in the air, at the same time that I stopp'd the receivers, stunk three days ago.

Jan. 1. In the receiver which held the lark, the height of the mercury was ten inches.

Jan. 2. The height of the mercury was $14 \frac{1}{2}$. The milk stagnant below the butyrous part, appear'd of a red colour.

Jan. 4. The height of the mercury was 19. Some white sediment was concreted at the bottom of the milk.

Jan. 9. The height of the mercury was 29 inches.

Jan. 25. I open'd both receivers: the lark smelt only strong, tho' it had been kept 32 days; when boil'd, it was of a pleasant taste. In the other receiver, the caseous part of the milk was sub-acid, and grateful; but the butyrous part was not sour at all.

This experiment informs us, that, sometimes, milk may be successfully used to preserve flesh.

A lark included
in a receiver
with butter.

(185.) *Decemb. 24. 1678.* I put a lark into a small receiver, and pour'd butter upon it, melted over a slow fire, till all the interstices were exactly fill'd; then I closed the cover with a screw.

Decemb. 27. The mercury approached nearer to the aperture of its gage. The butter seem'd to be alter'd; for the lowest part of it was yellower, and the middle whiter than before. The upper-part was fluid.

Jan. 5. 1679. The mercury return'd, by degrees, to its wonted height.

Jan. 9. The mercury was somewhat higher.

Jan. 28. The mercury was little changed. I open'd the receiver, and found that part of the butter, contiguous to the leather, spread over the cover, to be white, and of a very unpleasant taste. The butter, more remote from the leather, was yellow, and something fetid, tho' edible. But the

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the lark being roasted, was grateful to the palate, tho' it had been kept 34 days. This experiment seems to shew, that hot melted butter is not very successfully used to preserve flesh. PNEUMATICS

(186.) Jan. 4. 1679. I included boil'd flesh in an exhausted receiver, stopp'd with a screw; and fill'd the interstices, exactly, with broth of the same flesh, which seem'd a little too salt. Whilst I set the screw, all things in the receiver were compress'd; and the mercury ascended to the height of six inches into the gage; but it soon return'd to its wonted height. Boiled flesh in vacuum.

Jan. 28. The air was, gradually, more consumed, so that the mercury now descended eight inches below its usual standard. I open'd the receiver, and found the flesh very sweet and tender. The broth, also, had an acidish, but a very grateful taste.

This experiment shews, that boil'd flesh may be long preserv'd good; which is a great convenience at sea, where, perhaps, there might be no occasion for salt meat. For, after raw flesh hath been included in screw'd vessels, as long as experience shews there is no danger of its corrupting, it may be taken out, and, being perfectly boil'd, be again included in the same receivers; and so, doubtless, it may be kept for a great while without salt.

(187.) Jan. 30. 1679. I put raw flesh into two receivers; to the first, I added pepper and cloves; in the second, I mixed nothing. Raw flesh included, with and without Spice.

Feb. 11. The height of the mercury, in the first receiver, was three inches; in the second below $1\frac{1}{2}$.

Feb. 12. The height of the mercury, in the first receiver, was $4\frac{1}{2}$; in the second, not above $1\frac{1}{2}$.

Feb. 13. In the first receiver, the height of the mercury was above six inches; in the second, three. I boil'd the flesh of the first receiver, and it was very pleasant, and tender.

Feb. 14. The height of the mercury, in the second receiver, was five.

Feb. 19. The height of the mercury, in the second receiver, was eight.

Feb. 20. The height of the mercury, in the second receiver, was 11. I boil'd the flesh, and found it very tender, tho' it remain'd over the fire in *Balneo Mariae*, only for three quarters of an hour. I put some part of this flesh, before it was boil'd, into a receiver, and filled all the vacuities, as exactly as I could, with the same flesh, to try how long the flesh might be preserv'd, when the air was thus excluded.

Feb. 28. The mercury ascended very little.

March 20. The height of the mercury was about 16 inches. I open'd the receiver, and the flesh seem'd of a pleasant taste, yet inclining to corruption.

(188.) February 10. I put raw beef into three receivers: in the first, the beef was season'd with pepper and cloves; in the second, it was encompass'd with salt-water; in the third, I put neither salt nor spice. Raw beef included with Spice, and a-part, with salt-water.

Feb. 19. Four days ago, the mercury ascended in the third receiver; in the first, also, it began to ascend; but, in the second, not at all.

Feb.

Feb. 21. In the first receiver, the height of the mercury was four inches and a half; in the third, ten; but in the second, there was no ascent at all.

Feb. 25. The height of the mercury in the first receiver was six; in the third, nineteen; in the second half an inch.

Feb. 26. This night, there was no ascent of the mercury in any of the receivers. I open'd the third, and the flesh, after boiling, was very good.

By the former experiment, spices seem to hinder the production of air; but the present experiment proves the contrary. Whence this contrariety should proceed, I know not; unless, perhaps, because, I had left a space large enough for the air in these receivers; but in the former experiment, fill'd all as exactly as I could with flesh.

March 9. The height of the mercury, in the first receiver, was eight inches; in the second, none.

March 12. The height of the mercury in the first receiver was twelve; in the second, one.

April 3. The height of the mercury in the first receiver was eleven; but in the second it exceeded not, one. I open'd the receiver, and boiling the flesh, found it very tender, and of an excellent taste.

Hence the saltness of water, included with flesh, seems to hinder the production of air; but there being so small a quantity of water, compar'd with the quantity of flesh, I rather incline to think, that less air was produced in the second receiver, because it was more exactly fill'd. And, indeed, fresh water being used instead of salt, has the same effect; but the chief art to preserve flesh without salt, consists in excluding all air from it, and making a great compressure in the receiver.

These experiments, about the preservation of aliment, may be very useful in transporting fruits, venison, &c. from remote places, and towards affording better nourishment to mariners.

Boiling and distillation practised in vacuum, and first, beef boiled in an exhausted receiver.

(189.) *Decemb. 12. 1678.* I put two ounces, and six drams of beef into an exhausted receiver, able to hold twenty-two ounces of water; then I left it in boiling water for three hours; which done, I expos'd it to the air, to cool for a whole night: afterwards, using my pneumatic engine, I perceiv'd, that the air, formed in the receiver, could scarce sustain three inches of mercury: whence flesh in boiling, cannot form air enough to make an entire pressure in a receiver, capable of holding a double weight of water: that is, if you include one pound of flesh in an exhausted receiver, able to hold two pounds of water, it will not generate air enough to remove the cover from the receiver, unless heat greatly contribute to produce the effect: but, our flesh, I confess, was not boil'd enough.

(190.) *Decemb. 23.* I inclos'd three ounces of raw beef in a receiver able to hold thirty-two ounces of water; and in boiling, after it had been long on the fire, the cover was forc'd from the receiver, and so suffer'd the vapours to pass out: but being presently shut again, and the fire remov'd, the receiver soon lost its internal pressure; so that being re-plac'd on the fire, it was a long time before it could force away the cover a second time. I tried this

this again, and again; and unless the receiver had been expos'd to a very strong fire, the cover would never have been remov'd; but if the fire burns well, sweet exhalations continually pass out. PNEUMATICS.

Decemb. 24. The receiver having been cool'd, during the whole night, was, this day, by the use of the pneumatic engine, almost wholly evacuated. Whence we seem to have a confirmation, that the divulsion of the cover is not made by that air, which can keep the form of air, but from the steams exhaling from the flesh, and subsiding again therein; provided they be kept in, as they easily may, if we use not too fierce a fire to the evacuated receiver, whereby the loss of those sweet vapours may be prevented.

(191.) Jan. 21. 1679. I put paste, without leaven, into an exhausted receiver; and included another part of the same paste in a second receiver, full of common air. I inclos'd these two receivers in *Balneo Maria*, stopp'd with a screw; and when they had remain'd there, for three hours, expos'd to a moderate fire, I open'd the receivers: the paste in *vacuo* I found reddish on the superficies; but the other had admitted water; and the paste was not boil'd enough: and, therefore, I put both receivers again in *Balneo Maria*, where they staid a whole night. Paste boil'd in *vacuo*, and in common air.

Jan. 22. This morning, I found the *Balneum Maria* quite cold; and the paste, when taken out, was boil'd enough, but cover'd with no crust. That which I included in *vacuo*, was interspers'd with many cavities, but it seem'd too insipid; the other had no cavities, but a more pleasant taste. Both the receivers were found almost wholly empty'd of air.

(192.) Feb. 3. 1679. I inclos'd leaven'd paste in *vacuo*, and, as soon as it had fill'd its receiver with factitious air, transmitted it into the receiver; used to boil flesh in *Balneo Maria*; but, when the paste was thus remov'd, it pitch'd much; yet, when it had remain'd for three hours in a hot *Balneum Maria*, the bread made of it was interspers'd with many cavities, but cover'd with no crust. Leaven'd paste boil'd in *Balneo Maria*, after it had yielded its air in *vacuo*.

Feb. 5. I repeated the experiment, but now the paste was included in *vacuo*, in the same receiver, which was afterwards put in *Balneo Maria*; and therefore, there was no need to remove the paste, and expose it to the air. Hence, the bread made thereof, was much lighter than the former.

(193.) Feb. 12. I included rosemary, with water, in the distilling vessel; and, when the air was pump'd out, I put the vessel in *Balneo Maria*, and there came over a water of a very sweet smell, and some drops of essential oil of a very sweet scent, and not empyreumatical. But when I open'd the stop-cock, to let in the air, the noise so soon ceas'd, that I judg'd much air was produced from the rosemary. Rosemary and water distill'd in *vacuo*.

Feb. 13. I put the same rosemary into the same evacuated vessel, and administred a more intense fire, yet could extract no oil, sweet, or fetid; and the water was less fragrant than the former.

(194.) Feb. 10. 1679. I boil'd one pound of flesh in *vacuo*, in a vessel describ'd, which would contain almost four pounds of water: its upper part, which was made of glass, held the mercurial gage; by the help whereof, I perceiv'd, that the mercury ascended not three inches, tho' Flesh boil'd in *vacuo*.

PNEUMATIC. the flesh had boil'd for three hours, and more. It was not boil'd enough, and its taste was ungrateful: the liquor, form'd of the condens'd vapours, had, also, an unpleasant taste.

Feb. 11. I repeated the experiment, but now sprinkled the flesh with pepper and cloves: the mercury ascended to the height of six inches, tho' the flesh boil'd no longer than the other: it seem'd very grateful to the palate; and the liquor, form'd from the vapours, had a most pungent taste of pepper; but contracted nothing ungrateful from the flesh, as in the former experiment.

From these experiments, made *in vacuo*, it seems, that such vessels may be very useful for distilling, and boiling of such bodies, as contain thin, and very volatile spirits: for every thing will here be preserv'd, and nothing be suffer'd to fly away.

Boiling in
screw'd vessels or
digestors; and
first, beef and
water boil'd in
Balneo Mariae.

(195.) **Jan. 29.** Eight days ago, I fill'd a screw-vessel, with beef and water together; and when it had continu'd over a moderate fire for eight or nine hours in *Balneo Mariae*, stopp'd also with a screw, I took the flesh out; but it was boil'd a great deal too much, and the taste of it was very unpleasant. I boil'd other beef in the same vessel, after the same manner; only this was season'd with pepper and cloves, and remain'd expos'd to the fire but for three hours. This flesh preserv'd a most pleasant taste. I boil'd other flesh, without spices, for three hours, in the same vessel, and after the same manner: when the flesh was taken out, it tasted well; whence I conjectur'd, that what spoil'd the first flesh, was over-boiling: yet the spices may be convenient to correct some part of the ungrateful taste; for I left a place to condense the vapours, in the top of the vessel, and found, that the liquor, there formed, had an unpleasant taste; but not so when the flesh was season'd with pepper and cloves.

Apples boil'd in
a screw-vessel.

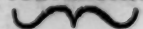
(196.) **Jan. 29.** I boil'd apples, after the same manner as I did the flesh, before mention'd; but mix'd no water with them. They were set upon a moderate fire, for almost two hours. They were very soft, and of a very good taste; but some pieces, which lay in the upper part of the receiver, where the vapours ascending from the lower part condens'd, were of an unpleasant taste; and the drops, form'd from the same vapours, had an ungrateful scent.

Flesh season'd
with spice boil'd
in a screw-vessel.

(197.) **February 4.** I inclosed flesh, with pepper and cloves, in a receiver stopp'd with a screw, but used no water to fill up the interstices; only compressed the flesh as much as I could, and then put the receiver in *Balneo Mariae*, already hot, and stopp'd it with a screw: when it had remained there, over a moderate fire, for an hour, the flesh was rather over-boil'd than under; but, when I open'd the *Balneum Mariae*, all the water burst out of it, with a great force; the liquor being hot, and now finding vent.

Feb. 5. I inclosed some part of this flesh in a receiver, stopp'd with a screw.

March 12. The flesh included five weeks ago, was, this day, found very good. I do not doubt, but that perfect boiling contributed something to its



its preservation: for I find, by experiments made upon other bodies, that boiling, the more perfect it is, hinders fermentation the more.

(198.) February 10. I boil'd a cow-heel, after the same manner as I had done the flesh above-mention'd; but left it, for four hours, or more, upon a moderate fire: then, the vessels being unstopp'd, we found the flesh excellently well boiled, and the bones so soft, that they might be easily cut with a knife, and eaten.

A cow-heel boil'd till the bones were tender.

Feb. 12. I repeated the experiment, and let the vessels remain exposed to the fire for twelve hours; and tho' the water of the *Balneum Maria* every where secured the vessel immersed in it, yet the flesh had contracted a very empyreumatical taste and smell; but the juice, which, in the former experiment, concreted into a very firm gelly, did not here congeal at all.

Hence it appears, that many bones, and hard tendons, which we daily throw away as unprofitable, may, by the help of a *Balneum Maria*, stopp'd with a screw, be converted into good nourishment.

(199.) February 10. I boil'd a fish, after the same manner, in a screw'd *Balneum Maria*, but mix'd no water therewith. The fish remain'd upon the fire for two hours only; when, the vessel being cool'd and open'd, it was found of a very good taste; and its bones were so soft, that they yielded to the pressure of the finger; and the head of it might be eaten like its flesh. The juice of it, in a short time, concreted into a gelly of a hard consistence.

A fish boil'd in a screw'd *Balneum Maria*.

This method is useful for boiling such fish as are very bony.

(200.) February 15. I put hart's-horn into a receiver, to be stopp'd with a screw, and fill'd the interstices with water; I included the receiver, thus stopp'd, in a screw'd *Balneum Maria*, and so exposed it, for four hours, to a moderate fire: the vessels being open'd, the hart's-horn was found soft, and the juice soon concreted into a very firm gelly.

Hart's-horn boil'd soft.

Feb. 17. I repeated the experiment, but no water was included with the hart's-horn, and the fire lasted six hours under the *Balneum Maria*; after this, the hart's-horn was found very soft; but a little juice had sweat out of it, and adhered to the external parts of the hart's-horn, like drops of gelly.

The excellency of such a *Balneum Maria* appears from this experiment; for since even hart's-horn can be boil'd by means thereof, without water, all the fresh water, usually consumed in boiling flesh at sea, may be preserved for other uses.

A
D E F E N C E
OF THE
Physico-Mechanical Experiments,
AGAINST

The Objections of FRANC. LINUS; his Hypothesis
examined, and his Answers to particular Experi-
ments consider'd.

*The objections a-
gainst the air's
spring examin'd.*

OUR author confesses, that the air hath both a spring and weight; but denies that spring to be great enough to perform what I ascribe thereto; and, particularly, labours to prove it unable, in a close place, to sustain the mercury in the *Torricellian* experiment. For, says he, "if a tube, only twenty inches long, be not entirely fill'd with quick-silver, but a small space be left betwixt it, and the finger that closes the upper end, with nothing but air there; and the tube be open'd at the bottom, the finger will not only be drawn downwards; but the quick-silver will descend, considerably; that is, as far as so small a parcel of air can be stretch'd by the descending weight; and, therefore, if, instead of air, any other liquor, not so easily extended, be here used, the quick-silver will not fall: but, if the external air cannot sustain twenty inches of mercury, how should it support twenty-nine and a half?" But to this argument, he has himself furnish'd us with an answer in these words. "But, you'll say that the mercury descends, because 'tis impell'd downwards, by the air dilating itself by its own spring." Which I think sufficient for the objection, notwithstanding the two exceptions he makes to it.

For, first, when he says, that then "the finger ought rather to be repell'd from, than fix'd to the tube, since the expansion is made every way;" he considers not, that tho' the included air extends itself at first, every way, yet the expansion, in our case, must necessarily be made downward; because, the finger that stops the tube, being expos'd on the upper parts, and the sides, to the external air, has the whole weight and pressure of the atmosphere

sphere upon it; and, consequently, cannot be thrust away, but by a force, able to surmount that pressure; whilst, on the lower side of the included air, there is the weight of the whole mercurial cylinder to assist the spring of the air to surmount the weight of the atmosphere, that gravitates upon the stagnant mercury. So that the air included, endeavouring to expand itself, finding no resistance upwards, and a considerable one downward, it is very natural, that it should expand itself that way, where it finds the least resistance: as will happen, till the spring of the air be so far weaken'd by expansion, that its pressure, together with the weight of the mercury, that remains suspended, will but balance the pressure of the outward air upon the stagnant mercury. And, if, instead of quick-silver, you employ water, and leave, as before, in the tube an inch of air, and then inverting it, open it under water, the included inch of air will not dilate itself near half so far, as it did when the tube was almost fill'd with mercury; because, the weight of so short a cylinder of water does but equal that of between an inch and an inch and half of quick-silver; and, consequently the internal air is far less assisted to dilate itself, and surmount the pressure of the outward, by the cylinder of water, than by that of mercury.

As for what our author says, that, "if, instead of air, or water, some other liquor be left at the top of the tube, the quick-silver will not descend;" we can readily solve that phenomenon, since water has either no spring at all, or but an exceeding weak one; and so scarce presses, but by its weight, which, in so short a cylinder, is inconsiderable.

Hence we see, why the finger is so strongly fasten'd to the upper orifice of the tube it stops: for the included air, being so far dilated, that an inch, for example, left, at first, in the upper part, reaches twice or thrice as far, as before the descent of the quick-silver, its spring must be proportionably weaken'd; and, consequently, that part of the finger within the tube will sustain much less pressure it, from the dilated internal air, than the upper part of the same finger, from the unrarify'd air without. By which means, the pulp of the finger will be thrust in.

Our author's second objection runs thus. "If you take a tube, open at both ends, of a considerable length, suppose forty inches, fill it with mercury, place your finger on the top, and open the lower end; the mercury will descend to its wonted station, and your finger, on the top, be strongly drawn within the tube, and stick close to it. Whence, again, it is evident, that the mercury at its own station, is not there sustain'd by the external air, but by a certain internal cord, whose upper end, being fasten'd to the finger, draws, and fixes it, after this manner, in the tube."

But this argument, being much of the same nature with the former, the answer made to that, may serve here, also; especially, because, in the present case, the pulp of the finger sustains less pressure on the inside of the tube, than in the other; the pressure of the atmosphere being here kept off from it, by the subjacent mercury; whereas there is nothing of that pressure abated against the other part of the finger, that kept it off from

from the deserted cavity of the tube; only from the pulp, contiguous to the tube, there may be some taken off, by the weight of the glass itself. But as for that part of the finger, which immediately covers the orifice, whether there be any spring in its own fibres, or other constituent substance, which finding no resistance in the place deserted by the quick-silver, may contribute to its swelling; he, who duly considers the account already given of this intrusion, will find no need of our author's internal *Funiculus*, which seems more difficult to conceive, than to solve the phenomena, in controversy, without it.

Our author proposes this as a clear demonstration; and it is, indeed, the principal thing in his book. "Take a tube of about 20 inches long, with both ends open, let its orifice be immers'd in stagnant mercury, and, one finger being plac'd underneath, that the mercury to be pour'd in, may not run thro', let it be fill'd with quick-silver, and then another finger apply'd to close its orifice. This done, if you take away the lower finger, the upper will be strongly drawn, and suck'd into the tube, and adhere to it so firmly, that it will elevate the tube itself, with all the quick-silver, and make it hang pendulous in the vessel. Since, then, the quick-silver in such a tube must be thrust upwards by the preponderating air; it can never be hence explain'd, how the finger is so drawn downwards, and made so strongly to adhere to the tube. For it cannot, by the air forcing upwards, be drawn downwards." In answer hereto, I alledge, that a good account may be given of this experiment, upon our hypothesis, which is sufficient to shew the argument not to be unanswerable.

I deny then, that the finger is drawn downward or made, by suction, to adhere to the tube, otherwise than we have already explain'd.

He says indeed, that the air, which thrust up the quick-silver, cannot so strongly draw down the finger: as if the air were not a fluid body, but a single and entire pillar of some solid matter.

However, when the tube is fill'd with quick-silver, the finger that stops the upper orifice is almost equally press'd above, and at the sides, by the contiguous air; and when the lower finger is remov'd, the cylinder of mercury, which before gravitated upon the finger, comes to gravitate upon the stagnant mercury, and, by its intervention, presses against the outward air; so that, against those parts of the finger, that are contiguous to the air, there is all the wonted pressure of the external air; but against that pulp contiguous to the mercury, not so much pressure, as against the other parts of the finger, by, about two thirds; because the mercurial cylinder, in this experiment, is suppos'd to be twenty inches high; and if it were but a little more than thirty inches high, the weight of the quick-silver would take off not two thirds only, but the whole pressure of the outward air from the pulp of the finger. For, in that case, the quick-silver would quite desert it, and settle below it. Wherefore, since I have before shewn, that the pressure of the outward air is taken off from the body that remains in the upper part of the tube, according to the weight of the liquor suspended in it; and since, on our hypothesis, the pressure of
the

the outward air is able to keep thirty inches of quick-silver, or thirty-two, or thirty-three feet of water suspended; 'tis no wonder, if a pressure of the ambient air, equal to the weight of a cylinder of water, of near twenty-two feet long, should be able to thrust in the pulp of the finger, at the upper orifice of the tube, and make it stick closely to the top of it.

I know our author affirms, that no pressure from without, can ever effect such an adhesion of the finger to the tube: but this should be proved. Nor could I, upon trial, find the adhesion of the finger to the tube, to be near so strong as our author relates: but, if you endeavour to thrust the pulp of your finger into the orifice of the tube, you may, through the glass, perceive it to be manifestly tumid, in the cavity of the pipe. And if, by pressing your finger against the orifice, you should not make the pulp adhere quite so strongly to the tube, nor swell quite so much within it, as may happen in some mercurial experiments; it is to be consider'd, that the air being a fluid, as well as a heavy body, does not press only against the upper part of the finger, but, upon as much of it as is exposed thereto, almost every way uniformly and strongly; and so, by its lateral pressure, thrusts the pulp of the finger into the orifice, where there is least resistance.

Hence, we need not borrow the objection, our author offers to lend, that, in the experiment under consideration, the quick-silver is press'd downward by the spring of some air lurking betwixt it and the finger; (tho' such a thing might easily happen) since we lately proved the contrary. And as for what he adds to confirm his argument, that "if the preponderating air succeed in the place of the lower finger, which was withdrawn; that is, if it sustain the quick-silver after the same manner, as by the lower finger apply'd under it; it is manifest, that the finger, on the top, ought to be no more drawn downwards, after the lower finger is removed, than before: but, experience teacheth the contrary;" we must consider, that the tube being supposed perfectly full of mercury, the finger, which stops the lower orifice, is usually kept strongly press'd against it, lest any of that ponderous fluid should get out: so that tho' the lower finger keeps up the mercury in the tube, and the pressure of the outward air would do so too; yet there is this difference, that the pressure of the atmosphere, depending upon its weight, cannot be increased, and weaken'd as we please, like the undermost finger. And, therefore, whereas the atmospherical cylinder will not sustain one of quick-silver, above 30 inches high, those who make the *Torricellian* experiment, often keep up, with the finger, a mercurial cylinder of, perhaps, 50 inches: so that, in our case, before the removal of the under finger, the pulp of the uppermost must sustain about the same pressure, where it is contiguous to the mercury, as the other part of the same finger; after the removal of the under finger, there is as much pressure of the atmosphere taken off from the pulp, as balances a cylinder of quick-silver 20 inches high.

The Pneumatical Experiments defended.

Our author's last experiment is thus proposed: "This opinion is false, because thence it would follow, that quick-silver, thro' a like tube, might be suck'd with the same ease out of a vessel, as water; which is contrary to experience: for, according to this opinion, that the fluid underneath, whether water, or mercury, may so ascend, no more is required, than that the air, in the tube, be drawn upwards by suction; where, the liquor below, will immediately ascend, being impell'd by the external air, which now preponderates." But we formerly shew'd, that when the mercurial cylinder, which rests upon the stagnant mercury, has, at the other end of it, air kept from any communication with the atmosphere, that included air has so much of the pressure of the external air taken off from it, as balances the mercurial cylinder. And the finger exposed to the whole pressure of the ambient air, in some of its parts, and in others but to the much fainter pressure of the included air, sustains an unusual pressure from the preponderating power of the atmosphere. Thus, the thorax, and the muscles of the abdomen, which serve for respiration, sustain the pressure of the whole ambient air; tho' these muscles are able, without any considerable resistance, to dilate the thorax; because, as fast as they open the chest, and, by dilating it, weaken the spring of the included air, the external air rushing in, for want of the usual resistance there, keeps that within the thorax, in an equilibrium to that without. We say, then, that if a cylinder of mercury be, by suction, rais'd in the tube to any considerable height, the pressure of the air in the thorax, is lessen'd by the whole weight of that mercurial cylinder; and, consequently, the respiratory muscles are thereby disabled from dilating the chest, as freely as usual. But, if instead of mercury, you substitute water, so short a cylinder of that takes off so little of the pressure of the included air, that it comes into the lungs with almost its usual strength; and, consequently, with almost the same force wherewith the external air presses against the thorax.

And there is an experiment of *M. Paschal's*, which shews clearly, that if we could free the upper part of a tube, from the pressure of all internal air, the quick-silver (as our author says, it should) would, by the pressure of the outward air, be impell'd up into the tube, as well as water, till it had attain'd a height sufficient to make its weight equal to that of the atmosphere. The experiment itself is this: "If a glass syringe be made of a sufficient length; and after the sucker is thrust into the utmost orifice, it be plung'd in the mercury, as soon as the sucker is drawn out, the mercury follows, and ascends to the height of two feet, three inches, and a half. And when, afterwards, tho' no greater force be added, the sucker is drawn higher, the mercury stands, and follows no farther; whence that space remains empty, which lies between the mercury, and the sucker." So that we may well explain our author's experiment, by saying, that in a more forcible respiration, the mercurial cylinder is rais'd higher than in a more languid one; because, in the former, the chest being more dilated, the included air is also more expanded, whereby its weakened spring cannot, as before,

before, enable the mercurial cylinder to balance the pressure of the ambient air. And the reason why the quick-silver is not, by respiration, rais'd as high as 'tis kept suspended in the *Torricellian* experiment, is not the pressure of the outward air being unable to raise it so high; but because the free dilatation of the thorax, is oppos'd by the pressure of the ambient air; which pressure being against so great a superficies, and but imperfectly resist'd by the weakned spring of the air in the thorax, will be very considerable; since, in our engine, the pressure of the external air against the sucker of less than three inches diameter, was able to raise an hundred weight. And, by the way, when we strongly suck up quick-silver in a glass tube, tho' the elevation thereof proceeded from our author's *Funiculus*, contracting itself every way; and tho' there be a communication betwixt the internal surface of the lungs, and the cavity of the tube; yet we feel not, in our lungs, any endeavour of the shrinking cord to tear off that membrane they are lined with.

Our author further says, that "the spring of the air can perform neither more nor less, in a close place, than its equilibrium in an open one." But I allow of this opinion, only in some cases; for, in others, we have performed much more by the spring of the air, which we can, within certain limits, increase at pleasure, than can be perform'd by the bare weight, which, for ought we know, remains always nearly the same. And of this difference, we formerly gave an instance; when, by compressing the air, in the receiver, we impell'd the mercurial cylinder higher than the station at which the balance of the air sustains it.

Our author adds, that "since the experiments of the adhesion of the finger, &c. succeed alike in a close and open place, the arguments produced against the equilibrium, make also against the spring of the air." This has, already, been answer'd; but since he says, that the experiments, concerning the adhesion of the finger, &c. succeed equally in a close and open place, I wish he had told us what way he took to make them; for, in ordinary rooms, there scarce ever wants a communication betwixt the internal and external air, by means whereof, the weight of the atmosphere has its effect within the room.

Our author supposes, that what we ascribe to the spring, and weight of the air, is performed by a sort of *Funiculus*, consisting of a thin substance, greatly expanded; which, lying between two bodies, endeavours to contract itself, and to bring these bodies together, to avoid a vacuum; by nature's abhorrence whereof, he, at length, solves all phenomena.

His first argument for this is, that the finger would not be drawn down, by the descent of the mercury in the *Torricellian* tube, were there not a *Funiculus*; and that, were nothin substance there extended, a vacuum must ensue.

But this argument being deduced from the suction of the pulp of the finger, upon the descent of the mercury, has been answer'd already. - Another argument, which he alledges against a vacuum, is, the transparency of that part of the tube, where 'tis said to be: for, were there a vacuum,

PNEUMATICS.

he says, it would be like a black pillar, neither able to afford any thing visible, nor to permit objects to appear thro' it.

But the invalidity of this assertion appears from the doctrine of the atomists, who teach, that light is made up of such subtile effluvia, as are able to penetrate glass, and, therefore, may leave many vacuities, tho' the cavity of the cylinder seems full of it; and no doubt, were the parts of the lucid matter contracted, they would not fill one tenth of that space; since the smoke, which fill'd our receiver, so as to make it appear opake, possess'd, when condens'd, only a small part thereof.

Thus a room may appear full of the smoke of a perfume, tho', if all the corpuscles that compose the smoke were re-united, they would make up but a small partil. A little camphire, also, will fill a room with its odour; but having, in well clos'd glasses, caught the fumes of it driven over by heat, and again reduced them into true camphire, I found its bulk very inconsiderable, in comparison of the space it possesses, when its scented corpuscles are scatter'd thro' the air.

I might add, that if the *Torricellian* experiment succeed in the dark, it may well be doubted, whether our author's argument will hold. For if he endeavours to prove, that the place in question was full in the dark, because, upon letting in the light, a light appears within it; we may reply, that this light is a new one, flowing from the lucid body, that darts its corporeal rays thro' the glass and space in dispute, which, for want of such corpuscles, were not, just before, visible.

And, supposing light to be made by a propagation of the impulse of lucid bodies thro' transparent ones, yet it will not thence follow, that the deserted part of the tube must be full: in one of our experiments, tho' many of those gross aerial particles, that appear necessary to convey a languid sound, were soon drawn out of the receiver, yet there remain'd so many, that the others were not miss'd, till a far greater number was extracted; and thus there may remain matter enough to transmit the impulse of light, tho' betwixt the particles of that matter there should be numberless vacuities: yet our author pretends to prove absolutely, that there is no vacuity in the disputed space. And should a *Cartesian* say, the deserted part of the tube is filled with *Materia subtilis*, he must allow the pressure of the outward air to be the cause of the suspension of the quick-silver; for tho' the *Materia subtilis* may readily fill the spaces deserted by the mercury; yet that within the tube cannot hinder so ponderous a liquor from subsiding as low as the stagnant mercury: since the whole tube, being pervious to that subtile matter, it may, with like facility, succeed, in whatever part of it shall be forsaken by the quick-silver.

Our author's next argument is, that the mercurial cylinder, resting at its wonted station, does not gravitate; as appears by applying the finger to the immersed, or lower orifice of the tube: whence he infers, that it must, of necessity, be suspended from within the tube. And, indeed, if the finger be applied to the open end of the tube, before 'tis quite lifted out of the stagnant mercury, the experiment will succeed; the

the finger, however, will feel a gravitation, or pressure, of the glass-tube, and the contained mercury, as of one body; but no sensible pressure of the mercury a-part, as if it endeavoured to thrust away the finger from the tube. Now, according to our hypothesis, the mercurial cylinder, and the air, balancing one another, the finger sustains not any pressure, sensibly differing from the ambient air, that presses against the nail, and sides of it, and from the included quick-silver that presses against the pulp. But if the mercurial cylinder should exceed the usual length, then the finger would feel some pressure from that additional quick-silver, which the air does not assist the finger to sustain: so that this phenomenon may as well be solved on our hypothesis, as on our author's. But how comes the mercury in the tube, when of a due altitude, to run out, upon removal of the finger beneath, if it be sustained only by an internal cord; and, when that sustains it, to resemble a solid body, if the pressure of the external air has no share in it?

If it be here said, that the finger must feel great pain, by being squeezed betwixt a pillar of thirty inches of quick-silver, and an equivalent pressure from the atmosphere; we must observe, that, in fluids, a solid has not that sense of pressure from surrounding bodies, which men are apt to imagine; as appears from divers: and I am informed, that the learned *Maignan*, tho' he purposely thrust his hands, three or four palms deep, into quick-silver, his fingers were not sensible of any weight, or pressure.

Lastly, our author tells us, that "those remarkable vibrations the quick-silver makes, in its descent, favours his hypothesis." But this phenomenon, also, is easily solved on our hypothesis: for when the experiment is made in a close place, as our receiver is, mercury, by its sudden descent, acquires an impetus, besides the pressure it has upon account of its gravity: whence it, for a while, falls below its station, and thereby compresses the air that rests upon the stagnant mercury; which air, by its own spring, again forcibly dilating itself, to recover its former extension, expands beyond it, and thereby impels up the quick-silver somewhat above its wonted station; in its fall from whence, it again acquires a power to compress the air: and this reciprocation of pressure, betwixt the quick-silver, and the external air, decreasing by degrees, at length wholly ceases, as the mercury loses that additional pressure it acquired by falling from parts of the tube, higher than its due station. But this way of explicating these vibrations, is not necessary in the free air; for if we consider the atmosphere only as a weight, and allow an impetus acquired by descent, the phenomenon will be easily explained by a balance, wherein one of the scales chancing to be depressed, they do not, till after many vibrations, regain their equilibrium.

I took a glass-siphon, whose two legs, unequal in length, were parallel, and both perpendicular to that part of the pipe which joined them; and poured quick-silver into it, till 'twas some inches high, and equal in both legs; then the siphon, being inclined, 'till most part of the quick-silver was fallen into one of the legs, I stopped the orifice of the other with my

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finger: and, erecting the siphon again, tho' the quick-silver were forced to ascend a little in that stopped leg; yet, because my finger prevented the air from getting away, the quick-silver was kept much lower in the stopped leg than in the other: but if, by suddenly removing my finger, I gave vent to the included compressed air, the preponderant quick-silver, in the other leg, would, with the mercury in the open one, make several undulations before, in both legs, it rested in an equilibrium. Now, in this case, there is no pretence for a *Funiculus* of violently distended air, to cause the vibrations of the mercury.

But there are many particulars which render the funicular hypothesis improbable.

And, first, our author acknowledges, that quick-silver, water, wine, &c. as well one as another, will descend in tubes, exactly seal'd at the top, in case the cylinder of liquor exceed the weight of a mercurial cylinder of twenty-nine inches and a half, but subside no longer than till it is a balance to a cylinder of quick-silver of that height. Now it's very strange, that, whatever the liquor be, there should be just the same weight, or strength, to extend them into a *Funiculus*. And this is the more surprising, because our author makes so great a difference betwixt the disposition of bodies of various consistences to be extenuated into a *Funiculus*, that he will not allow any human force able to produce one by the divulsion of two flat marbles, in case the contact of their surfaces were so exquisite, as quite to exclude all air; tho' his reasoning plainly agrees with experience, that adhering marbles may be forcibly sever'd; and, therefore, according to him, the superficial parts may be distended into a *Funiculus*, that prevents a *Vacuum*. But our hypothesis labours not under this difficulty; for the weight of the external air, being that, which keeps liquors suspended in seal'd tubes, it matters not of what nature or texture the suspended liquor is, provided its weight be the same with that of a mercurial cylinder equiponderant to the aerial one.

In the next place, I observe, that the account our author gives of his *Funiculus*, is much more strange than satisfactory, and not made out by any unquestionable parallel operations of nature: whereas, the weight and spring of the air may be inferr'd from such certain experiments, as are not concern'd in the present controversy. For the gravity of the air may be manifested by a pair of scales; and its spring is disclos'd so clearly in wind-guns, and other instruments, that our author does not deny it. But in the explanation of his *Funiculus*, he would have us remark two things; first, "that the quick-silver which fills the whole tube, doth not only touch the top of it, but firmly stick to it; and that the finger adheres to the mercury; since tho' the orifice of the tube be oil'd, that will not hinder it from sticking, as firmly as before." But two bodies, by trusion, may easily be made to stick together, as much as the tube and finger do, tho' one of them be oil'd; besides, this adhesion of the finger to the tube will happen, not only when the surface of the included quick-silver is contiguous to the finger, but many inches below it. Water and quick-silver, he says, ascend
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by suction, "because the parts of the air included in the tube, are now so firmly glued to one another, that they make a strong chain, whereby the water and quick-silver are drawn up." Which way of wreathing a little rarified air into so strong a rope, is highly improbable.

Secondly, he says, that "the rarification, or extension of a body, so as to make it take up more space, is not only caused by heat, but by distension, or, a certain disjoining power; as condensation is not only made by cold, but, also, by compression." And, 'tis true, and obvious, that the condensation of bodies, (taking that word in a large sense) may be made as well by compression, as cold. But, I wish he had more clearly express'd, what he means, in this place, by that rarification, which, he says, is to be made by a disjoining power; whereof, he tells us, there are innumerable instances. For, as far as may be gather'd, from the three examples he subjoins, 'tis only the air that is capable of being so extended, as his hypothesis requires quick-silver, and, even stones, to be. And, how will he prove, that even air may be thus extended, to fill two thousand times the space it possess'd before? For, that the same air, adequately fills more space at one time, than another, he proves but by the rushing of water into the evacuated glass, and almost filling it; which, he says, is done by the distended air, that contracting itself, draws up the water with it. The explanation he gives of his *Funiculus*, is this: "Since 'tis manifest, that the quick-silver sticks to the top of the tube; and that rarification is made by the mere distension of a body, it happens, that the descending quick-silver leaves its upper superficies fix'd to the top of the tube; and, by its weight, so stretches, and extenuates it, till it becomes easier to leave another superficies, in like manner, than to extend that any further. It leaves, therefore, a second; and, by its descent, extends that a little further, till it becomes easier to separate a third, than to extend that any more; and so on, till, at length, it hath no power to separate, or extend any more surfaces, when it comes to the height of $29 \frac{1}{2}$ inches, where it rests." Hence 'tis easy to discern, that he is oblig'd to assign his *Funiculus* a strange and unparallel'd way of production. Now, I must demand, by what force, upon the bare separation of the quick-silver, and the top of the tube, the new body, he mentions, comes to be produced; or, how it appears, that the mercury leaves any such thing, as he speaks of, behind it? For, the sense perceives nothing of it at the top of the tube; nor, is it necessary to explain the phenomena; as we have formerly seen. And how should the bare weight of the descending mercury, be able to extend a surface into a body? Besides, the succession of surfaces is a chimera: or, supposing some of the quick-silver were turn'd into a thin, subtle substance, yet, how comes that substance to be contriv'd into a *Funiculus* of so strange a nature, that scarce any weight can break it; and that, contrary to all other strings, it may be stretch'd, without becoming more slender, and obtains other very odd properties?

Our author says, indeed, that "these surfaces seem to be separated from the quick-silver, and to be extended into a most slender string, by the

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I farther demand, how the *Funiculus* comes by hooks, or parts proper to take fast hold of all contiguous bodies; and even the smoothest, such as glass, the calm surface of quick-silver, water, oil, &c. and how these slender, and invisible hooks, find innumerable loops, in smooth bodies, to take hold on so strongly, as to lift up a tall cylinder of quick-silver; and draw inwards the sides of strong glasses, so forcibly, as to break them to pieces? 'Tis, also, somewhat strange, that water, and other fluid bodies, should, when the *Funiculus* once lays hold on their superficial corpuscles, presently, like consistent bodies, be drawn up, in one entire continued piece; though,
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even in the exhausted receiver, they appear, by many signs, to continue fluid.

I know, that by calling this extenuated substance, a *Funiculus*, he intimates, that it has its spring inwards, like lute-strings, and ropes forcibly stretch'd; but there is no small disparity betwixt them: for, in strings, there is requir'd either wreathing, or some peculiar, and artificial texture of the component parts; but, a rarification of air, does not infer any such contrivance of parts, as is requisite to make bodies elastic. And, since lute-strings, &c. must, when they shrink inwards, either fill up, or lessen their pores, and increase in thickness, as they diminish in length; our author's *Funiculus* differs widely from them; since it has no pores to receive the shrinking parts; and contracts its length, without increasing its thickness. Nor does it, to me, seem very probable, that when, for instance, part of a polish'd marble is extended into a *Funiculus*, that *Funiculus* strongly aspires to turn into marble again. And 'tis very unlikely, that the space, our author would have replenish'd with his funicular substance, should be full of little, highly-stretch'd strings, that lay fast hold on the surfaces of all contiguous bodies, and always violently endeavour to pull them inwards. For, a pendulum being set a moving, in our exhausted receiver, vibrated as freely, and with the string as much stretch'd, as in the common air. Nay, the balance of a watch did there move freely; which is hard to conceive, if the moving bodies were to break thro' a medium consisting of innumerable strings, exceedingly stretch'd. And 'tis strange, if these strings, thus cut, or broken, by the passage of these bodies thro' them, could so readily have their parts re-united, and immediately be made entire again. And, in this case, the two divided parts of each small string, do not, like those of other broken strings, fly back from one another, but meet, and unite again; yet, when in the *Torricellian*-experiment, the tube, with the contain'd mercury, is suddenly lifted out of the stagnant quick-silver into the air, the *Funiculus* so strangely contracts itself, that it quite vanishes; so that the ascending mercury may rise to the very top of the tube.

But this is not all that renders our author's hypothesis improbable; for it necessarily supposes such a rarification, and condensation, as is unintelligible.

We must here premise, that a body is commonly said to be rarified, or dilated, when it acquires greater dimensions than it had before; and to be condensed, when it is reduced to less dimensions, that is, into a less space; and that there are three ways of explaining rarification: for, either we must say, that the corpuscles whereof the rarified body consists, depart from each other, so that no other substance comes in between them, to fill up the deserted spaces; or, that these new interstices, are but dilated pores, replenish'd, as those of a tumid sponge by water, with some subtle ethereal substance; or, lastly, that the same body does not only obtain a greater space in rarification, and a lesser in condensation; but, adequately, and exactly fills it: and so, when rarified, acquires larger dimensions,

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sider'd.*

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'Tis to this last way of rarification, that our author has recourse, in this hypothesis; tho', I confess, it appears to me so difficult to be *constellian* rarification. For the easier consideration of this matter, let us ceiv'd, that I doubt whether any phenomenon can be explain'd by it. Let us suppose, that in the *Magdeburg* experiment, he so often urges to prove his hypothesis, that the undilated air, which, as he tells us, possess'd about half an inch of space, consisted of 100 parts, 'twill not be deny'd, that as the aggregate is adequate to the whole space it fills, so each of the 100 parts is, likewise, adequately commensurate to its respective space, which is 100th part of the whole. Now, our author says, that "if a body possesses twice as much space, each part of that body must do the same." Whence the whole capacity of the sphere, which, according to him, was 2000 times bigger than the space possess'd by the unexpanded air, there must, likewise, be 2000 parts of space, commensurate, each of them, to one of the aforesaid 100th parts of air; and, consequently, when he affirms, that half an inch of air possess'd the whole cavity of the globe, if we will not admit, as he does not, either vacuities, or some subtiler substance in the interstices of the aerial particles, each part of air must, adequately, fill 2000 parts of space. Now, that this should be resolutely taught, to be really, and regularly done, in the *Magdeburg* experiment, will, questionless, appear very absurd to the *Cartesians*, and those other philosophers, who take extension to be but notionally different from body; and, consequently, impossible to be acquir'd, or lost, without the addition, or detraction of matter: and will, I doubt not, appear strange to every one who considers how generally extension is allow'd inseparable, and immediately to flow from matter; and bodies to have a necessary relation to a commensurate space. Nor do I see, if one portion of air may so easily be brought, exactly to fill a space 2000 times as great as that it did but fill before, without the addition of any new substance, why the matter contain'd in each of these 2000 parts of space, may not be farther brought to fill 2000 more, and so on; since each of these newly replenish'd spaces, is presum'd to be exactly fill'd with body; and no space, and, consequently, that which the un-rarified air replenish'd, can be more than adequately full. And since, according to our author, not only fluids, but even solids, as marble, are capable of such a distension; why may not the world be made many thousand times bigger than it is, without either admitting a vacuity betwixt its parts, or being increas'd with the addition of one atom of new matter?

He further attedges, that the phenomena of rarification cannot be explain'd, either by vacuities, or the sub-ingression of an ethereal substance; and that there are two ways of explaining that kind of it, which he contends for.

After our author's objections against the two ways of rarification proposed; the one by the vacuists, and the other by the *Cartesians*, who admit the most solid bodies, and, even glass itself, to be pervious to an ethereal,
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or subtle matter, he attempts to explain the manner by which his own rarification is perform'd; and having premis'd, that the explanation of the way how each part of the rarified body becomes extended, depends upon the quality of the parts into which the body is ultimately resolv'd; and, having truly observ'd, that they must, necessarily, be either really indivisible, or endlessly divisible, he endeavours to explain the *Aristotelian* rarification, according to these two hypotheses. But tho' he thus proposes two ways of making out his rarification, yet they are irreconcilable; and he speaks of them very doubtfully, and obscurely.

And, first, having told us how rarification may be explain'd, if we admit bodies to be divisible *in infinitum*, he makes an objection against the infinity of parts in a continuum, whereto he gives so dark an answer, that, I confess, I do not understand it.

And 'tis not clear to me, that even such a divisibility of a continuum, as is here supposed, would make out the rarification he contends for: since, let the integrant parts of a continuum be more or less finite, or infinite in number, still each part, being a corporeal substance, must have some particle of space commensurate to it; and if the whole body be rarified, for instance, to twice its former magnitude, then will each part be, likewise, extended to double its former dimensions; and fill both the place it took up before, and another equal to it; and, consequently, two places. I will not, however, pretend to affirm which of the two ways, by atoms, or by parts infinitely divisible, our author declares himself for: but, whichever of them it be, I think he has not intelligibly made it out; as himself seems willing to confess. So that, in his discourse of rarification, to which our author frequently refers, as that which should make good what seems the most improbable, he has, instead of a probable hypothesis, substituted a doctrine which himself dares not pretend capable of being well freed from the difficulties with which it may be charged.

As for the other way of explaining rarification, by supposing that a body is made up of parts indivisible, he is, upon this hypothesis, reduced to allow, that "one and the same part must be in two places adequately; for since it is indivisible, and takes up a greater space than before, it must, of necessity, be also in every point of that space; or be virtually extended thro' all that space." When, therefore, he, presently after, affirms, that by this virtual extension of the parts, the difficulties which have, for so many ages, perplex'd philosophers, may be easily solved, he must give me leave to desire he would explain what this *extensio virtualis* is; and how it will remove the difficulties charged upon the *Aristotelian* rarification. For the easier consideration of this matter, let us resume what we lately suppos'd, that, in the *Magdeburgic* experiment, the half-inch of undilated air, consisted of a hundred corpuscles; I demand, how the indivisibility of these corpuscles will qualify them to make out such a rarification, as our author imagines? For, what does their being indivisible, in this case, but make it the less intelligible, how they can fill above

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100 parts of space? He will answer, they are virtually extended. But, not here to question, how this indivisibility makes them capable of being so; I demand, whether by an atom's being virtually extended, its corporeal substance does really fill more space than it did before, or not? If it do, then 'tis a true, and real, and not barely a virtual extension: but such an extension, we have shewn, will not serve the turn; and our author seems to confess as much, by devising this virtual extension, to avoid the inconveniencies to which he saw his doctrine of rarification would otherwise be exposed. But if it be said, that when an atom is virtually extended, its corporeal substance fills no more space than before; I demand, how that which is not a substance, can fill a space; and how this improper, and only metaphorical extension, will solve the phenomena of rarification? As how the half-inch of air, at the top of the fore-mention'd sphere, shall, without a corporeal extension, fill the whole cavity of 2000 times its bigness, when the water is suck'd out of it, and act at the lower-part of the sphere? For, our author teaches, that the whole globe was fill'd with a certain thin substance, which, by its contraction violently snatch'd up the water wherein the neck of the glass was immers'd. And, in a parallel case, he makes it his grand argument, to prove, there is no vacuum in the deserted part of the tube, in the *Torricellian* experiment, that the attraction of the finger cannot be but from some real body.

Our author's *Funiculus*, also, supposes a condensation, that, to me, appears incumber'd with no less difficulties. For, since he teaches, that a body may be condens'd, without either having any vacuities for the compress'd parts to retire into; or, having its pores fill'd with any subtile, and yielding matter, that may be squeezed out of them; it follows, that the parts of a body to be condens'd, immediately touch each other: which supposed, I demand, how bodies, that are already contiguous, can be brought closer, without penetrating each other? So that I see not how this condensation can be perform'd, without penetration of dimensions. In the *Magdeburgic* experiment, he tells us, that the whole capacity of the globe is fill'd with an extremely rare body; which, according to him, intercepts neither pores, nor any heterogeneous substance. Now let us consider, that before the admission of water into the exhausted globe, there was, according to him, 2000 half-inches of a true and real body; and that, after the admission of the water, there remain'd, in the same globe, no more than one half-inch of body besides. Since, then, our author does not pretend, that the 1999 half-inches of matter, that now appear no more, travers'd the body of water; and since he will not allow, that it gets away thro' the pores of the glass; I demand, what becomes of so great a quantity of matter? For that 'tis annihilated, I suppose, he is too rational to pretend; and to say, that so many parts of matter, should be retir'd into that one part of space that contains the half-inch of air, is little less incredible: for, that space was suppos'd perfectly full of body before; and how a thing can be more than perfectly full, who can conceive? In short, according to our author's way of condensation, two, or, perhaps,

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perhaps, two thousand bodies, may be crowded into a space that is adequately fill'd with one of them apart. And, if this be not penetration of dimensions, I desire to be inform'd what is.

But as the hypothesis I am opposing, is a kind of inversion of ours; ^{The pressure, and spring of the air con- firm'd.} supposing the spring, or motion of restitution in the air, to tend inwards, as, according to us, it tends outwards, many of the phenomena would, if it were true, be plausibly explicable by it; the same motions, in an intermediate body, being, in many cases, producible alike, whether we suppose it to be thrust, or drawn; provided, both the endeavours tend the same way. But then we may be satisfied, whether the effect be to be ascrib'd to pulsion, or to traction, if we can find out an experiment, wherein there is a reason that such an effect should follow, in case pulsion be the cause inquir'd after; and not, in case it be traction. And such an *experimentum crucis* is afforded us by M. *Paschal*, who observ'd, that the *Torricellian* experiment, being made at the foot, and in different parts of a very high mountain, after he had ascended an hundred and fifty fathom, the quick-silver was fallen two inches and a quarter below its station at the foot of the mountain; and that at the very top of the hill, it had descended above three inches below the same station. Whence it appears, that the quick-silver being carried up towards the top of the atmosphere, falls down the lower, proportionably to the height of the place wherein the observation is made: the reason of which, on our hypothesis, is, that the nearer we come to the top of the atmosphere, the shorter, and lighter is the cylinder of air, incumbent upon the stagnant mercury; and, consequently, the less weight of mercury will that air be able to balance, and keep suspended. And, since this noble phenomenon, thus clearly follows upon ours, and not upon our author's hypothesis, it seems to determine the controversy; because, in this case, it cannot be pretended, that the descent of the quick-silver, in the tube, is caus'd from the preternatural rarification, or distension of the external air, when, by trying to restore itself, it endeavours to draw up the stagnant mercury: for, there appears no such forcible dilatation of that air, as in many of the phenomena of our engine, he is here pleas'd to imagine.

To this experiment he replies but two things, which, neither singly, nor together, will amount to a satisfactory answer.

And first, he questions the truth of the observation itself, because, having made trial on a low hill, the event did no ways answer his expectation. But *Gassendus* relates, that the observation was five times repeated, with circumstances, which sufficiently argue the diligence wherewith the experiment was made: and, I can confirm these observations, by two more made on hills in *England*. But, however the proportion of the descent of the quick-silver may vary according to the different consistence, and other accidents of the air, in the particular places, and times of the experiments being made; yet all observations agree in this, that nearer the top of the atmosphere the quick-silver falls lower, than when further from it. And, in one of these experiments, a determinate quantity of air being left in the tube,

before

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before the mouth of it was open'd, under the stagnant mercury, and notice taken how low such a quantity of that air depress'd the mercurial cylinder, 'twas observ'd, that at the mountain's foot, the included air was not able to depress the quick-silver so much. Whence we infer, that the cylinder of air, at the top of the hill, being shorter and lighter, did not so strongly press against the included air, as did the ambient air at the bottom of the hill, where the aerial cylinder was longer, and heavier.

We, also, attempted a trial, wherein we hoped to find a sensible difference in the weight of the atmosphere, in a far less height, than that of an ordinary hill. But instead of a common tube, we made use of a weather-glass, and instead of quick-silver, employ'd common water in the pipe belonging to the glass; that small changes in the weight or resistance of the atmosphere, in opposition to the included air, might be the more discernible.

Fig. 83.

The instrument, we made use of consisted only of a glass AB, with a broad foot, a narrow neck, and a slender glass pipe CD, open at both ends; the pipe so plac'd, that the bottom of it, almost reach'd to the bottom of the bigger glass AB, within whose neck A, it was fasten'd with a close cement, that both kept the pipe in its place, and hinder'd all communication betwixt the inward II, and the outward air KK, except by the cavity of the pipe CD. Now we chose this glass AB, more than ordinarily capacious, that the effect of the dilatation of the included air II, might be the more conspicuous. Then conveying a convenient quantity of water HD, into this glass, we carry'd it to the leads of the abbey-church at Westminster, and there blew in a little air, to raise the water to the upper part of the pipe, that, being above the vessel AB, we might the more precisely mark the several stations of the water. Afterward, having suffer'd the glass to rest a pretty while upon the leads, that the air II, within, might be reduced to the same state with KK, that without; having mark'd the station of the water F, we gently let down the vessel by a string to the foot of the wall, where one attended to receive it, who having suffer'd it to rest upon the ground, told us, that it was subsided about an inch below the mark F; whereupon, having order'd him to put a mark at this second station of it E; we drew up the vessel again, and suffering it to rest a while, observ'd the water to be re-ascended to the first mark F, which was, indeed, about an inch above E: and this we did a second time, with almost a like success; tho', two or three days after, the wind blowing strongly upon the leads, we found not the experiment to succeed quite so regularly; yet the water, always, manifestly, fell lower at the foot of the wall, than at the top. But, to avoid mistakes, and prevent objections, we made the experiment within the church, at the same height with the leads; but the upper part of the pipe being, accidentally, broken off, we order'd the matter so, that the surface G, of the remaining water in the pipe, should be about an inch higher than the surface of that in the vessel. And then, letting down the glass, I found that, almost, as soon as it was settled upon the pavement, it was not only fallen as low as the other water, but the outward air depress'd it so far, as, whilst I was looking on

to break in below the bottom of the pipe, and ascend thro' the water in bubbles; after which, the glass being drawn up again, the water was, very manifestly, re-ascended. Hence 'tis evident, that the atmosphere gravitates more, *ceteris paribus*, near the surface of the earth, than in the more elevated parts of the air: for the leads, on which we made our trials, were found, in perpendicular height, but 75 feet.

But, for an experiment of the same kind, made at a greater height, take the following, communicated by Dr. Power.

On the 15th of October, 1661, we took a weather-glass AB, about two feet in length, and carrying it to the bottom of *Hallifax-hill*, the water stood in the shank at 13 inches above that in the vessel: thence carrying it, thus fill'd, with the whole frame, immediately to the top of the said hill, the water fell down to the point D; that is, an inch and a quarter lower than it was at the bottom of the said hill; which proves the elasticity of the air: for the internal air AC, which was of the same power and extension with the external, at the bottom of the hill, manifested a greater elasticity, than the mountain-air there manifested pressure; and so extended itself further by CD.

The like experiment, I hear, the same ingenious person has lately repeated, and found the descent of the water to be greater than before. And tho' some have thought it strange, that, on a hill, far inferiour to the *Alps*, and *Appennines*, so short a cylinder of water should fall so much; yet I see not any reason to distrust, upon this ground, either this experiment, or ours made at *Westminster*; but rather wonder the water fell no more, if the hill be considerably high: for their suspicion seems grounded on a mistake; as if because the quick-silver, in the *Torricellian* experiment, made without purposely leaving any air in the tube, would not at the top of the mention'd hill, have subsided above an inch, the water, that is near 14 times lighter, should not fall above a 14th part of that space; whereas, in the *Torricellian* experiment, the upper-part of the tube has little, or no air left in it, while the correspondent part of the weather-glass contain'd air, whose pressure was little less than that of the atmosphere at the bottom of the hill; and, consequently, must be much greater, than the pressure of the atmosphere at the top of the hill.

Another particular, which confirms our hypothesis, is that experiment made by the same M. *Paschal*, by carrying a slack-blown foot-ball, from the bottom to the top of an high mountain; for, the foot-ball gradually swell'd, the higher it was carry'd: so that at the top of the mountain it appear'd as if it were full-blown; and became gradually lank again, as it was carry'd downwards; so that, at the foot of the hill, it was flaccid as before. We have here an experiment to prove our hypothesis, wherein recourse cannot be had to any body, forcibly, and preternaturally distended, such as is pretended to remain in the deserted space of the tube, in the *Torricellian* experiment.

But, further, our author's hypothesis is needless; for, he denies not that the air has some weight and spring, but affirms it very insufficient to

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The elastic
force of com-
press'd and di-
luted air,
measured.

to counterpoise a mercurial cylinder of 29 inches. We shall, therefore, now endeavour to manifest by experiments, purposely made, that the spring of the air is capable of performing far more than is necessary to solve the phenomena of the *Torricellian* experiment. We took a long glass tube, so bent at the bottom, that the part turned up, was almost parallel to the rest of the tube; and the orifice of this shorter leg being hermetically seal'd, the length of it was divided into inches, each of which was sub-divided into eighths, by a list of paper carefully pasted along it: then putting in as much quick-silver as fill'd the bended part of the siphon, that the mercury standing in a level, might reach, in the one leg, to the bottom of the divided paper, and just to the same height, in the other; we took care, by frequently inclining the tube, that the air, at last, included in the shorter cylinder, should be of the same laxity with the rest of the air about it. This done, we began to pour quick-silver into the longer leg of the siphon; which, by its weight, pressing upon that in the shorter, gradually straitned the included air; and continuing to pour in quick-silver, till the air, in the shorter leg, was, by condensation, reduced to take up but half the space it possess'd before, we observ'd, in the longer leg of the glass, on which was, likewise, pasted a list of paper, divided into inches, and parts, that the quick-silver was 29 inches higher than in the other. Hence we see, that as, according to our hypothesis, the air, in that degree of density, and correspondent measure of resistance, where-to the weight of the incumbent atmosphere reduces it, is able to balance, and resist the pressure of a mercurial cylinder of about 29 inches; so, here, the same air, brought to a degree of density, about twice as great as it had before, obtains a spring twice as strong; being able to sustain, or resist a cylinder of 29 inches, in the longer tube, together with the weight of the atmospherical cylinder, that rested upon those 29 inches of mercury.

After some other trials, one of which we made in a tube, whose longer leg was perpendicular; and the other, that contain'd the air parallel to the horizon; we, at last, procured a tube, which, tho' large, was so long, that the cylinder, whereof the shorter leg of it consisted, admitted a list of paper divided into 12 inches, and their quarters; and the longer leg another, several feet in length, and divided after the same manner: then quick-silver being poured in, to fill up the bended part of the glass, that the surface of it, in either leg, might rest in the same horizontal line; more quick-silver was pour'd into the longer tube: and notice being taken, how far the mercury rose therein, when it appear'd to have ascended to any of the divisions in the shorter; the several observations that were thus successively made, and set down, afforded us the following table.

Fig. 85.

which is the Torricellian experiment. But the air has some weight and spring, and is not a vacuum. Hence, our author's hypothesis is needless; for he denies not that, further, our author is very inconsistent.

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A TABLE of the Condensation of the AIR.

A	B	C	D	E
48	12	00	29	29
46	11 $\frac{1}{2}$	01 $\frac{1}{2}$	30	30
44	11	02 $\frac{1}{2}$	31	31
42	10 $\frac{1}{2}$	04 $\frac{1}{2}$	33	33
40	10	06 $\frac{1}{2}$	35	35
38	9 $\frac{1}{2}$	07 $\frac{1}{2}$	37	36 $\frac{1}{2}$
36	9	10	39	38
34	8 $\frac{1}{2}$	12 $\frac{1}{2}$	41	41
32	8	15 $\frac{1}{2}$	44	43 $\frac{1}{2}$
30	7 $\frac{1}{2}$	17 $\frac{1}{2}$	47	46 $\frac{1}{2}$
28	7	21 $\frac{1}{2}$	50	50
26	6 $\frac{1}{2}$	25 $\frac{1}{2}$	54	53 $\frac{1}{2}$
24	6	29 $\frac{1}{2}$	58	58
23	5 $\frac{3}{4}$	32 $\frac{1}{2}$	61	60 $\frac{1}{2}$
22	5 $\frac{1}{2}$	34 $\frac{1}{2}$	64	63 $\frac{1}{2}$
21	5 $\frac{1}{4}$	37 $\frac{1}{2}$	67	66 $\frac{1}{2}$
20	5	41 $\frac{1}{2}$	70	70
19	4 $\frac{3}{4}$	45 $\frac{1}{2}$	74	73 $\frac{1}{2}$
18	4 $\frac{1}{2}$	48 $\frac{1}{2}$	77	77
17	4 $\frac{1}{4}$	53 $\frac{1}{2}$	82	82 $\frac{1}{2}$
16	4	58 $\frac{1}{2}$	87	87
15	3 $\frac{3}{4}$	63 $\frac{1}{2}$	93	93
14	3 $\frac{1}{2}$	71 $\frac{1}{2}$	100	99 $\frac{1}{2}$
13	3 $\frac{1}{4}$	78 $\frac{1}{2}$	107	107 $\frac{1}{2}$
12	3	88 $\frac{1}{2}$	117	116 $\frac{1}{2}$

Added to 29 $\frac{1}{2}$ makes

AA The number of equal spaces in the shorter leg, containing the same parcel of air, differently expanded.

B The height of the mercurial cylinder, in the longer leg, that compress'd the air into those dimensions.

C The height of a mercurial cylinder, that balanced the pressure of the atmosphere.

D The aggregate of the two last columns B and C, exhibiting the pressure sustain'd by the included air.

E What that pressure should be, supposing it in reciprocal proportion to the expansion.

For the better understanding of this experiment, it is proper to observe the following particulars. 1. The tube being very tall, we were obliged to use it on a pair of stairs, which were very well illumined; and for preservation, it was suspended by strings. 2. The lower, and bent part of the pipe, was placed in a square wooden box, large and deep, to prevent the loss of the quick-silver. 3. We were two, to make the observation together; the one to take notice at the bottom, how the quick-silver rose in the shorter cylinder; and the other, to pour it in at the top of the longer. 4. The quick-silver was pour'd in but slowly, according to the direction of him who observ'd below. 5. At the beginning of the operation, that we might the more truly discern where the quick-silver rested, from time to time, we made use of a small looking-glass, held in a convenient posture, to reflect to the eye what we desired to see. 6. When the air was crowded into less than a quarter of the space it possessed before, we try'd whether the cold of a linen-cloth, dipp'd in water, would condense it: and it, sometimes, seem'd a little to shrink, but not so manifestly, that we dare build upon it. We then try'd, likewise, whether heat would dilate it; and, approaching the flame of a candle to that part where the air was pent up, it had a more sensible operation than the cold before; so that we scarce doubted the expansion of the air would, notwithstanding the weight that oppress'd it, have been made conspicuous, if the fear of breaking the glass had not kept us from increasing the heat. This.

This sufficiently proves the principal thing for which I here alledge it; since 'tis evident, that as common air, when reduced to half its natural extent, obtain'd a spring, about twice as forcible as it had before; so the air, thus compress'd, being farther crowded into half this narrow room, thereby obtain'd a spring as strong again as that it last had, and consequently, four times as strong as that of common air. And, there is no cause to doubt, that if we had been furnish'd with a greater quantity of quick-silver, and a very strong tube, we might, by a further compression of the included air, have made it balance the pressure of a far taller, and heavier cylinder of mercury. For no man, perhaps, yet knows, how near to an infinite compressure the air may be reduced, by a force competently increas'd. So that, here our author may plainly see, the spring of the air can resist, not only the weight of twenty-nine inches, but, in some cases, above one hundred inches of quick-silver; and this, without the assistance of his *Funiculus*, which, in our present case, has no pretence to be employ'd. And, to shew, that the weight of the incumbent atmosphere, made a part of the weight resisted by the imprison'd air; when the mercurial cylinder, in the longer leg of the pipe, was about one hundred inches high, we caus'd a man to suck at the open orifice, whereupon the mercury in the tube considerably ascended: which phenomenon cannot be ascrib'd to our author's *Funiculus*; since, by his own confession, that cannot pull up a mercurial cylinder of above twenty-nine or thirty inches. And, therefore, the pressure of the atmosphere, being in part taken off, by expanding itself into the man's dilated chest, the imprison'd air, was, thereby enabled, manifestly, to dilate, and repel the mercury that compress'd it, till there was an equality of force betwixt the strong spring of the compress'd air on the one part, and the tall mercurial cylinder, with the contiguous dilated air, on the other.

Now, if to what we have deliver'd concerning the compressure of the air, we add some observations of its spontaneous expansion, it will the better appear, how much the phenomena of these mercurial experiments depend upon the different measures of strength to be met with in the air's spring, according to its various degrees of compression and laxity.

At the beginning of the operation, that we might the more truly discern where the quick-silver rested, from time to time, we made use of a small looking-glass, held in a convenient position to reflect on the eye what we desired to see. When the air was crowded into the tube, and the space reduced, as before, we took whether the end of a black-board, dipping in water, would contain it: and it sometimes seem'd a matter of doubt, but not so manifestly, that we durst build upon it. We then try'd, likewise, whether heat would dilate it; and, approaching the flame of a candle to that part where the air was, it did a notable dilatation: then the cold before, notwithstanding the weight of the atmosphere, the air would, notwithstanding the weight of the atmosphere, have been made to dilate, if the heat of heat, and the glass had not kept us from seeing the heat.

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A T A B L E of the Ravifaction of the Air.

A	B	C	D	E
1	00		29½	29½
1½	10½		19½	19½
2	15½		14½	14½
3	20½		9½	9½
4	22½		7½	7½
5	24½		5½	5½
6	24½		4½	4½
7	25½		4½	4½
8	26		3½	3½
9	26½		3½	3½
10	26½		3	3
12	27½		2½	2½
14	27½		2½	2½
16	27½		2	2
18	27½		1½	1½
20	28		1½	1½
24	28½		1½	1½
28	28½		1½	1½
32	28½		1½	0½

Subtracted from 29 ½ leaves

- A. The number of equal spaces at the top of the tube, that contain'd the same parcel of air.
- B. The height of the mercurial cylinder, that together with the spring of the included air balanc'd the pressure of the atmosphere.
- C. The pressure of the atmosphere.
- D. The complement of B to C, exhibiting the pressure sustain'd by the included air.
- E. What the pressure should be, according to the hypothesis.

* To make the experiment of the debilitated force of expanded air the plainer, we must mention some particulars, especially with relation to the manner of performing it. 1. We made it on a light pair of stairs, and with a box lin'd with paper to receive the mercury, that might be spilt; and in a glass tube about six feet long, hemetically seal'd at one end. 2. We also provided a slender glass pipe about the bigness of a swan's quill, and open at both ends, all along which, was pasted a narrow list of paper, divided into inches and half quarters. 3. This slender pipe, being thrust into the greater tube, almost fill'd with quick-silver, the glass help'd to make it swell to the top of the tube; and the quick-silver getting in at the lower orifice of the pipe, fill'd it up till the mercury, included in that, was near upon a level with the surface of the surrounding mercury in the tube. 4. There being little more than an inch of the slender pipe left above the surface of the stagnant mercury, and, consequently, unfill'd therewith, the prominent orifice was carefully clos'd with melted sealing-wax; after which, the pipe was let alone for a while, that the air, dilated by the heat of the wax, might, upon refrigeration, be reduced to its wonted density. And then we observ'd, by help of the list of paper, whether we had included more or less than an inch of air, and in either case, we rectify'd the error, by a

* "The open air, in which we breathe," says Sir. I. Newton, "is 8 or 900 times lighter than water, and by consequence 8 or 900 times rarer. And since the air is compress'd by the weight of the incumbent atmosphere, and the density of the air is proportionable to the compressing force, it follows, by computation, that at the height of about 7 English miles from the earth, the air is

"four times rarer than at the surface of the earth; and at the height of 14 miles it is 16 times rarer than at the surface of the earth; and at the height of 21, 28, or 35 miles it is respectively 64, 256, or 1024 times rarer, or thereabouts; and at the height of 70, 140, and 210 miles, it is about 1.000000, 1.000000, 000000, or 1.000000.000000.000000." &c. Newton. Optic. p. 341. 342.

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small hole made with a heated pin in the wax, and afterwards clos'd it up again. 5. Having thus included a just inch of air, we lifted up the slender pipe, by degrees, till the air was dilated to an inch, an inch and a half, two inches &c. and observ'd, in inches and eighths, the length of the mercurial cylinder, which at each degree of the air's expansion was impell'd above the surface of the stagnant mercury. 6. The observations being ended, we presently made the *Torricellian* experiment with the above-mention'd large tube, six feet long, that we might know the height of the mercurial cylinder for that particular day and hour; which we found to be twenty-nine inches and three quarters. 7. Our observations, made after this manner, furnish'd us with the preceding table, in which here would not, probably, have been found the difference here set down betwixt the force of the air, when expanded to double its former dimensions, and what that force should have been, precisely, according to the theory. but that the included inch of air receiv'd some little accession during the trial; which this difference causing us to suspect, we found, by plunging the pipe again into the quick-silver, that the included air had gain'd about half an eighth; which we guess'd to have come from some little aerial bubbles in the quick-silver contain'd in the pipe.

Here we find that the inch of air, when first included, sustain'd no other pressure than from the incumbent air, and was no more compress'd than the rest of the air we breath'd and mov'd in; that this inch of air, when expanded to twice its former dimensions, was able, with the help of a mercurial cylinder, of about fifteen inches, to counterpoise the weight of the atmosphere; and that this was impell'd up into the pipe by the external air gravitating upon the stagnant mercury, which, also, sustain'd above 28 inches of mercury, when the internal air had its spring too far weakened, to make any considerable resistance: from whence 'tis plain, that the free air, here below, is, almost, as strongly compress'd by the weight of the incumbent atmosphere, as it would be by the weight of a mercurial cylinder, 28, or 30 inches high; and, consequently, is not in such a state of laxity, as men usually imagine; but acts like some mechanical agent, with a force decreasing, in a stricter proportion to its increase of dimension, than has been, hitherto, taken notice of.

And hence, at length, we see, that our author's hypothesis is unnecessary to solve the phenomena in dispute: which is no small acquisition, since the two principal things, that induced him to reject our hypothesis, are, nature's abhorrence of a vacuum; and that, tho' the air have some weight and spring, yet these are insufficient to make out the known phenomena, for which, we must, therefore, have recourse to his *Funiculus*. But, he has not disprov'd a vacuum, yet we have manifested, that the spring of the air may perform greater things, than what our explanation of the *Torricellian* experiments, and those of our engine, require.

We come now to the last part of our defence, wherein we are to consider what our author objects to some particular experiments.

Against

Against our first experiment, he objects nothing, but that, by applying the finger to the orifice of the valve, when the pump is freed from air, the sucker will not appear to be thrust inward by the external air, but, as the finger, to be drawn inwards, by the internal. But this phenomenon has been, formerly, accounted for, upon our hypothesis.

Of our third experiment, he says; that "it very well agrees with his principles; for, since by this depression of the sucker, the air, in the cavity of the cylinder, is separated from the cylinder, and descends, together with the sucker, in that whole depression; new surfaces are taken from that descending air, and stretch'd out, as in the case of descending water. Since, therefore, such surfaces, are as easily slipp'd off, and extended at the end of the depression, as at the beginning, it is no wonder there should be the same difficulty of depressing, in both cases." By which, he seems to intend an opposition to a part of the third experiment, which I oppos'd not against his opinion: yet he offers nothing at all to invalidate my inference; but, instead of that, proposes a defence of his own opinion, which supposes the truth of his hypothesis; and is unsatisfactory, even according to that, or else, disagrees with what himself hath taught us, but a little before. For, 'tis evident, that the more the sucker is depress'd, the more the cylinder is exhausted of air. And, speaking of the air, in the receiver, he affirms, that "tis the more extended and rarified, the more is drawn out; and, therefore, acquires the greater force to contract itself." Though here he would have us believe, that the little internal air, in the cavity of the shank of the stop-cock, as strongly attracts the sucker, or resists its depression, when the sucker is near the top of the cylinder, as when, being forced down to the lower part thereof, the same portion of remaining air must be exceedingly more distended.

To the fourth experiment, our author objects nothing, but endeavours explain it his own way, whereto he says, this circumstance excellently agrees, that, upon the return of the external air, into the receiver, the tumid bladder immediately shrinks; because the air in the receiver, which drew the sides of the bladder outward, from the middle of it, is hereby relax'd: which explication, whether it be more natural than ours, let any one judge, who has consider'd what we have alledg'd against the *Funiculus*.

To the breaking a glass receiver, not of a globular figure, by exhausting most of the internal air, whereby its diminish'd pressure became unable to resist that of the outward air; our author confidently says, "it seems incredible, that the most soft air should so vehemently compress such a glass, on all sides, as to break it." As if it were more credible, that the air within, should be able to act more powerfully upon the glass, than that without, which himself confesses to be a heavy body; and which, not only reaches from the surface of the earth, to the top of the highest mountains, but may, for ought we know to the contrary, be heap'd upon the receiver, to the height of some hundreds of miles.

After a recital of the ninth experiment, he proposes his objection to it thus. "But this seems far remov'd from truth, because, if the pressure of the air, which descends by that tube into the vial, be so great as to break the vial itself, it should, certainly, first, very much move the water, in which the tube is immers'd, excite bubbles in it, &c. yet it is certain, that the water, before the vial breaks, doth not move at all." But, for all this, I think our explanation true: for, we put the water into the vial that was broken, upon a particular design; and, in the second trial, the water was omitted. But, notwithstanding this water, the sides of the glass being expos'd to the pressure of the atmosphere, wholly sustain'd it, before the exhaustion of the receiver: so that there needed no such blowing in of the air a-fresh, as our author imagines, to break the vial; it being sufficient for that purpose, that the pressure against the convex superficies of it, was taken off, by exhausting the receiver; the pressure against the concave superficies, remaining as great as ever. And, therefore, we need not altogether deny what he says, that "tho' the tube had been clos'd at the top, the vial would still have broke." For, since, in such cases, the air is shut up, with the whole pressure of the atmosphere upon it, this may, almost, as easily, break the glass, as if it were unstopp'd; and, accordingly, we mention the breaking of a thin glass, hermetically seal'd, upon the extraction of the ambient air: But, as confidently as our author speaks, such thin vials are subject, upon withdrawing the ambient air, to stretch a little; whereby the spring of the included air, may, in some cases, be so far weakned, as not to be able to break them, unless assisted by the pressure of the atmosphere: and when the vial actually begins to break, the ensuing pressure of the outward air, upon that within the vial, may help to throw the parts of the glass more forcibly asunder.

The author, having recited our conjecture, as to the reason why two flat smooth marbles stick so closely together, approves my way of examining that conjecture. But, I say, tho' the marbles were kept together, by the pressure of the ambient air, yet they did not fall asunder, in our exhausted receiver, because of some small leak in the receiver; yet he tells us, with his usual confidence, that this very experiment sufficiently shews, that opinion false. But, possibly, he would have spoken less resolutely, if he had made all the trials, about the adhesion of marbles, that I have. For he speaks, as if all that we ascribe to the air, in such experiments, were to sustain the lower marble, with the weight, perhaps, of a few ounces; whereas, if the air be wholly kept from getting between the stones, it may sustain a weight equal to that of a pillar of air, as broad as the basis of the lower marble, and as high as the atmosphere; or, to the weight of a column of quick-silver, of the same thickness, and about 30 inches long. And, therefore, since when we had exhausted our receiver, as far as we could, there remain'd air enough to sustain in the tube, a cylinder of quick-silver an inch high; and since the broader the contiguous marbles are, the greater weight, fasten'd to the lowermost, may be sustain'd, by the

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the resistance of the air ; it's no wonder, that the air, remaining in the receiver, should support the lower-most marble, whose diameter was near two inches, and a weight of four ounces ; those two weights being inferior to that of a mercurial cylinder, of the same diameter, and an inch in length : and tho' they were not, yet, perhaps, the receiver was less empty'd, when we made the 31st experiment, than when we made the 17th. And 'twas with the same pair of marbles, that, before an illustrious assembly, the upper-most drew up the lower-most, tho' clogg'd with a weight of above 430 ounces.

As for the account our author gives of this phenomenon, few, I believe, will acquiesce in it : for, not to insist upon the objection, which himself takes notice of, that, according to him, the distended air in the receiver, should draw the adhering marbles asunder, his explanation supposes, that there cannot, naturally, be a vacuum ; whence he infers, that " the stone could not descend, but by leaving such a thin substance behind : " it, as happens in the descent of quick-silver, or water." He adds, that the adhesion, in our case, proves obstinate, because such a substance is far more difficult to be separated from marble, than from quick-silver, or any other kind of body ; but this assertion is precarious. And though I have made numerous experiments, with stones of several sizes, yet I could never find, that, by their cohesion, they would sustain a weight greater than that of a pillar of the atmosphere, that press'd against the lowest ; which is a considerable circumstance, that much better agrees with our explanation, than with his.

Of the sudden extinction of animals, included in our receiver, which I ascribe to the excessive thinness of the air therein, he says, " it seems impossible they should die so soon, merely thro' want of a thick air : " but gives no other reason, than the suddenness of the effect ; which, too, seems grounded upon a mistake : for, the creatures, he mentions, were a bee, a fly, and a caterpillar ; and those included in a small receiver, which could be suddenly exhausted ; and these, indeed, became moveless, within a minute. And tho' these insects did, in so short a time, grow moveless, yet they were not so soon kill'd, as appears by the narrative. The sanguineous animals, that did, indeed, die, were kill'd, more slowly. And having, purposely, enquir'd of a diver, how long he could, before he was accustomed to dive, remain without breathing, or the use of a sponge ; he told me, that at first he could hold out about two or three minutes, at a time : which made me think, that divers become able to continue under water so long, either by a peculiar constitution of body, or, a gradual exercise. And, I am apt to think, that, as 'tis usual, he hereby meant a much shorter time than, when exactly measur'd, it amounts to. For, having made trial upon two live moles, one of them, included in a small receiver, was between two and three minutes in killing ; whereas, the other being detain'd under water, did not there continue full a minute and a quarter, before it finally ceas'd from giving any sign of life. Hence 'tis not impossible, that the want of respiration, should dispatch an animal in as little time, as is mention'd in the experi-

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experiment. And, indeed, our author should either have prov'd it impossible, for the want of air to destroy animals so soon; or have given us some better account of the phenomenon.

'Twere a needless task, to examine any more of our author's objections to particular experiments, since they wholly proceed upon the supposition of his *Funiculus*; which has been sufficiently proved a chimæra: whereas the spring, as well as the weight of the air, is not only allow'd by himself, but demonstrable by experiments uncontroverted betwixt us.



Mr.

of the air. It will bear, therefore, further to prove, when he is unwilling to

And first, that the air is not a simple body, but a compound of various parts, one of which is, that

Weight, we have known by various experiments, one of them is, that

blown bladder, carefully weigh'd, in an exact pair of scales, was found

manifestly heavier, when it was full of air, than when it was empty. Secondly, it has been

and the other, left at the top of the pipe, when the solids were removed

this sort upon opening of the hole, when the solids were removed

again, the excess air, which was drawn up, was found to be

of water.

Thirdly, in the experiment, which I have already mention'd, they found

Fourthly, when a glass bubble, each of which was full of air, was

at was pump'd out, we found that the weight of the bubble, when

Let us now see what Mr. Hobbs says, in his account of the experiment of the

that the scale which contains the bubble, when it is full of air, is

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MR. Hobbs, in disputing against me, seems, generally to have misapprehended my notion of the air. For, when I say the air has gravity, and an elastic power; or, that the air is, in great part, pump'd out of the receiver; 'tis plain, that I take the air, in the obvious sense, for part of the atmosphere, which we breathe; and wherein we move: nor do I find, that any other of my readers understand me otherwise. But Mr. Hobbs thinks he has sufficiently confuted me, if, in some cases, he proves, that there is a subtile substance, or æther, in some places, which I take not to be fill'd with air; and that the æther has, or wants some properties, which I deny, or ascribe to the air: but I do not deny that the atmosphere, or fluid body, which surrounds the terraqueous globe, may, besides the grosser, and more solid corpuscles, wherewith it abounds, consist of a thinner matter, which, for distinction sake, I, also, agree to call æthereal.

The weight and spring of the air asserted.

But he does not, that I remember, deny the truth of any of the matters of fact, I have deliver'd; nor attempt to prove, that the explanations I have given of my experiments, are contradictory to the doctrine I advance: but rather rejects our two grand hypotheses, the weight and spring of

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of the air. It will here, therefore, suffice to prove, what he is unwilling to grant.

And first, that the air, in my sense of the word, is not destitute of weight, we have shewn by various experiments: one of them is, that a blown bladder, carefully weigh'd, in an exact pair of scales, was found manifestly heavier when full of air, than when empty of air.

Secondly, it has, also, been observ'd, that an æolipile, being well heated, and the little orifice, left at the top of the pipe, being stop'd, whilst it was thus hot; upon opening of this hole, when the æolipile was grown cold again, the external air rushing in at the foremention'd orifice, caus'd the æolipile to weigh so much more than it did just before the external air got in, as amounted to near a thousandth part of the weight of an equal bulk of water.

Thirdly, in the *Magdeburgic* experiment, the great receiver they were to exhaust, being weigh'd both before and after the extraction of the air, they found the difference to be 1 ounce $\frac{1}{4}$, "which," says the learned *Schottus*, "is a very clear demonstration of the gravity of the air."

Fourthly, we have weigh'd the air shut up in bodies, in our exhausted receiver, wherein of two materials, different in nature, a blown bladder, and a glass bubble, each equi-ponderant to a more solid weight, before the air was pump'd out, we found that which included a large quantity manifestly to preponderate after the exhaustion. To these we might add other proofs to the same purpose; but these afford such a variety of cases, that it would be superfluous.

Let us now see what *Mr. Hobbs* objects against the experiment of the bladder, weigh'd in the exhausted receiver; for he quarrels not with the rest. "That the scale which contains the bladder, is more depress'd than the other, they may be certain by sight; but that this proceeds from the natural gravity of the air, they cannot be certain, especially if they know not the efficient cause of gravity." But can we not be sure, that lead is, *in specie*, heavier than cork, unless we know the efficient cause of gravity? The reason, he gives, why the bladder outweighs, is this. "That the bladder, whether blown up with a pair of bellows, or with human breath, is heavier than when flaccid, I will not deny, because of the greater quantity of atoms, or of fuliginous corpuscles: but there's nothing certain in this experiment. They ought to have put into the scales two vessels of equal weight, whereof one should be shut, and the other open: for thus air, not blown in, but barely inclos'd, had been weigh'd; when, therefore, air shall be so weigh'd, we will afterwards consider what may be said to the phenomenon." The first part of this passage does not deny the gravity of that we call air, but only endeavours to shew, what parts they are that make it heavy. And, as to the second, he seems to mistake the present case: for there is no need that the air in the bladder, before the exhaustion of the receiver, shou'd be heavier than the outward air. Wherefore, when he subjoins, that from this experiment we can deduce nothing certain, the affirmation is precarious. And *Mr. Hobbs* might

might easily have perceiv'd, that we did make a trial much of the same nature with that he desires : for, we weigh'd the air in a glass, hermetically seal'd, wherein it was not blown in, but barely included. And since, in his elements of philosophy, he grants, and gives his reason for it, that, "if air be blown into a hollow cylinder, or, into a bladder, it will increase the weight of either of them a little ;" and, since here he likewise confesses, that there are mix'd with the æther, many aqueous and earthy particles: he confesses, that the air is not destitute of weight ; and it concerns us no more than himself, to shew how the corpuscles, upon whose account the air is heavy, make it so.

This is all which Mr. *Hobbs*, in several places, thinks fit to object against the gravity of the air ; leaving the experiment of the æolipile, and some others, unanswer'd ; which, alone, prove the air has a manifest weight, even when uncompress'd, and in its laxity. Let us now examine whether the air has not, also, a spring. This, tho' he calls it (as he likewise does the weight of the air) a dream, yet himself, in effect, grants all that is requisite to prove the spring of the air. For, delivering that known experiment, wherein the air is compress'd in a glass bottle, by the forcible injection of water, which, when the glass is unstopp'd, the air again throws out, in recovering its former dimensions; he says, that "the air, with which the spherical glass was fill'd, being mov'd by earthy corpuscles, in a simple, circular motion ; and being compress'd by the force of the injection, that of it, which is pure, gets out into the open air, and gives place to the water. It follows, that these earthy corpuscles have less space left, wherein to exercise their natural motion ; therefore, beating one upon another, they force the water to fly out, when the external air penetrates it, and successively takes up the place of the evacuated air, till the corpuscles of the same quantity of air being restored, regain a liberty natural to their motion." But, to pass by several other of his concessions, to this purpose, we can prove the spring of the air by many phenomena of our engine, of which he offers no other explanation.

If the *Torricellian* experiment be made in a tube, between two feet and a half, and three feet in length ; and if, when the mercury rests at its wonted station, you dextrously stop the orifice of the tube, with your finger, that orifice being rais'd as near the surface of the stagnant mercury, as possible, without admitting the external air ; if, then, you quite lift up the tube, thus stopp'd, into the free air, you shall feel, upon your finger, little or no pressure from the weight of the mercurial cylinder, distinct from the weight of the tube ; because the gravity of the quick-silver, is balanc'd by that of the outward air, which thrusts your finger against it : but, if you invert the tube, and having let in the air at the orifice, stop it again with your finger ; and again let the mercurial cylinder rest upon that finger, you will find your finger strongly press'd, and ready to be thrust away ; which new pressure, since it cannot come from the mercury, nor from the weight of the admitted air, to what can we, rationally, ascribe it, but to the spring of the included air ? And the force hereof will be as well manifest to the

PNEUMATICS eye, as the finger, if the tube be unstopp'd under the surface of the stagnant mercury; for then that in the glass, will not rest, as before, at the usual station, but be depress'd far beneath it. And, if you make the *Torrice-
lian* experiment, in a short, open tube, stopp'd, above and below, with your fingers, upon unstopping the upper orifice, a new, and forcible pressure will be felt upon the finger that stops the lower orifice, made by the gravitation of the external air, which was before kept from resting upon the mercurial cylinder, by the upper finger, the pulp of which, by that gravitating air, was, before, thrust into the deserted cavity of the tube; which demonstrates both the spring of the air, and the gravity of the atmosphere.

But to the experiment of the swelling and shrinking of a bladder, hung in our receiver, as the ambient air is withdrawn, and suffer'd to return; Mr. *Hobbs* replies, "that every skin is made up of small threads, or filaments, which, by reason of their figures, cannot exactly touch in all points. The bladder, therefore, being a skin, must be pervious, not only to air, but to water also; whence, there is the same compresure within the bladder, as without. The endeavour of which (the way of its motions being every way cross) tends to the concave superficies of the bladder; wherefore it must, of necessity, swell every way, and the vehemency of the endeavour increasing, be, at last, torn." But, if this be a sufficient answer to such an experiment, I fear, it will be harder than we are yet aware of; to prove any thing by experiments. For, first, how improbable is it, that such bladders, as we used, are readily pervious to the air; when easy experience shews us, that, by leisurely compressing such blown bladders, betwixt our hands, we shall rather break them, than squeeze out the air at their pores? So that the rest of his answer being built upon what is so repugnant to common experience, will not need a particular confutation: however, we shew, that by the exhaustion of the air, even a glass, hermetically seal'd, was broke: and to say, that glass, also, is pervious to the air, were to affirm what the greatest part of his book supposes to be false. Besides, there is not any sensible, and unquestionable phenomenon, to prove that the receiver is full of any such air, as he speaks of; for we see, plainly, that when the air, manifestly, gets into the receiver, the bladder it not, thereby, made to swell, but to shrink. Moreover, according to Mr. *Hobbs*, the bladder is pervious to the air; and the air, within the receiver, is universally compress'd, as well that which is within the bladder, as that which is without it; how then comes the air, that bears gainst the convex surface of the bladder, not to resist that which is contiguous to the concave superficies of the same; at least, how comes the bladder to be broken by the air, which, according to Mr. *Hobbs*, can get in and out at pleasure? And, lastly, to shew, that to the swelling of the bladder, there needs nothing but the spring of the included air; and no such vehement agitation of the ambient air, as he supposes in our engine; it appears, by the experiment of M. *Paschal*, that, in the free, and ordinary air, a foot-ball, half blown, will gradually swell, the nearer it is carried.

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ed to the top of an high mountain, where the incumbent cylinder of the atmosphere is shorter, and its weight the less; and will, for the contrary reason, grow more flaccid, the nearer it approaches to the foot of the mountain.

Mr. *Hobbs* attempts to explain the phenomena of our engine, by supposing, that "many earthy particles are interspers'd with the air, which have a simple, circular motion, congenite to their nature; and that there is a greater quantity of these particles in the air, near the earth, than remote from it." But this assumption, to me, seems very precarious; for, I know no unquestionable example, or experiment, whereby it can be made out, that any small parcel of matter, has such a simple, circular motion, as he ascribes to each of these innumerable earthy, and, as he adds, aqueous particles. The only argument he here brings to prove, that each atom would have this motion, if all the rest of the earth were annihilated, does not seem clear to me. For, it is not always true, that each minute part of a homogeneous body, has, in every respect, the same qualities with the whole: as the roundness which a small drop of water, or quick-silver, is observ'd to have upon a dry plain, is not to be met with in a large portion of either of these fluids, tho' plac'd upon the same plain. And Mr. *Hobbs*, as well as we, makes the terrestrial atoms in the air to have gravity; a quality that does not properly belong to the whole globe of the earth: nor is it manifest why, because the terrestrial globe moves in a vast circle about the sun, each particular atom of it must describe a small circle in the air, about I know not what center. And, tho' he asserts, that the air, near the earth, abounds with such terrestrial corpuscles, 'tis not likely they should have such a regular motion, as he attributes to them; but, striking against one another, they must, in probability, be put into, almost, as various, and confused a motion, as *Des Cartes* ascribes to his terrestrial particles, swimming in the atmosphere.

Mr. *Hobbs* farther endeavours to prove, that, by the exhaustion of our cylinder, no vacuum is produc'd; and gives a very different account of the experiment itself: he says, that "while the sucker is drawn back, the more space is left within, the less is left to the external air; which being thrust backwards, by the motion of the sucker, towards the outermost parts, moves, in like manner, the contiguous air; and that, the next; and so forwards: so that, of necessity, at last, the air must be compell'd into the space deserted by the sucker, and enter between the convex surface of the sucker, and the concave of the cylinder. For, the parts of the air being infinitely subtile, must insinuate themselves that way by which the sucker is drawn down; since the contact of those surfaces cannot be perfect in all points, because the surfaces themselves cannot be made infinitely smooth: and then, that force, which is applied to draw back the sucker, in some measure distends the cavity of the cylinder; and if, betwixt the two surfaces, one single hard atom should enter, pure air will enter by the same way, tho' with a small force. And thus air, for the same reason, insinuates itself through

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*Mr. Hobbs's
explanation of
the phenomena
of the air-pump,
examin'd.*

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“ the valve of the cylinder ; and, therefore, the retraction of the sucker
 “ will not prove a vacuum. It follows, also, that the air, which is dri-
 “ ven up into the space deserted by the sucker, because it is forcibly im-
 “ pell’d, has a very swift, and circular motion, betwixt the top and the
 “ bottom of the cylinder ; because there is nothing there to weaken its
 “ motion ; and nothing can give motion to itself, or diminish it.” But,
 many exceptions may be made to this reasoning. And, first, I know not
 why Mr. *Hobbs* should, here, confine his discourse to the pump, without
 taking notice of the glass it is design’d to evacuate. We will, therefore,
 consider how he can account for the exhaustion of the receiver, as well as
 of the cylinder, since we usually employ them both together. And he be-
 ing obliged to explain the exhaustion of the one, as well as the other, it
 will be convenient to take into consideration the receiver, because that be-
 ing of glass, and transparent, we can better see what happens in it, than
 in the opaque cylinder. This premis’d, I do not clearly perceive, by this expla-
 nation, how he avoids a vacuum ; for, according to his first words, the
 external air is displac’d by the motion of the sucker outward ; and this
 displac’d air must move that which is next to it ; and that the next ;
 and so onward, till, at length, the air must be compell’d into the space
 deserted by the sucker : so that till this returning air get in betwixt the
 sucker and the cylinder, how appears it, from this discourse, that the de-
 serted space was not empty for some little time ? Certainly all these mo-
 tions of the air, forward and backward, could not be perform’d in an
 instant ; as may appear by the motion of sounds and echoes, whose velo-
 city is reducible to measure. Secondly, tho’ he take his adversaries to be
 vacuists, yet he here supposes the plenitude of the world. I wish, thirdly,
 that Mr. *Hobbs* had declar’d, from whence the return of the air’s impulse
 should begin ; for that may well be requir’d from one, who, making the
 world full, and, for ought appears, fluid, allows us to believe it infinite, if
 the magistrate shall enjoin us that belief. Fourthly, I demand, what ne-
 cessity there is for so forcible a return of the impulse, as is requisite to
 thrust in the air at so narrow a passage as that between the sucker and cy-
 linder ? For, why may not that impulse, when diffused in the vast ambient
 medium, be so communicated, and blended among the different motions
 of the other parts of it, as not to return again from whence it begun ?
 As a voice, tho’ strong, will not move the air, beyond a certain distance,
 smartly enough to be reflected in an echo, to the speaker ; and, as a stone
 cast into a lake, will have the waves, it makes, diverted from returning
 to the place they began at. Fifthly, I do not, likewise, see, that ’tis pro-
 bable, what Mr. *Hobbs* affirms of so thick a cylinder as ours, that it should
 be distended by depressing the sucker. But this I insist not on ; the prin-
 cipal thing, peculiar in Mr. *Hobbs*’s explanation, is, that as much air as is
 driven away by the sucker, presently gets in again, betwixt that and the
 cylinder. But, by the air thus suppos’d to get in, he either means in the
 usual sense, and in ours, the common air, such as we live and breathe in ;
 or, he does not.

If he speaks of such air, I can plainly prove, by several experiments, that our engine is, in great part, destitute of it. For, first, if there be a contrivance made, whereby the whole pump may be cover'd with water, we may, as we have try'd, plainly see the air that is drawn out of the receiver, at each reciprocation of the sucker, pass, in great bubbles, out of the valve thro' the water.

Next, it appears, by the *Magdeburgic* experiment, that, by reason of the recess of the air, the globe of glass, whence it went out, was diminish'd in weight, above an ounce. Thirdly, the same truth may be prov'd by the experiments formerly mention'd, of the swelling of a bladder, and the breaking of an hermetically seal'd glass, upon the recess of the ambient air; these experiments having been already vindicated from Mr. *Hobbs's* very improbable solutions. Fourthly, the same may be prov'd, by the breaking of weak, or ill-figur'd receivers, inwards; of which, on our hypothesis, the reason is clear; but not on his. And, fifthly, what I contend for, may be sufficiently prov'd from this one phenomenon; that tho', if the receiver being full of common air, the key be turn'd under water, the water will not at all ascend at the open orifice; yet the like being done, after the exhaustion of the receiver, we have had several gallons of water violently impell'd into the cavity of the glass: which could not happen, if it were full of air, both in regard there can be no probable cause assign'd why the water should be thus spurted up; and because the receiver being already full of air, either two bodies must be contain'd in one place, and so we must allow penetration of dimensions; or else common air, to which glass is impervious, must pass thro' the water; which, we conclude, it does not, because no such bubbles are made in the external water, as would appear, if common air pass'd thro' it. Nay, so little of this common air was, sometimes, left in the globe used at *Magdeburg*, that when the water was suffer'd to rush in, it reduced the air into less than the thousandth part of the capacity of the globe; and even if our receiver be unstopp'd, not under water, but in the open air, the ambient air will, violently, press in, with a great noise, durable enough to argue, that the glass was far from being full of such air before.

And thus we may argue against Mr. *Hobbs*, if he would have the engine, when we call it exhausted, fill'd with common air; as his words seem to intimate. But because, by some other passages of this dialogue, he may be favourably thought to mean, that the pure air is that which gets in by the sides of the sucker, into the pump, and so into the receiver, let us consider his explanation in this sense also. I desire it may be observ'd, that if Mr. *Hobbs* takes the air in this second sense, he does not oppose what I have deliver'd; the air, I pretend to be pump'd out of the receiver, being the common air, which consists, in great part, of grosser corpuscles, than the æthereal substance. Yet, even this explanation will be liable to the two first inconveniencies, lately objected against the other, in favour of the vacuists; and to several objections besides. I observe, again, that tho' the pump be kept all the while under water, yet

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Mr. *Hobbs*, too, will have the air, impell'd by the sucker, to move very swiftly betwixt the top and bottom of it; as also, when it gets into the cavity of the receiver; yet, when a light bladder is suspended in the cavity of the receiver, it betrays no such motion: nay, the flame of a taper was not blown out, nor stirr'd by this supposed wind; and smoke, produced in the exhausted receiver, was not, by this vehement motion of the air, blown about the receiver. But, if the common external air be admitted at the stop-cock, that, indeed, will rush in with noise and violence, and whirl about the bladder, which hung quietly before.

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In explaining the *Torricellian* experiment, he spends many words to prove, that the place deserted by the suspended mercury, is full of air. But this exposition supposes a plenum: and, if he takes the air in the common sense of the word, 'tis manifestly repugnant, to several phenomena; as that, if the experiment be carefully made, we may, by inclining the tube, impel the mercury from its wonted station to the top; which will not happen, in case the air were, before inclination, let into the deserted space; that if, when the mercury is settled at its usual station, the tube be lifted up out of the stagnant quick-silver, the outward air will drive up the heavy mercurial cylinder, oftentimes, with force enough to beat out the seal'd end; and, lastly, the quick-silver resting at its standard height, if you carefully stop the lower orifice, under the surface of the stagnant quick-silver, and then lifting up the tube into the air, keep it well stopp'd, and first depress one end, and then the other; the quick-silver will fall against the depress'd end of the tube, with a surprizing force and swiftness: whereas, if unstopping the tube, whilst the same quantity of mercury remains in it, you let the outward air into the cavity, unpossess'd by the mercury; and then, again, stop the orifice with your finger, and proceed as before, you shall perceive the motion of the included fluid, to be much slower, and less violent than formerly, by reason of the resistance of the admitted air; which, also, manifestly discloses itself, by the conflict, and bubbles produced betwixt the air and quick-silver, in hastily passing by one another, to the opposite ends of the tube. But, Mr. *Hobbs*, not pretending that any attraction intervenes in the case; I see not how he can possibly make out, to omit other phenomena, the gradual descent of the mercury, in the tube, beneath its wonted station, upon the exhaustion of the receiver; and the re-ascent of the same, in the same tube, as we let in more or less of the outward air, without admitting as much of spring or pressure in the air, as I contend for. The weight of the terrestrial particles, by which he endeavours to account for the quick-silver's falling lower at the top, than at the bottom of a hill, will by no means serve his turn; it being utterly improbable that the air, contain'd in so little a vessel as one of our receivers, can, by its weight, counter-balance so ponderous a cylinder of quick-silver: whence we may be allow'd to argue, that the air sustains it by such a pressure, or spring, as we plead for, whether that proceed from the texture of the aerial particles, from their motion, or from both.

The last of Mr. *Hobbs*'s principal explanations, is of the experiment wherein above 100 pound weight, being hung at the depress'd sucker, the sucker was, notwithstanding, impell'd up again, by the air, to the top of the cylinder. This phenomenon Mr. *Hobbs* accounts for thus. "The air being beaten back by the retraction of the sucker, and finding no void place, wherein to dispose of itself, besides that which it may make, by driving out other bodies, is, by perpetual trusion, at length, forc'd into the cylinder, with so great swiftness, between the concave surface of the cylinder, and the convex surface of the sucker, as may answer
"the

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the power necessary to draw back the sucker. Now, the air within retains the swiftness wherewith it enters; and then, every way, distends the sides of the brass cylinder, which is elastic. Therefore, the air in the cylinder, being vehemently mov'd, thrusts against all parts of the concave surface of the cylinder, tho' in vain, till the sucker is drawn back; but, as soon as the sucker, being loose, ceases to impel the air, that air which was before driven in, will, by reason of its endeavour against every point of the internal superficies of the cylinder, and of the elastic force of the air, insinuate between the same surfaces, with a velocity equal to that by which it was impell'd; that is, with a velocity which answers the strength of the impulse: when, the swiftness wherewith the same air goes out of the cylinder, finding no where to dispose of itself, will, again, impel the sucker to the top of the cylinder; for the same reason that the sucker, a little before, made an impulse upon the air." But this whole conceit, of the air's running in and out, with a strange velocity, between the sucker and the cylinder, is precarious; nor does he propose one phenomenon to countenance it. Farther, we found, in an engine so contriv'd, that the pump lay cover'd with water, when the sucker was drawn back, the atmosphere would strongly press the water against it; and if the handle were let go, swiftly repel up the sucker into the deserted cavity of the cylinder; which being a case parallel to that under consideration, let any unbiass'd person judge how likely it is that the air could perform all these excursions, without exciting bubbles, notwithstanding the water's constant interposition betwixt it and the cylinder. And there is as little probability in what our author teaches of the air's *conatus*. I might here repeat, what we formerly mention'd, of the breaking of our receivers inwards, not outwards; and add, that I see no reason why the *conatus* of the included air, if granted, should be frustrated, when the sucker is depress'd; but, if the air within have so strong an endeavour outwards, as to stretch the thick sides of the brass cylinder, as Mr. Hobbs would have it, I demand, why the air does not throw out the wooden peg, or valve, which we have often, to our trouble, seen thrown out with great force and noise, when, the depress'd sucker being thrust up again, whilst there was air in the cylinder, we forgot to leave the valve open; tho', in this case, the air, that drove out the peg, was far enough from stretching the cylinder. I further demand, how it comes to pass, that if, having stopp'd the hole of the cylinder, with your finger, instead of the peg, you swiftly depress the sucker, you shall be so far from feeling a pressure outwards, against the pulp of the finger, from any thing contain'd in the cavity of the cylinder, that your finger will be strongly press'd in by the ambient air. But, as to the last part of the passage, under consideration, I do not understand why the air that was impell'd in so swiftly, betwixt the cylinder and the sucker, should not resist the swift egress which Mr. Hobbs ascribes to the included air, by the same passage; nor why this impell'd air, that has so strong an endeavour outward, should never depress the sucker, as well as the same air, diffusing its motion thro' the

the vast ambient medium can enable the external air to thrust up the sucker again; especially, since during such a depression of the sucker, made by the rebound of the air, forcibly impell'd in from the close bottom of the cylinder, the air from without, may, all this while, according to Mr. *Hobbs's* principles, get in between the said sucker and the cylinder. But, I further say, that the lifting up of the sucker, either is not necessary to prevent a vacuum; or, that, in some cases, it will be hard to shew how a vacuum can, by Mr. *Hobbs*, be avoided. For, when the depress'd sucker is ready to be thrust up again, if you hang a much greater weight at it, than a hundred, it will not be lifted up at all. And whereas this progress, and regress of the impulse of the external air, cannot reasonably be suppos'd to be very lasting, you may, by a competent weight, detain the sucker depress'd, till the ambient air is as quiet as usual; yet, if you then take off the overplus of weight, or, perhaps, a little more; in case the pump has been very stanch, the sucker, and the great weight hung on, will, nevertheless, be carried up: which 'tis no way likely it could be, by the impulse of the outward air, that had time to decay, and be confounded. And, as for the inward air, we have prov'd, that it has no such endeavour outwards, as Mr. *Hobbs* pretends; otherwise, why should not that throw out the sucker, rather than cause it to be impell'd inward; it being no way likely, that, in case some air should get out of the cavity of the cylinder, it could so move the outward air, as that the reflex of that impulse should make that free outward air bear more strongly against the outside of the sucker, than the inside of the same sucker is press'd against by the included air, whose impetus is, incomparably, less diffused? In short, whatever is resolv'd to be in the cavity of the cylinder, when the sucker is depress'd, yet, since 'tis manifest, that 'tis at least, in great part, destitute of common air; and since the sucker, with the weight, may, if the instrument leak not, be impell'd up, when, in all probability, those forc'd undulations of the air, that may be suppos'd to have been made by the sucker, have ceas'd; the *Cartesians*, Mr. *Hobbs*, and others, who will not have recourse to the unintelligible attraction of some rarified substance within, must ascribe so strange a phenomenon to the pressure of the air without.*

The reason why water ascends in small glass tubes, Mr. *Hobbs* tells us, is, that "the small particles interspers'd in the air, near the water, by their motion beat upon the surface of the water, so that, the fluid must, of necessity, ascend into the pipe; and the more sensibly into one exceeding slender." But 'tis manifest, that I did not, in relating this phenomenon, take upon me to assign the true reason of it. However, I

* That experiment of Mr. *Hauksbee's*, whereby it appears, that 140 pound weight was requisite to separate two hemispheres, $3\frac{1}{2}$ inches in diameter, squeez'd together, by an additional atmosphere of air; proves the pressure of that fluid, beyond all contradiction, against the *Funicular* hypothe-

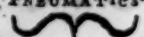
sis, that of suction, &c. especially considering that, when the air was exhausted out of these hemispheres, and an atmosphere injected upon their external surfaces, the weight of 280 pound was necessary to cause their separation. See *Hauksbee's Experiments*, p. 88—93.

am no way satisfied with Mr. *Hobbs's* explanation : for, to say nothing of the motion he ascribes to the particles dispers'd thro' the air, he leaves the difficulty unsolved ; since there being common air, as well within the cavity of the slender pipes, as without, he offers no reason why the pressure of the air within, should not resist the pressure of the same kind of air without, as we see it does in greater pipes. And, possibly, he would have past by this particular, if he had not overlook'd the advertisement I gave, that it would concern those who should undertake to shew the causes of this phenomenon, to consider, also, of a reason why, if the experiment be try'd with quick-silver, instead of water, the surface of that fluid will, instead of being higher, be lower within the pipe, than without it ; whereas, if Mr. *Hobbs's* explication be sufficient, why should not the contrary happen in quick-silver, as well as in water ? The next passage to be consider'd, is this : " If any one, after the frequently repeated impulse, and retraction of the sucker, endeavour to draw out the stopple of the upper orifice of the receiver, he shall find it gravitate very much ; as if a weight of many pounds hung upon it. Whence comes this ? From a strong, circular endeavour of the air within the receiver, made by the violent ingress of the air, between the convex surface of the sucker, and the concave of the cylinder ; procured by the repeated impulse, and revulsion of the sucker, improperly call'd the exsuction of the air : for, by reason of the fulness of nature, the stopple cannot be drawn out ; but whilst that circulates very swiftly, it comes out very hardly ; that is, it seems to be very heavy : for, as soon as fresh air is, by degrees, let into the receiver, it likewise, by degrees, loses this seeming gravity." To this I answer, first, that if there be such a vehement, circular endeavour of the air, in the receiver, by which motion he teaches, that the air rushes out with violence enough to make the atmosphere lift up, in our cylinder, above 100 pound weight ; I see not, why it should not, rather, throw out the stopple, under consideration, than hinder its extraction : and why, when the external air is re-admitted at the stop-cock, into the exhausted receiver, and thereupon there sensibly follows, for a little while, a rotation of the included air ; the stopple, that, just before, seem'd so much to resist the being drawn out, should cease to make any such resistance. Nor do I see how the plenitude of nature should, as is here intimated, hinder the extraction of the stopple ; for, according to the plenists, the world, and the receiver, must be, at all times, equally full. And if the contiguous air must, for Mr. *Hobbs's* reason, necessarily be extracted with the stopple, in one case, I see not why the like should not happen in another. Now, it appears, by our experiments, that there is no such very strong, circular endeavour, in the exhausted receiver, as he pretends ; but, indeed, an endeavour of the ambient atmosphere, to press the parts of the glass inwards, and cover those that are contiguous to it. For, as I have noted already, a light bladder, suspended in the receiver, manifested no such motion, as he here supposes ; neither were a pair of scales suspended within the same ; nor a long magnetical needle, that

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rested upon the point of another needle, at all whirl'd about, by this imaginary motion of the air. Besides, if you leisurely loosen the brass stopple, so that it may be very near, but not contiguous to the sides of the socket, you shall, manifestly, perceive a strong current of air to flow into the receiver, at that passage; and more than once, when, instead of that piece of brass, we stopp'd the hole in the cover, with our cement, we observ'd sometimes whilst we were pumping, sometimes after we had done pumping, that the outward air, by degrees, depress'd the superficies of the cement expos'd to it, and made it concave; and, now and then, would break through it, thrusting it inward with great violence and noise.

Mr. *Hobbs*, afterwards, attempts to rectify another of our explanations, in these words. "We have seen, also, that water let down into the receiver, after some returns of the motion of the sucker, has bubbled so, as if it had boil'd over a fire; which happens by reason of the swiftness of the circulating air: unless, perhaps, you find the water hot too, whilst it bubbles. For if we were sure it was hot, we must find out some other cause of the phenomenon. But we are certain it is not sensibly hot; how, therefore, can the greater, or less motion of the atmosphere, promote such a motion as this?" But, I confess, I see not how the circular motion of the air, within the receiver, could, in a vial, with a long neck, produce such effects as we mention; especially I see not how such a wind, passing along the surface of the water, could raise so many very large bubbles, which seem'd, generally, to rise from the lower parts of the water, thro' the long neck of the vial, and spout it, as it were, into the receiver.

As for what he says, about the gravity of the atmosphere, 'tis plain enough, that my conjecture ascribes the phenomenon to the taking off the pressure of the air within the receiver; tho' I see not why the removal of the weight of the atmosphere, if it could be effected, out of the engine, should not have a like operation. And that the greater, or less pressure of the air concurs to this phenomenon, appears from common experience; for water, by being heated, is expanded, and generates bubbles: and our experiments have made it appear, that there are aerial particles in water, and other liquors, which tend to expand themselves; and actually do so in numerous bubbles, when the pressure of the incumbent air is considerably lessen'd. In the present phenomenon, that pressure being, by the exhaustion of the receiver, taken off, the aerial particles, and agitated vapours abounding in the hot water, are allow'd to expand themselves; and to make such numerous, and great bubbles, as thereby to carry off a large part of the water out of the vial. So that I wonder what makes Mr. *Hobbs* speak, as if there were no sensible heat in the water, under consideration; since 'tis expressly said, that 'twas put in hot; and, if it were put in cold, could, by no pumping, be brought to the least shew of effervescence. And, I have already shewn, by experiments, that there is, in our exhausted receiver, no such peculiar motion of the air, as he ascribes

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the phenomenon to. Nay, when there is, manifestly, a whirling motion of the air in the glass, upon the admission of the external air, the production of numerous bubbles in the water, presently ceases. And, therefore, Mr. *Hobbs* might have let alone my conjecture, unless he could either have disprov'd it better, or substituted a more probable one in its stead.

Mr. *Hobbs*, having recited our experiment of killing animals, in the exhausted receiver, in two or three minutes, asks; "if these animals were so quickly kill'd, for want of air; how do divers live under water: for some of them, accusom'd to it from their childhood, will sustain the want of air for a whole hour? Certainly, that most vehement motion, by which included bladders are distended, and broken, kills these animals." But, I still think, we want some clearer discovery about this matter, notwithstanding what has been deliver'd concerning it, by Mr. *Hobbs*; for, his argument, against my conjecture, is answer'd by himself, since he plainly intimates, that divers, who can live without air for a whole hour, are accusom'd to it from their childhood. Wherefore, unless the animals that died in our engine, had been, for a long time, train'd up, by degrees, to live without air, it will not follow, that the want of it would not soon dispatch them; as ordinary men may be drown'd in a few minutes. And, having purposely let down some mice, and small birds, into a deep glass, fill'd with water; and kept them from emerging, by a weight tied to their legs, or tails; tho' some liv'd longer than others, yet I observ'd them to be kill'd fast enough, to keep my conjecture from being incredible: especially, the last we made trial of, tho' a large and lusty mouse, appear'd to be quite dead, within somewhat less than a minute. And, we particularly took notice, that, before drowning, several bubbles, which seem'd to be the respired air, came out of their mouths, and ascended thro' the water. We have already disprov'd the supposition whereon Mr. *Hobbs's* opinion, as to the cause of the death of animals, in the receiver, is built; for no such vehement motion, as he pretends, is there to be found. Nor does it, at all, clear the difficulty, that he would have this motion the same whereby included bladders are distended, and broken. For, 'tis very hard to conceive, how the tenacity of the air; or its beating from all parts, upon the convex surface of an almost empty bladder, (for, in such, also, the experiment will succeed) should make it burst outwards. And, we have already prov'd, that the distension, and breaking of bladders, in our receiver, proceeds not from any such motion of the neighbouring air, as is here presumed, but from a quite different cause.

Mr. *Hobbs* has a long discourse concerning the extinction of fire, in our receiver, upon exhausting the air; and, he endeavours to reduce what happens to kindled coals, placed in our engine, to what happens in certain mines, wherein, when some thick-damps ascend, both charcoal, and candles are soon extinguish'd. He, also, attempts to shew, that, by the reciprocation of the sucker, the air, impell'd first into the cylinder, and then into the receiver, is put into such a motion, as gives it a certain middle

die consistence, betwixt that of pure air, and that of water. But I shall not need to examine this second part of his discourse, because I deny the first; and being able to disprove the thickness of the air, in the exhausted receiver, I need not spend time about what he teaches of the manner. His history of the damp, to be met with in mines, is more largely set down in his elements of philosophy; where he seems to mention it as a story of doubtful credit, which 'tis likely he would not have done, if he had then seen it; and if he has not, since, observ'd the thing himself; there may easily be a mistake in some of the circumstances: as, for instance, the number of minutes wherein the thick air choaks the fire; and 'tis upon that circumstance, the validity of what he deduces from the observation, chiefly depends. But, however the matter stands with these subterranean damps, we have already prov'd, by several of the experiments of our engine, that, in the exhausted receiver, there is no such motion of the air, as is here supposed. And it may be sufficiently prov'd, that whatever remains in the receiver, is not such a substance as Mr. *Hobbs* would have it; for, he here tells us, it is of a consistence betwixt air and water; and, in his elements, says, 'tis not much lighter than water. But, by the *Magdeburgic* experiment, 'tis evident, that the receiver, by being exhausted of common air, is so far from growing heavier, that it lost above an ounce of its former weight. And to this agrees, what happens in æolipiles, that grow lighter when the air is expell'd. Besides, if the receiver be, in the present case, fill'd with a substance, whose consistence is so much nearer that of water, than is our common air, as Mr. *Hobbs* would have it, why does not a pendulum move sensibly slower in it; when, in water, the vibrations are so exceedingly slacken'd? And, the breaking of an hermetically seal'd bubble, in our receiver, outwards, when the air was much exhausted, and not before, with several other experiments, that might be easily apply'd to this purpose, sufficiently prove, that 'tis not a thicker, and far heavier air, but a more yielding, and lighter, that remains, after pumping, in the cavity of our receiver. But, as for the thing itself, when I related it, I thought it might admit a farther enquiry: and, indeed, there may be many ways of extinguishing fire; so that, as 'tis not, in all cases, easy to assign the true cause of its extinction; 'tis unsafe to conclude, with Mr. *Hobbs*, that because a candle, or a live coal, may be extinguish'd by a thick damp, therefore the effect must proceed from the like cause, in our receiver, where there is no sign of any damp, or unusual thickness of the air, but of the contrary.

Mr. *Hobbs* next, pretends to shew, that neither the spring, nor weight of the air, have any thing to do with the two flat polish'd marbles, not falling asunder in our receiver; "because," says he, "the weight of the atmosphere can do nothing there:" he adds, "that a stronger, or more evident argument could not be brought against a vacuum, than this experiment. For, if two coherent marbles should, either of them, be thrust forwards that way that their surfaces lie contiguous, they

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would easily be sever'd; the neighbouring air successively flowing into the deserted place. But so to pull them asunder, that, at one time, they should lose their whole contact, is impossible; the world being full: for then, either motion must be made from one term to another, in an instant; or, two bodies, at the same time, must be in the same place: to say either of which, is absurd." But how this should be so cogent, and manifest an argument, against the vacuists, I do not well discern; for what it proves, if it prove any thing, seems to be, that in case the cohering marbles could be so sever'd, as to lose, at once, their whole contact, the world might be concluded not to be full; but, I see not how it thence follows, that therefore there can be no vacuum. For my part, I would demand, whether the strong adhesion of the marbles be necessary, or not, to the plenitude of the world? If it be, how chance a sufficient weight, hung to the lower marble, can immediately draw them asunder? And, if it be not, why does not Mr. *Hobbs* assign some other cause of their so strong adhesion, if it depend neither upon the spring, nor weight of the air? As for the non-separation of the two marbles, in our receiver, I have said, that the cause may, probably, be the pressure of the air, remaining in the receiver; for, 'tis no way unlikely, that the remaining air should be able to sustain a weight of four or five ounces, hanging at the lower marble; since the free air was able to support between four and five hundred ounces, hanging at the same.

But Mr. *Hobbs* tells us, that the cause I assign of the cohesion of our marbles, is liable to great inconveniencies; "for they confess," says he, "that all ponderation is an endeavour, every way, by right lines, to the center of the earth: so that it is made not by the figure of a cylinder, or column, but by a pyramid, whose top is the center of the earth; and whose basis is part of the surface of the atmosphere." As if it were material whether a body, whose basis is scarce two inches diameter, and length some thousands of miles, be consider'd as a cylinder, or a pyramid. Certainly, *Stevinus*, and other learned hydrostatical writers, would scarce have made this an objection; since they scruple not to postulate, that all perpendiculars, not very distant, be look'd upon as parallel; tho' they allow such perpendiculars would meet in the center of the earth. What he adds, "that, therefore, the endeavour of all the points, which ponderate, will be propagated to the surface of the upper marble, before it can be propagated further to the earth;" to prove that the whole endeavour of the pyramid, that rests upon the upper marble, is terminated there; and that there is no endeavour of the atmosphere, against the under superficies of the lower-most marble; seems grounded, partly, upon a conceit of his, about the nature of gravity: according to which, I see not why any body, plac'd between the sides of that pyramid, or cone, whereof the upper superficies of the higher marble is the basis, should descend upon the account of gravity; and, partly, from a mistake of my opinion: for, I no where speak as if

I thought the lower marble was sustain'd by little globules, or other minute bodies, protruding one another, directly towards the center of the earth, and rebounding from a perfectly smooth surface. Nor, need I say, that the lower stone is sustain'd by the pressure of the same pillar of the atmosphere, as rests on the upper; since other parts of the atmosphere, some on the one hand, and some on the other, pressing obliquely upon the uneven surface of the earth, may have their pressure upward, terminated against the lower surface of the undermost marble. Thus, an æolipile being, by heat, well freed from air; if the little orifice, at the extremity of the neck, by which the air gets in and out, be soon carefully stopp'd with wax, and afterwards suffer'd to cool, there will not be, in the cavity of the æolipile, a resistance near equal to the pressure of the outward air. And, therefore, if you perforate the wax, that air will be violently impell'd in, at the unstopp'd orifice, whether the neck be held parallel, or perpendicular to the horizon; or, in any other situation, in respect of the center of the earth. And the like will happen, if the æolipile be unstopp'd under water.

And, I lately met with the relation of an experiment, which abundantly makes out the power of the ambient atmosphere, to press bodies against each other, when it cannot get between their internal surfaces. The ingenious author of the *Magdeburgic* experiment, writes to the industrious *Schottus*, that having caus'd two copper plates to be made, almost in the form of scales, a little above half an ell in diameter, and exactly fit; if when laid upon one another, "the air be drawn out, they are kept so compress'd, and united, by the gravity of the external air, that six strong men cannot pull them asunder; but if, at length, they are disjoin'd, they make a noise equal to the report of a musquet; yet as soon as ever there is the least entrance given to the air, they fall asunder of their own accord." And, if a glass vial have a pipe open at both ends, so fitted to it, that no air can get in or out, betwixt the neck and it; and if the vial be so far fill'd with water, that the lower-end of the pipe be well immers'd therein; if you suck at the upper-end of the pipe, the water will ascend to a great height; which argues its being forc'd upwards, by the oblique pressure of the air in the vial: for 'tis only in the pipe, and not in the vial, that there is any air in the same perpendicular with the water that is impell'd.

As to the ascent of liquors by suction, Mr. *Hobbs* says, "whoever sucks water into his mouth by a pipe, first sucks up the air between; whereby he removes the external distended air, which can have no place, but by removing the next; and so, by continual pulsion, the water is, at length, driven into the pipe, and succeeds the air which is suck'd out." And this account of the phenomenon is, also, given by the learned *Gassendus*, and other atomists; and seems generally acquiesc'd in by the modern philosophers. But, though I deny not, that many

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many phenomena of nature, may be, probably, explain'd by this propagation, and return of motion; yet there are some others, here below, which I see not how the *Cartesians*, the atomists, or Mr. *Hobbs*, can solve, without admitting the spring of the air; and which, perhaps, may, by the spring of the air, be explain'd, without recurring to such a propagation, and return of impulse. I took a glass vessel, consisting of two parts; the one, a vial, capable of containing about a pound of water; and the other, a pipe, open at both ends; the lower of which reach'd within two inches of the bottom of the vial: this pipe was fasten'd into the neck of the vial, with melted glass, of a good thickness; and into the vessel, by the open pipe, I pour'd water enough to swim a pretty way above the lower-end of the pipe; and then often inclin'd the vessel to give a free intercourse betwixt the air within the vial, and that without it: whence, if the external air were compress'd by the affusion of the water, it might free itself, as it readily did, by ascending in bubbles along the inclined pipe, till the outward and inward air were reduced to an equality of pressure. Now, if all suction were produced by the pressure of the air, thrust away by the dilated chest of him that sucks, and so thrusting the fluid into the pipe, it seems evident, in our case, that the water would not ascend by suction; since by the contrivance of the vessel, the air thrust away by him that sucks, cannot, at all, come to press upon the water. Yet, whether the pipe were inclin'd, or erect, the water ascended, upon suction, to the top of the pipe, and ran over into my mouth, and that with tolerable ease: for tho' the spring of the air, pent up in the vial, were able, upon the decrease of the pressure of the outward air, occasion'd by my sucking, to impel the water strongly into the pipe; yet, when a moderate quantity of water had been so impell'd, the included air, gaining, thereby, more room to expand its spring, was so much weaken'd, that the water ascended far less easily than in ordinary suction. And, to shew that the ascent of the water, in the pipe, did not proceed from any such tendency, in the water itself, to ascend, for prevention of a vacuum; I carefully took out the water, by degrees, till the lower-end of the pipe lay but very little under the surface of the water; tho' in the cavity of the pipe, the water was considerably higher: then suffering the vessel to rest, and sucking at the upper-end of the pipe, the water was impell'd up, yet without reaching near the top, till the surface of it was fallen a little below the bottom of the pipe. But then, though I continu'd sucking, no more water ascended into the pipe; but the air passing thro' it, towards my mouth, in its passage toss'd up the water that was already in the pipe, and turn'd it into bubbles, which broke with noise one after another. And thus the ascending air long kept the water, in the pipe, from falling back to that in the vial; but, when I remov'd my mouth, the spring of the air, remaining in the cavity of the vial, being weaken'd by the recess of the air, I had suck'd out,

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it was not able to resist the pressure of the outward air; and, accordingly, the water in the pipe, was not only depress'd into the vial, but the external air forc'd its way, in many bubbles, and not without some noise, through the water contiguous to the bottom of the pipe, till the pressure of the included air, and that of the atmosphere, were reduced to an equality.



VOL. II.

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RE-

R E M A R K S

UPON

Mr. *H O B B S*'s Problems,

ABOUT A

V A C U U M.

I Here only propose to consider, whether Mr. *Hobbs* has cogently prov'd his assertion, that there is no such thing as a vacuum ; and whether he has rightly explain'd those phenomena of nature, which he undertakes to solve ; and especially those produc'd by means of our engine.

Arguments a-
gainst a va-
cuum consider'd.

And, first, as I account for the adhesion of two flat polish'd marbles, by the pressure of the air, Mr. *Hobbs* thinks this phenomenon so fully proves the plenitude of the world, that tho' he tells us, he has many strong arguments to make it out, yet he mentions but this one, as abundantly sufficient ; for, says he, since the vacuists allow interspers'd vacuities, not only in the air that surrounds the join'd marbles, but in the rest of the ambient air, there is no reason why there should be any difficulty in separating the marbles ; or, at least, any greater difficulty than in moving them in that air, after their separation. But they will easily answer, that, notwithstanding the vacuities they admit in the ambient air, a manifest reason may be given, on their hypothesis, of a difficulty in the divulsion of the marbles : for, the vacuities they admit, being but interspers'd, and very small ; and the corpuscles of the atmosphere being, according to them, endow'd with gravity, there rest so many upon the upper surface of the marble, that this stone cannot be, at once, perpendicularly drawn up from the lower, contiguous to it, without a force able to surmount the weight of the aerial corpuscles that press upon it. And this weight has already so condens'd the neighbouring parts of the ambient air, that he, who would per-

perpendicularly raise the upper marble from the lower, needs a considerable force to make the revulsion, and compel the contiguous parts of the incumbent air, to enter the pores, or intervals intercepted between them. For, the force of him, who endeavours to raise the upper marble, whilst the lower surface of it is fenc'd from the pressure of the atmosphere, by the contact of the lower, which suffers no air to come in between them, is not assisted by the weight, or pressure of the atmosphere; which, when the marbles are once separated, pressing as strongly against the under surface of the upper marble, as the incumbent atmospherical column does against the upper surface of the same marble; the hand which endeavours to raise it, in the free air, has no other resistance, than that small one of the marble's own weight to surmount.

And as for the reason which Mr. *Hobbs*, and, as he thinks, all others give of the difficulty of this divulsion, "that the whole space between the two separated marbles, cannot be instantaneously fill'd by the air, let the separation be ever so sudden;" the plenists may give a more plausible account of this experiment, than he has here done; and, therefore, abstracting from the two opposite hypotheses, I may say, that the genuine cause of the phenomenon seems to be that which I have already assign'd; and that the difficulty of raising the upper stone, attending the air's not being able to come in all at once, to possess the space left between the surfaces of the two marbles, upon their separation, proceeds from hence; that, till this space be fill'd with the atmospherical air, the hand, which would raise the upper marble, cannot be fully assisted by the pressure of the air, against the lower surface of that marble: for, since I declare not for the hypothesis of the plenists, as 'tis maintain'd by Mr. *Hobbs*, I am not bound to allow, what the common explanation, adopted by my adversary, supposes, that either nature abhors a vacuum; or, that there could be no divulsion of the marbles, unless at the same time, the air were admitted into the space that divulsion makes for it. And a vacuist may say, provided the strength employ'd, to draw up the upper marble, be able to surmount the weight of the aerial corpuscles, accumulated upon it; the divulsion would ensue, though no air, or other body, should be permitted to fill the room made for it, by the divulsion; and that the air's rushing into this space, does not, necessarily, accompany, but, in order of nature, and time, follow, upon a separation of the marbles; the air that surrounded their contiguous surfaces, being, by the weight of the collateral air above, impell'd into the room newly made by the divulsion. But, having purposely, in our pneumatic receiver, accommodated two flat and polish'd marbles, so that the lower being fix'd, the upper might be laid upon it, and drawn up again, as there should be occasion; I found, that if, when the receiver was well exhausted, the upper marble was, by a certain contrivance, laid flat upon the lower, they would not then cohere, as before, but, with great ease, be separated; tho' it did not, by any phenomenon, appear, that any air could rush in, to possess the place given it by the recess of the upper marble; whose

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But the cause Mr. *Hobbs* assigns for this phenomenon, is improbable; and a better has been assign'd already. For, first, he requires, to the divulsion of the marbles, a force great enough to surmount the hardness of the stone. But, this is *gratis dictum*; and, it seems very unlikely, that so small a weight, as will suffice to separate two coherent marbles, of about an inch, for instance, in diameter, should be able to surmount the hardness of such solid stones, as we usually employ in this experiment. And, tho' it be, generally, judg'd more easy to break a broader piece of marble, *ceteris paribus*, than a much narrower; yet, neither I, nor, I believe, Mr. *Hobbs*, ever observ'd any difference in the resistance of marbles to separation, arising from the greater or less thickness of the stones. Yet, I find, by constant experience, that, *ceteris paribus*, the broadness of the coherent marbles, exceedingly increases the difficulty of disjoining them.

But Mr. *Hobbs*, upon the supposition of the world's plenitude, illustrates our phenomenon, by drawing asunder the opposite parts of a piece of wax: a very improper instance, surely! For, the parts to be divided in the wax, are of a soft, and yielding consistence, and, according to him, of a ductile nature; and not, as the parts of the coherent marbles, very solid and hard. The parts of the wax, also, do not stick together, barely, by a superficial contact of two smooth planes, as do the marbles we are speaking of; but have their parts intangled with one another; and, therefore, they are far from a disposition to slide asunder, like the marbles. Besides, 'tis manifest, that the air has opportunity to succeed in the places successively deserted by the receding parts of the attenuated wax: but 'tis neither manifest, nor well proved by Mr. *Hobbs*, that the air, after the same manner, succeeds between the two marbles; which, as I lately noted, are not forced asunder, after such a manner; but, as himself speaks, sever'd, in all their points, at the same instant.

In the second place, a better solution of the phenomenon has been already given, from the pressure of the atmosphere, upon all the superficial parts of the upper marble, except those that touch the plane of the lower. And to shew, that when two coherent marbles are sustain'd, horizontally, in the air, the cause why they are not to be forc'd asunder, if they have two or three inches in diameter, without the help of a considerable weight, is the pressure of the ambient air; I caus'd two such coherent marbles to be suspended in a large receiver, with a weight at the lower, that might help to keep them steady; but was very inconsiderable to that which their cohesion could have surmounted: then causing the air to be pump'd, by degrees, out of the receiver, the marbles long stuck close together; because, during that time, the air could not be so far exhausted, but that there remain'd enough to sustain the small weight, which endeavour'd their divulsion. But, when the air was further evacuated, at length, the spring
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of the little expanded air, that remain'd, being grown too weak to sustain the lower marble, and its small clog, they dropp'd off. And, to confirm my explanation of our phenomenon, I shall add, that as this trial, which I had, several times, occasion to repeat, shews the cohesion of our two contiguous marbles, would cease, upon withdrawing the pressure of the atmosphere; so, by another experiment, it appears, that such a pressure sufficed to cause that cohesion: for, having found, that when the receiver was well exhausted, two marbles, tho' considerably broad, being laid upon one another, after the requisite manner, their adhesion became so weak, that the upper would be easily drawn up from the other, we laid them again one upon the other; and then letting the external air flow into the receiver, we found that the marbles now adhered so well together, that we could not raise the upper without the lower.

But, farther, Mr. *Hobbs*, in arguing against our engine, unjustly supposes the whole design of it to be the proof of a vacuum; for, or against which, I have never yet declared.*

Besides, he seems not to have rightly understood, or, at least, not to have sufficiently regarded, in what, chiefly, consists the advantage which the vacuists may make of the air-pump, against him. In several places he is very solicitous to prove, that the cavity of our pneumatical receiver, is not, altogether, empty; but, the vacuists may tell him, that, since he asserts the absolute plenitude of the world, he must reject, not only great vacuities, but, also, those very small, and interspers'd ones, which, they suppose, are intercepted between the solid corpuscles of other bodies, and particularly

* The most cautious, and reserv'd philosopher, need, now, make no scruple to assert a vacuum. Sir *Isaac Newton* has furnish'd us with several arguments, which are decisive of this point. Those, indeed, who make the essence of matter to consist in extension, may well deny a vacuum; but the nature of gravity, which is proportionable to the matter, or solid content of bodies, fully demonstrates, much the largest part of the universe to be empty space. The motions of the comets, also, demonstrate a vacuum; for, these move in all manner of planes, and directions, thro' the vast celestial spaces, without any sensible resistance. The vibrations of pendulums confirm the same; for, if they be made to move in a space free from air, they, likewise, meet with no resistance; and, therefore, there is no sensible matter remaining in that space. If there were no vacuum, a projectile in the air, or, in a space destitute of air, would move as slowly as in quick-silver; for, the resist-

ance of fluid mediums is nearly in proportion to their density: and, 'tis a great mistake, to think that the resistance of projectiles will decrease in *infinitum*, by an infinite division of the parts of the fluid; for, 'tis plain, that resistance cannot be considerably lessen'd, by dividing the parts of the fluid. For, why should not the same quantity of matter have the same power of resistance, when divided into many subtile parts, as when divided into a few that are larger? A sphere, moving in a compress'd fluid at rest, and of the same density with itself, would lose half its motion, before it had gone twice the length of its diameter.

In another place, the same great philosopher argues thus. "The resistance of water arises principally, and, almost, intirely, from the *Vis inertia* of its matter; and, by consequence, if the heavens were as dense as water, they would not have much less resistance than water; if as dense as quick-silver, they would

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larly, of the air: whence it would not confute them to prove, that, in our receiver, when most thoroughly exhausted, there is not one great and absolute vacuity; or, as they speak, a *vacuum coarctatum*; since smaller, and diffeminated vacuities would serve their turn. And, therefore, they may think their pretensions highly favour'd, as by several particular effects, so by this general phenomenon of our engine, that the common, or atmospherical air, which, before the pump was work'd, possess'd the whole cavity of our receiver, is, by the contrivance of the pump, made, in a great measure, to pass out of the cavity into the open air, without being able, at least, for a while, to get in again; yet it does not appear, by any thing Mr. *Hobbs* has alledg'd, that any other body succeeds, adequately to fill the spaces deserted by such a multitude of aerial corpuscles.

Glas impervious to air.

And, tho' he endeavours to prove our receiver to be always full of air; yet, that the common air cannot enter thro' the pores of glass, appears by the following experiment. We took a bubble of thin white glass, about the bigness of a nutmeg, with a very slender stem, four or five inches long, and of the thickness of a crow's quill; and holding the end of this stem in the flame of a lamp, blown with a pair of bellows, it was readily seal'd up; and presently the spherical part of the glass being held by the stem, was kept turning in the flame, till it became red-hot, and ready to melt; then, being a little removed from the heat, as the included air began to lose of its agitation, and spring, the external air manifestly, and considerably press'd in one of the sides of the bubble. But the glass being again,

" would not have much less resistance
 " than quick-silver; if absolutely dense,
 " or full of matter, without any vacuum,
 " let the matter be never so subtle and
 " fluid, they would have a greater resist-
 " ance than quick-silver. A solid globe,
 " in such a medium, would lose above
 " half its motion, in moving three times
 " the length of its diameter; and, a globe,
 " not solid, such as are the planets, would
 " be retarded sooner. And, therefore,
 " to make way for the regular, and last-
 " ing motions of the planets, and co-
 " mets, 'tis necessary to empty the hea-
 " vens of all matter, except, perhaps,
 " some very thin vapours, steams, or effluvia,
 " arising from the atmosphere of the earth,
 " planets, and comets. A dense fluid can
 " be of no use for explaining the pheno-
 " mens of nature; the motions of the pla-
 " nets, and comets, being better explain'd
 " without it. It serves only to disturb,
 " and retard the motions of those great
 " bodies, and make the frame of nature
 " languish: and, in the pores of bodies, it

" serves only to stop the vibrating motion
 " of their parts, wherein their heat and
 " activity consists. And, as it is of no use,
 " and hinders the operations of nature,
 " and makes her languish; so there is no
 " evidence for its existence, and, there-
 " fore, it ought to be rejected."

" If all space, was equally full, the
 " specific gravity of the fluid, where-
 " with the region of the air is fill'd,
 " would, by reason of the vast density
 " of its matter, be as heavy in specie,
 " as quick-silver, gold, or any other
 " the most dense body; whence neither
 " gold, nor any other matter, would
 " descend in that air: for, no bodies
 " descend in fluids, that are not speci-
 " fically lighter than they."

"Tis farther demonstrable, that all bo-
 " dies have more pores, than solid parts;
 " and, that some have infinitely more pores
 " than others; whence arises another proof
 " of a vacuum. See *Newton. Optic.* p. 310,
 " 311, &c. *Princip.* p. 316, 317. 328. 342,
 " 343. 368.

before

before the cold crack'd it, held, as before, in the flame; the rarified air distended and plump'd up the bubble; which, being the second time remov'd, was a second time compress'd; and being, the third time, brought back to the flame, swell'd as before, and removed, it was again compress'd; till, at length, having satisfied ourselves, that the included air was capable of being condens'd, or dilated, without the ingress or egress of air, properly so called, we held the bubble so long in the flame, strengthen'd by nimble blasts, that not only it had its sides plump'd up, but a hole violently broken in it, by the over-rarified air, tho', till then, it was no way crack'd.

Hence, it easily appears, how impervious our thick pneumatic receivers are to common air; since a thin glass bubble, when its pores were open'd, or relax'd by flame, would not give passage to the springy particles of the air, tho' violently agitated: for, if these particles could have got out of the pores, they never would have broke the bubble; nor, probably, would the compression, that afterwards ensued, of the bubble, by the ambient air, be check'd near so soon, if those springy corpuscles had not remain'd within to make resistance.

Yet, what I principally design'd, in this experiment, was, to shew, and prove at once, by an instance not liable to the ordinary exceptions, the true nature of rarification, and condensation, at least, of the air: for, 'tis here plain, that when the bubble, after the glass had been first thrust in, towards the center, was expanded again by heat, the included air possess'd more room than before; yet it could perfectly fill no more room than before; each aerial particle taking up, both before and after the heating of the bubble, a portion of space adequate to its own bulk: so that in the cavity of the expanded bubble, we must either admit vacuities interspers'd between the corpuscles of the air; or allow, that some fine particles of the flame, or other subtil matter, came in, to fill up those interstices; which matter must have enter'd the cavity of the glass at its pores. And, afterwards, when the red-hot bubble was remov'd from the flame, it is evident, that since the grosser particles of the air could not get thro' the glass, which they were unable to do, even when vehemently agitated by an ambient flame, the compression of the bubble, and the condensation of the air, necessarily consequent upon it, cou'd not, supposing the plenitude of the world, be perform'd without squeezing out some of the subtil matter, contain'd in the cavity of the bubble, whence it could not issue but at the pores of the glass.

Mr. Hobbs is pleas'd to compare our pneumatic engine to a pot-gun, and attributes the phenomena, exhibited in the exhausted receiver, to the expansive endeavour of the air outwards: I shall, therefore, shew, that there is, in our exhausted receiver, no such strong endeavour outwards, as he supposes; but that the weight of the atmospherical air, when 'tis not resisted by the counter-pressure of any internal air, is able to perform what a weight of many pounds would not suffice for. Glass not being a yielding body, cannot, by the alteration of its figure, from an external uniform pressure, shew when such an one is exercis'd upon it; and, therefore,

The nature of
rarification, and
condensation.

The pressure of
the air mani-
fested to the eye.

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fore, instead of a receiver of glass, we provided one of pewter, and applied it to the engine, after the usual manner. And tho' the inverted vessel, by reason of its stiffness, thickness, and the convexity of its superficies, were strong enough to have supported a great weight, without changing its figure; yet, as soon as by an exsuction or two, the remaining part of the included air was brought to such a degree of expansion, that its weaken'd spring was able to afford little assistance to the tenacity and firmness of the metal, the weight of the pillar of the incumbent atmosphere, presently depress'd the upper-part of the vessel; at once lessening its capacity, and changing its figure; so that, instead of the convex surface, it gain'd a concave one. The experiment succeeded, also, with a common pewter porringer. And, sometimes I found, also, that the vessel would be thrust in, not at the top, but on the side; if that were the only part made too thin to resist the external pressure.

Whether air penetrates quick-silver, in the Torricellian experiment.

Mr. Hobbs, afterwards, proceeds to the *Torricellian* experiment; which he will needs have perform'd by means of air in the tube; and attempts to prove, that air may pass thro' quick-silver, because a blown bladder, forcibly detain'd under it, will, of itself, emerge, when the detaining force is remov'd.

This, I confess, is surprizing. It concern'd Mr. Hobbs to prove, that as much air as was displac'd by the descending mercury, did, at the orifice of the tube, immers'd in stagnant mercury, invisibly ascend to the upper-part of the pipe; and, he tells us, that a bladder full of air, being depress'd in quick-silver, will, when the hand that depress'd it is remov'd, be squeez'd up by the very weight of the mercury; whence it follows, that air may penetrate quick-silver. But, who ever deny'd that air, surrounded with quick-silver, may, thereby, be squeez'd upwards? And, since even very small bubbles of air, may be seen to move in their passage thro' mercury, how will this example help Mr. Hobbs? For, 'tis by mere accident that the air, included in the bladder, comes to be buoy'd up, because the bladder itself is so; and, if it were fill'd with water, instead of air; or with stone, instead of water; it would, nevertheless, emerge, as himself confesses, if it were iron, or any matter, except gold; because all other bodies are lighter in specie, than quick-silver. But, since the emersion of the bladder is manifest enough to the sight, how does this prove, that the air gets into the *Torricellian* tube invisibly; since our eyes discover no such motion of the air, which must not only pass unseen thro' the sustain'd quick-silver, but, likewise, imperceptibly dive, in spite of its comparative lightness, beneath the surface of the stagnant mercury, to get in at the orifice of the erected tube.

But, to clear up this matter, having made the *Torricellian* experiment in a strait tube, after the ordinary manner, we took a piece of fine bladder, and, raising the pipe a little in the stagnant mercury, but not so high as the surface thereof, we dextrously convey'd it into the quick-silver, so as to be apply'd by the finger to the immers'd orifice of the pipe, without letting

ring the air get into the cavity of it; then the bladder was, cautiously, ty'd very tight to the lower end of the pipe, whose orifice it cover'd before, and the pipe, being now slowly lifted out of the stagnant mercury, the sustain'd quick-silver appear'd to press, but very lightly upon the bladder; being so near an exact equilibrium with the atmospherical air, that if the tube were but a very little inclin'd, whereby the perpendicular gravitation of the quick-silver came to be somewhat lessen'd, the bladder would immediately be driven into the orifice of the tube, and to the eye, plac'd without, appear to have acquir'd a concave superficies, instead of the convex it had before. And, when the tube was again erected, the bladder would no longer appear suck'd in, but be again somewhat protuberant. And, if, when the mercury in the tube was made to descend, a little below its station, into the stagnant quick-silver, the piece of bladder were, at the juncture, nimbly and dexterously apply'd, as before, to the immers'd orifice, and fasten'd to the sides of the pipe; upon lifting the instrument out of the stagnant mercury, the cylinder of quick-silver being now somewhat short of its due height, was no longer able, fully to counterpoise the weight of the atmospherical air; which, consequently, tho' the glass were held erect, would press up the bladder into the orifice of the tube, and cause a cavity, sensible both to the eye and touch.

This experiment fully shews, that the pressure of the external air is able to sustain a cylinder of twenty-nine or thirty inches of mercury, and, upon a small diminution of the gravity of that ponderous fluid, to press it up higher into the tube. But a farther use may be made of it against Mr. Hobbs. For, when the tube is again erected, the mercury will subside as low as at first, and leave as great a space, as formerly was left deserted at the top; into which, how the air should get to fill it, will not appear easy to them, who know, that a bladder will rather be burst by air, than afford it passage. And if it should be pretended, either, that some air from without had got thro' the bladder, or, that the air presum'd to have been, just before, included between the bladder and the mercury, made its way from the lower part of the instrument to the upper; we reply, 'tis no way likely, that it should pass all along the cylinder, unperceiv'd by us; since when there are really any aerial bubbles, tho' smaller than pins heads, they are easily discernible. And, in our case, there is no such resistance of the air to the ascent of the stagnant mercury, as Mr. Hobbs pretends in the *Torricellian* experiment, made after the usual way.

But further, we took a cylindrical pipe of glass clos'd at the upper end, and so long, that being dexterously bent at some inches distance from the bottom, the shorter leg was made parallel to the longer. In this glass, we found an expedient to make the *Torricellian* experiment; the quick-silver in the shorter leg serving instead of the stagnant portion in the usual baroscope; and that in the longer leg, reaching above the mercury in the shorter about eight or nine and twenty inches. Then, by another artifice, the shorter leg, into which the mercury did not rise within an inch of the top, was so order'd, that it could in a trice be hermetically seal'd up; an inch of common air be-

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ing left in it, without disordering the mercury: and having, in this manner, shut up such a quantity of uncompress'd air, we warily held a pair of heated tongs near the outside of the glass, whereby, the air being agitated, was enabled to expand itself to double its former dimensions; and, consequently had its spring so strengthen'd, that it was able to raise all the quick-silver in the longer leg, and sustain a mercurial cylinder above nine and twenty inches high; when, were it not for the heat, it would have lost half the force of its elasticity.

Now Mr. *Hobbs* will find it very difficult to shew, what keeps the mercury suspended in the longer leg of such a barometer, when the shorter leg is unstop'd, at which it may run out; since this instrument is portable. And when the shorter leg is seal'd, it will be very hard for Mr. *Hobbs* to shew there the odd motions of the air, to which he ascribes the *Torricellian* experiment. For if you warily incline the instrument, the quick-silver will rise to the top of the longer leg, and immediately subside, when the instrument is again erected; and yet no air appears to pass thro' the quick-silver interpos'd between the ends of the longer, and the shorter leg. But that, which I would chiefly take notice of in this experiment, is, that upon the external application of a hot body to the shorter leg, when seal'd up, the included air was expanded from one inch to two; and so rais'd the whole cylinder of mercury in the longer leg; and whilst the heat continu'd undiminish'd, kept it from subsiding again. For if the air were able to get, unseen, thro' the body of the quick-silver, why had it not been much more able, when rarify'd by heat, to pass thro' the quicksilver, than for want of doing so, to raise and sustain so great a weight of mercury?

How water comes to ascend in glasses after some of their air is drawn out.

The last thing attempted by Mr. *Hobbs*, in his problems, is to account for the rise of water into a vial plung'd therein, with the mouth downwards, after some air has been suck'd out of it: where he demands, upon supposing a *Vacuum*, and, consequently, interspers'd vacuities in the air of the vial, "how it happens, that the water would not ascend before the suction was made?" To this the vacuists will easily answer, by acknowledging, that there were, indeed, interspers'd vacuities in the air contain'd in the vial, before the suction; but adding that there was no reason why the water should ascend to fill them, because, being a heavy body, it cannot rise of itself, but must be rais'd by some prevalent weight or pressure, which was then wanting. Besides, there being interspers'd vacuities, as well in the rest of the air that was very near the water, as in that contain'd in the vial, there is no reason why the water should ascend to fill the vacuities of one portion of air, rather than that of another. But, when once, by suction, many of the aerial corpuscles were made to pass out of the vial, the spring of the remaining air being weaken'd, whilst the pressure of the ambient air, which depends upon its constant gravity is undiminish'd, the spring of the internal becomes unable to resist the weight of the external air; which is therefore able to impel the interpos'd water, with some violence, into the cavity of the glass, till the air, remaining in that cavity, being reduced, almost, to its usual density, is able, by its spring, and the weight of the

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the water got up into the vial, to hinder any more from ascending. For, as to what Mr. *Hobbs* affirms; that “the person, who sucks the vial, draws “nothing into his stomach, lungs or mouth;” how he will reconcile this with what he elsewhere delivers about suction, I leave him to consider. I cannot, however, but wonder at his confidence, who can positively assert a thing so repugnant to the common sentiments of men of all opinions, without offering any proof for it. But, I suppose, they who are, by trial, acquainted with suction, and have felt the air come in at their mouth, will prefer their own experience to his authority. And as to what he adds, that the person who sucks, agitates the air, and turns it within the vial into a kind of circulating wind, that endeavours every where to get out; I wish he had shewn us, by what means a man in sucking makes this odd commotion in the air, especially, in such vials as I employ about this experiment, the orifice whereof is sometimes less than a pin’s head.

But that real air may be extracted, by suction, out of a glass, appears by an experiment made with a receiver exhausted by our pump; and, consequently by suction. For, when we had counterpois’d it in very nice scales, and, afterwards, by turning a stop-cock, let in the outward air, there rush’d in as much to fill the space deserted by the evacuated air, as weigh’d some scruples, tho’ the receiver were not of the largest size.

Mr. *Hobbs* pretends, that as soon as the neck of the vial is unstopp’d under water, the air that whirl’d about before, makes a sally out, and forces in as much water. But if the orifice be any thing large, you, will, instead of feeling an endeavour to thrust away your finger that stop’d it, find the pulp of it, so thrust inward, as to appear to be suck’d in. And this may be the reason why the lip of him who sucks, is often strongly fasten’d to the orifice of the vial’s neck; which Mr. *Hobbs* ascribes to a most exquisite contact, but without clearly telling us how that extraordinary contact is effected. And, when your finger is remov’d, instead of perceiving any air go out of the vial thro’ the water; which, if any such thing happen’d, would easily be discover’d by the bubbles; you shall see the water briskly spring up in a slender stream to the top of the vial; which it could not do, if the cavity were already full of air. And, to prove, when the air really passes in and out of the vial immers’d under water, that ’tis very easy to perceive its motions; if you dip the neck of the vial in water, and then apply to the globular part of it, either your warm hands, or any other competent heat; the internal air being rarify’d, a portion of it, answerable to the degree of heat apply’d, will manifestly pass thro’ the water in successive bubbles, whilst yet no water gets into the vial to fill the place deserted by that air. And, if, when you have fill’d the neck, and part of the belly of the vial with water, you immerse the orifice into some that is stagnant, and apply your warm hands to the spherical part as before; the water in the vial will be driven out, before any bubbles pass out of the vial into the surrounding water; which shews, that the air is not so forward to dive under water, as Mr. *Hobbs* supposes.

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But



But to clear up this matter still farther, we took a glass bubble with a slender cylindrical stem, and by applying a convenient heat to the outside of the ball, we expell'd so much of the air, that, when the end of the pipe was dipt in water, and the inward air had time to recover its former coolness, the water ascended to the top of the pipe. This done, we gently, and warily rarify'd the air in the cavity of the bubble, till, by its expansion, it had driven out, almost all the water, which had got up into the stem; so that it might attain, as near as possible, to that degree of heat and measure of expansion, it had, when the water began to rise in it. And, we left two or three drops of water, unexpell'd, at the bottom of the pipe; to be sure, that none of the included air was, by this second rarification, driven out at the orifice of it: as the depression of the water so low assured us, on the other side, that the included air wanted nothing considerable of the expansion it had when the water began to ascend into the pipe. Whilst the air was in this rarify'd state, we presently removed the little instrument, out of the stagnant water, into stagnant quick-silver, which, in a short time, began to rise in the pipe. Now, if the ascent of the liquor were the effect of nature's abhorrence of a *Vacuum*, or of some internal principle of motion, or of the compression or propagated trusion of the outward air, by that which had been expell'd; why should not the mercury have ascended to the top of the pipe, as the water did before? But in fact, it did not ascend near half so far; and if the pipe had been long enough, as well as 'twas slender enough, I question, whether the mercury would have ascended, in proportion to the length of the stem, half so high as it did.

Now, of this experiment, which we try'd more than once, I see not how any good account can be given without our hypothesis, according to which 'tis clear; for the ascent of liquors, being an effect of the prevalence of the external air's pressure against the resistance it meets with in the cavity of the instrument, and the quick-silver being bulk for bulk many times heavier than water; the same surplus of pressure that was able to impel up water to the top of the pipe, ought not to be able to impel up the quick-silver to any thing near that height. And if it be here objected, as it very plausibly may, that the rais'd cylinder of mercury was much longer than it ought to have been, with regard to a cylinder of water, the proportion in gravity between those two fluids being consider'd; I answer, that when the cylinder of water reach'd to the pipe, the air possessed no more than the cavity of the spherical part of the instrument; being very little assisted to dilate itself by so light a cylinder as that of water: but when the quick-silver came to be impell'd into the instrument; by the weight of the external air; that ponderous body did not stop its ascent, as soon as it came to be equiponderant to the expell'd cylinder of water; because to attain that height it reach'd but a little way into the pipe, and left all the rest of the cavity to be fill'd with part of that air, which formerly was all shut up in the bubble; by which means, the air included in the whole instrument must needs be in a state of expansion, and, thereby

thereby have its spring weaken'd, and, consequently, disabled to resist the pressure of the external air, as much as the same included air did before, when it was less rarify'd: on which account, the undiminish'd weight or pressure of the external air was able to raise the quick-silver gradually higher, till it had obtain'd that height, at which the pressure, compounded of the weight of the mercurial cylinder, and the spring of the internal air, now less rarify'd than before, was equivalent to the pressure of the atmosphere.

And, to confirm this experiment, by a kind of inversion of it, we, by heat, drove a little air out of the bubble, and dipt the open end of the pipe into quick-silver, which, by this means, ascended, till it had fill'd about a fourth part of the pipe when held erect. Then, carefully, removing it, without letting fall any quick-silver, or letting in any air, we held the orifice of the pipe a little under the surface of a glass full of water; and applying a moderate heat to the outside of the ball, we warily expell'd the quick-silver, yet leaving a little, to be sure, that no air was driven out with it; then suffering the included air to cool, the external made the water, not only ascend to the very top of the pipe, and thence spread itself a little into the cavity of the ball, but carry'd up before it, the quick-silver that had remain'd unexpell'd at the bottom of the stem. And, if, in making the experiment, we first rais'd, as we sometimes did, a greater quantity of quick-silver, and afterwards drove it out; the quantity of water that would be impell'd into the cavity of the pipe, and ball, was accordingly increas'd.

In this experiment, 'tis manifest, that something is driven out of the cavity of the glass, before the water or quick-silver begins to ascend in it. And here, also, we see not, that the air can pass thro' the pores of quick-silver or water, but that it drives them on before it, without easily mixing with them. And there appears no circular wind, as Mr. *Hobbs* fancies in the suck'd vial, nor any tendency outwards of the included air, upon the account of such a wind: but that instead of these, the ascent of the liquors into the cavity of the pipe, depends upon the external air pressing them up, appears from hence, that the same weight of the atmosphere, impell'd into the pipe so much more of the lighter fluid, water, than of the heavier, mercury.

And that there is no need of the sallying of air out of a vial to make the atmospherical air press against a body that closes the orifice of it, when the pressure of the internal air is much weaken'd, I have shewn, by sucking out, by the help of an instrument, a considerable portion of the air contain'd in a glass: for, having then, instead of unstopping the orifice under water, suddenly apply'd a flat body to it, the external air press'd that body so forcibly against it, as to keep it fasten'd and suspended, tho' 'twere clogg'd with a weight of many ounces. Or if there be such a circular wind, as Mr. *Hobbs* pretends, produced by suction, in the cavity of the vial, it must needs be strangely lasting. For I have seen more than once, that when, by an instrument, much air has been suck'd out of a vial, which was afterwards

The Pneumatical Experiments defended.

terwards carefully closed, and thus kept for some months; yet, when 'twas open'd under water, a convenient quantity of the liquor would be briskly impel'd up into the neck and belly of the vial.

And, having with the sun-beams produced smoke in one of those well-stop'd vials; this circular wind did not at all appear to blow it about, but suffer'd it to rise, as it would have done, if the included air had been very calm.

I shall add but one experiment more, which will not be liable to some of the objections, invalid as they are, that Mr. *Hobbs* has alledg'd in his account of the suck'd vial; and which will shew, that the weight of the atmosphere is a very considerable thing; and also, that whilst Mr. *Hobbs* does not admit a more subtile matter than common air to pass thro' the pores of close and solid bodies; the air he has recourse to, will sometimes come too late to prevent a *Vacuum*. Having caus'd an æolipile very light, considering its bulk, to be made by a famous artist, I had occasion to put it so often into the fire, for several trials, that the copper scal'd off by degrees, and left the vessel much thinner than when it first came out of the artificer's hands; and now, after a long interval, I had occasion to employ it, as formerly, to weigh the air it would contain. To make this experiment the more exactly, the air was, by a strong fire, carefully driven away; when clapping a piece of sealing-wax to the pin-hole, at which it had been forc'd out, we prevented a communication betwixt the cavity of the instrument, and the external air; and supposing the æolipile to be very well exhausted, we laid it by, that when it should be grown cold, we might, by opening the orifice again, let in the outward air, and observe what increase of weight it would make. But the instrument had been so far exhausted, that what air remain'd, being unable by its spring to assist the æolipile to support the weight of the surrounding air; this external fluid did, by its weight, so strongly compress it, and thrust it so considerably inwards, and, in more than one place, so change its figure, that when I shew'd it to the Gentlemen assembled at *Gresham College*, they were pleas'd to command it of me, to be kept in their repository.



AN INQUIRY

INTO THE

Cause of Attraction

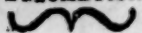
BY

SUCTION.

SUCTION being, generally, look'd upon as a kind of attraction, it The Nature of Attraction. will be requisite to premise something about the latter, in order to clear the nature of the former. The cause, nature, and notion of attraction, are, generally, either left untouch'd, or happen to be but very obscurely deliver'd.

How general and antient soever, the common opinion may be, that attraction is a kind of motion quite different from pulsion, I confess, it seems to me, a species of pulsion; at least, among inanimate bodies. I have not, yet, observ'd any thing which shews attraction cannot be reduc'd to pulsion: for they seem but extrinsical denominations of the same local motion, in which, if a moved body precede the movent, or tend to get at a greater distance from it, we call it pulsion; and if, upon account of the motion, the same body follow the movent, or approach to it, attraction. But this difference may consist only in an accidental respect; which does not physically alter the nature of the motion, but is founded upon the respect which the line, wherein the motion is made, happens to have to the situation of the movent. And, that which seems to have been the chief cause of mistaking attraction, for a motion opposite to pulsion, is, that men look upon both the moving, and moved bodies, in a popular and superficial manner; and consider in the movent, rather the situation of the conspicuous, and more bulky part of the agent, than the situation of that part of it, which immediately impresses the motion upon the mobile.

Who-



Whoever attentively regards this, may easily observe, that some part of the body, or of the instrument, which, by reason of their conjunction, in this operation, is to be look'd on as but making one with it, is really plac'd behind some part of the body to be drawn; and therefore cannot move on-wards itself, without thrusting the body forward. Thus, when a man draws a chain after him, tho' his body precede the chain, yet his finger, or some other part of the hand, wherewith he draws it, has some part, or other, which reaches behind the fore-part of the first link: and the hinder-part of this link comes behind the first part of the second link; and so each link has one of its parts placed behind some part of the next link after it, till you come to the last link of all. And so as the finger, that is in the first link, cannot move forwards, but it must thrust on that link, by this series of trusions, the whole chain is thrust forwards: and, if any other body be drawn by that chain, you may perceive some part of the last link comes behind some part of that body, or of some intervening body, which, by its cohesion with it, ought, in our present case, to be consider'd as part of it. And thus attraction seems to be but a species of pulsion; and usually belongs to that kind of it, which, for distinction sake, is called trusion; whereby we understand that kind of pulsion, wherein the movent goes along with the moved body, without quitting it, whilst the progress lasts; as happens in propelling a wheel-barrow, without letting go the hold of it.

But, it may be said, there are attractions where it cannot be pretended that any part of the attrahent comes behind the attracted body; as in magnetical, and electrical attractions; and in that which is made of water, when drawn up into springs, and pumps. Now, the *Cartesians*, and other modern philosophers, have recourse, either to screw'd particles, and other magnetical emissions, to solve phenomena of this kind; and, according to such hypotheses, we may say, that many of these magnetical, and electrical effluvia, come behind some parts of the attracted bodies; or, at least, of the little solid particles, that are, as it were, the walls of their pores; or procure some discussion of the air, that may make it thrust the moveable towards the load-stone, amber, &c. But, if there were none of these, nor any other subtile agents, to cause this motion, by a real, though unperceiv'd pulsion, I should make a distinction betwixt other attractions, and these which I should then call attractions by invisibles. However, in raising water into the barrel of a syringe, there is no true attraction made of the liquor. For, by the ascending rammer, as a part of which, I here consider the obtuse end, plug, or sucker, there is no attraction made of the contiguous and subjacent water, but only room made for it to rise into, without being expos'd to the pressure of the superior air. For, if we suppose the whole rammer to be annihilated, and, consequently, incapable of exercising any attraction; yet, provided the superior air were kept off from the water, by any other way, as well as 'twas by the rammer, the liquor would as well ascend into the cavity of the barrel; since the surface of the terra-

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terraqeous globe, being continually press'd on by the incumbent atmosphere, the water must, by that pressure, be impell'd into any cavity, here below, where there is no air to resist it; as, by supposition, there is not, in the barrel of our syringe, when the rammer was annihilated. Thus, if the external air, and consequently, its pressure, be, by the air-pump, withdrawn from about the syringe, you may raise the sucker as much as you please; but none of the sub-jacent water will follow. In short, let us suppose, that a man, standing in an inner-room, does, by his utmost resistance, keep shut a door, that is neither lock'd, nor latch'd, against another, who, with equal force, endeavours to thrust it open; in this case, if one should forcibly pull away the first man, it could not be said, that he, by his recess from the door he endeavour'd to press outwards, did truly and properly draw in his antagonist, though, upon that recess, the coming in of his antagonist, would presently ensue; so it cannot, properly, be said, that, by the ascent of the rammer, which displaces the superior air, either the rammer itself, or the expell'd air, properly attracts the sub-jacent water, tho' the ingress of that liquor, into the barrel, thereupon necessarily ensues. And that, as the comparison supposes, there is a pressure of the superior air against the upper-part of the sucker, may be easily perceiv'd, if, having well stopp'd the lower orifice of the syringe with your finger, you forcibly draw up the sucker to the top of the barrel; for, if then you let go the rammer, you will find it impell'd downwards, by the incumbent air, with a considerable force.

Having thus premis'd something, in general, about the nature of attraction, as far as 'tis necessary for my present design, we may proceed to the consideration of that kind of it, employ'd to raise liquors, which is, by a distinct name, called suction.

About the cause of this, there is great contention, between the modern philosophers, and the *Peripatetics*. The former ascribe the ascent of liquors, upon suction, to nature's abhorrence of vacuum: for, say they, when a man dips one end of a straw, or reed, into stagnant water, and sucks at the other, the air contain'd in the cavity of the reed, passes into that of his chest; and, consequently, the reed would be left empty, if no other body succeeded in the place it deserts: but there are only two bodies that can succeed, the air, and the water; now the air cannot do it, because of the interposition of the water, that denies it access to the immers'd orifice of the reed; and, therefore, it must be the water itself, which, accordingly, ascends, to prevent a vacuum, abhorr'd by nature.

The cause of suction inquired into.

But many of the modern philosophers look upon this *Fuga vacui* as an imaginary cause of suction. The atomists, who willingly admit vacuities, properly so called, both within our world, and out of it, cannot think that nature dreads a vacuum, and declines her usual course to prevent it. And the *Cartesians*, tho' they, as well as the *Peripatetics*, deny a vacuum, yet, since they affirm, not only that there is none in nature, but that there can be none, they will not grant nature to be so indiscreet, as to strain herself to prevent the making of a thing that is impossible to be made.

The Nature of Suction explained.

The *Peripatetic* opinion, about the cause of suction, though commonly defended by the schools, as well modern as antient, supposes in nature, such an abhorrence of a vacuum, as neither has been well prov'd, nor agrees with the late phenomenon of suction: since, according to their hypothesis, water, and other liquors, should ascend, upon suction, to any height, to prevent a vacuum, which yet is not agreeable to experience. For, I have carefully tried, that by working a pump, far more stanch than those that are usually made, and, indeed, as well clos'd as we could, possibly, bring it to be, we could not, by all our endeavours, raise water, by suction, to above $33\frac{1}{2}$ feet. The *Toricellian* experiment shews, that the weight of the air is able to sustain, and some of our experiments shew, 'tis able to raise a mercurial cylinder, equal, in weight, to as high a cylinder of water, as we were able to raise by pumping. For, mercury being near 14 times as heavy as water, of the same bulk, if the weight of the air be equivalent to that of a mercurial cylinder of 29, or 30 inches, it must be able to counterpoise a cylinder of water, near 14 times as long; that is, from 34, to 36 feet. And, very disagreeable to the common hypothesis, but consonant to ours, is the experiment that I have, more than once, made, by taking a glass pipe, about three feet long, dipping one end of it in water, and sucking at the other, whereby the water will be, suddenly, made to flow briskly into one's mouth; when if, instead of water, you dip the lower-end into quick-silver, though you suck as strongly as ever you can, provided, in this case, as in the former, you hold the pipe upright, you will never be able to raise the quick-silver near so high as your mouth: so that, if the water ascended, upon suction, to the top of the same pipe, because there would, otherwise, have been a vacuum left in the cavity of it; why should not we conclude, that when we have suck'd up the quick-silver, as strongly as we can, so much of the upper-part of the tube, as is deserted by the air, and yet not fill'd by the mercury, admits, in part at least, a vacuum, as to air; of which, consequently, nature cannot, reasonably, be supposed, to have so great and unlimited an abhorrence, as the *Peripatetics* presume. Yet, I will not determine whether there be any more than many little vacuities, or spaces destitute of air, in the cavity of the pipe, unfill'd by the mercury, so that the whole cavity is not one entire empty space; it being sufficient, for my purpose, that the experiment affords a good argument against the *Peripatetics*; and warrants us to seek for some other cause, than a *Fuga vacui*, why a much stronger suction, than that which made the water ascend, with ease, into the mouth, will not also raise quick-silver to near the same height.

Those modern philosophers, who admit not a *Fuga vacui* to be the cause of the ascent of liquors in suction, generally agree in referring it to the action of the thorax: for, when a man endeavours to suck up a liquor, he, by means of the muscles, enlarges the cavity of his chest; which he cannot do, but, at the same time, he must thrust away those parts of the ambient air, that were contiguous to his chest; when, the displaced air, according to some, compresses the contiguous air, and that, the next to it, and

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and so onwards, till the pressure, successively, passing from one part of the air to the other, arrive at the surface of the liquor; so that all other places being, as to sense, full, the impell'd air cannot find place, but by thrusting the water into the room made for it in the pipe, by the recess of the air, that pass'd into the lungs of the person. And, they differ not much from this explanation, who, without taking in the compression of the ambient air, made by the thorax, refer the phenomenon to the propagated motion, or impulse, impress'd on the air, displac'd by the thorax, in its dilatation, and yet unable to move in a world perfectly fill'd, as they suppose ours to be, unless the liquor be impell'd as much into the cavity of the pipe, as fast as 'tis deserted by the air, said to be suck'd up. But, tho' I readily confess this explanation to be ingenious, and such as I wonder not they should acquiesce in, who are only acquainted with the obvious phenomena of suction; and tho' I am not sure, that in the most familiar cases, the causes assign'd by them, may not contribute to the effect, yet I cannot acquiesce in this theory: for, I think the cause of suction assign'd, is, in many cases, not necessary; in others, not sufficient. And, first, as to the condensation of the air, by the dilatation of the chest; when I consider the extent of the ambient air, and how small a compression, no greater an expansion than that of the thorax is likely to make; I can scarce think so slight a condensation of the free air, should have so considerable an effect on the surface of the liquor, to be rais'd, as the hypothesis requires: and, that this impulse of the air, by the dilated thorax, though it accompanies the ascent of the water, procured by suction, is not of absolute necessity thereto, will, I presume, be easily granted: even a propagated pulsion, abstracted from any condensation of the air, is not so necessary, but that the effect may be produc'd without it. Now, suppose so much air as is displaced by the thorax, annihilated, yet the ascent of the liquor would still ensue; for, when a man begins to suck, there is an equilibrium between the pressure which the air contain'd in the pipe, has, by virtue of its spring, upon that part of the surface of the water, that is surrounded by the sides of the pipe, and the pressure which the atmosphere has, by virtue of its weight, upon all the rest of the surface of the stagnant water: so that when, by the dilatation of the thorax, the air, within the pipe, comes to be rarified, and, consequently, to lose of its spring, the weight of the external air continuing, in the mean time, the same; it must necessarily happen, that the spring of the internal air will be too weak, to balance, any longer, the gravitation of the external; and, consequently, that part of the surface of the stagnant water, which is included in the pipe, being less press'd upon than all the other parts of the same surface, must necessarily give way, and, therefore, be impell'd up into the pipe, where the air, having had its spring weaken'd, is no longer able to resist it, as before. Thus, conceiving that, within a chamber, three men thrust all together, with their utmost force, against a door, to keep it shut, at the same time that three others, of equal strength, endeavour to thrust it open; though, whilst their opposite endeavours are equal, the door will continue

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shut,

shut, yet, if one of the three men, within the room, should cease to act, there would need no new force, in the three men, to make them prevail, and thrust open the door, against the resistance of the two.

The long neck of a glass bubble, being seal'd up, and, almost, all the air, by heat, driven out of the whole cavity thereof, the glass was laid aside, for some hours; afterwards, the seal'd apex of the neck was broken off, under water: I demand, now, of a *Peripatetic*, whether the liquor ought to be suck'd, or drawn into the cavity of the glass; and why? If he says, as, questionless, he will, that the water wou'd be attracted, to hinder a vacuum, he, thereby, acknowledges, that, till the glass was unstopp'd, under water, there remain'd some empty space in it: for, 'till the seal'd end was broken off, the water cou'd not get in; and therefore, if the *Fuga vacui* had any thing to do in the ascent, the liquor must rise, not to prevent an empty space, but to fill one that was made before. Nor does our experiment much more favour the other philosophers, I dissent from: for there is, here, no dilatation made of the sides of the glass, as in ordinary suction there is made of the thorax; but only so much air driven out of the cavity of the bubble, into whose room, since neither common air, nor water, is permitted to succeed, it appears not how the propagated, and returning impulse, or the circle of motion, as to common air and water, takes place. Again, I demand, what becomes of the air, that has been, by heat, driven out, and is, by the hermetical seal, kept out of the cavity of the bubble? If it be said, that it diffuses itself into the ambient air, and mixes with it; this is to grant, what I contend for, that so little air, as is usually displac'd in suction, cannot make any considerable compression of the free ambient air: for, what can one cubic inch of air, which is, sometimes, more than one of our glasses contains, do, towards condensing a whole chamber full, when the expell'd corpuscles, are evenly distributed among those of the ambient? And how comes this inconsiderable condensation to have so great an effect in every part of the room, as to be able there to impel into the glass, as much water, in extent, as the whole air that was driven out of the cavity of it? But, if it be said, that the expell'd air only condens'd the contiguous air; 'tis no way probable that the expell'd particles of the air, should not, by the different motions of the ambient air, be quickly made to mix with it; but should rather wait till the vessels, whence 'twas driven out, were unstopp'd again. But though this could, probably, be pretended, it cannot, truly, be asserted: for, if you carry the seal'd glass quite out of the room, and unstop it at some other place, tho' two or three miles distant, the ascent of the water will, as I found by trial, nevertheless, ensue; in which case, I presume, it cannot be said, that the air expell'd out of the glass, and which condens'd the contiguous air, attended the bubble in all its motions; and was ready at hand, to impel in the water, as soon as the seal'd apex of the vial was broken off.

The Nature of Suction explained.

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In our own hypothesis of suction, we suppose, first, without disputing either the existence or the nature of elementary air, that the common air, we breathe in, and which I call atmospherical, abounds with corpuscles not destitute of weight, and endow'd with elasticity, whereby the lower parts, compress'd by the weight of the upper, incessantly endeavour to expand themselves; by which expansion, and in proportion to it, the spring of the air is weaken'd, the more they are permitted to stretch themselves.

Next, we suppose, that the terraqueous globe, being surrounded with this gravitating and springy air, has its surface, and the bodies placed on it, press'd by as much of the atmosphere, as either perpendiculary rests on them, or can otherwise come to bear upon them. This pressure is, by the *Torricellian* and other experiments, found to be equivalent to a perpendicular cylinder of about twenty-nine or thirty inches of quick-silver.

Lastly, we suppose, that, air being contain'd in a pipe or other hollow body, having but one orifice open to the free air, if this orifice be hermetically seal'd, or otherwise clos'd, the included air, whilst it continues without any farther expansion, will have an elasticity equivalent to the weight of as much of the outward air as did before press against it. For, if the weight of the atmosphere had been able to compress it further, it would have done it; and then the closing of the orifice, at which the internal and external air communicated, as it fence'd the included air from the pressure of the incumbent, so it hinder'd the same included air from expanding itself; whence, as it was shut up with the pressure of the atmosphere upon it, that is in a state of as great compression as the weight of the atmosphere could bring it to, so being shut up, and thereby kept from weakening that pressure by expansion, it must retain a springiness proportionable to the pressure 'twas before expos'd to. But if, as was said in the first supposition, the included air should come to be dilated or expanded, its spring, like that of other elastic bodies, would be weaken'd, answerably to that expansion.

To me then it seems, in general, that liquors are, upon suction, rais'd into the cavities of pipes, and other hollow bodies, when there is a less pressure on the surface of the liquor in the cavity, than on the surface of the external liquor, that surrounds it; whether that pressure of those parts of the external liquor, which are from time to time impell'd up into the orifice of the pipe, proceed from the weight of the atmosphere, the propagated compressure, or impulse, of some parts of the air, the spring of the air, or some other cause; as the pressure of bodies quite distinct from air.

Upon a general view of this hypothesis, it seems very agreeable to mechanical principles. For, if there be, on the different parts of the surface of a fluid body unequal pressures, 'tis plain, as well by the nature of the thing, as by what has been demonstrated by *Archimedes*, that the greater force will prevail against the less; and that such a part of the water's surface must give way, as is the least press'd.

To proceed to some experiments made in favour of this hypothesis. We took a glass pipe bended like a siphon; but so, that the shorter leg was parallel

parallel to the longer, and hermetically seal'd at the end: into this siphon we convey'd water, so that the crooked part being held downwards, the liquor reach'd to the same height in both legs; about an inch and an half of uncompress'd air being shut up in the shorter. This little instrument, about fifteen inches long, being thus prepared, 'tis plain, that, according to the hypothesis I dissent from, there is no reason why the water should ascend upon suction. For, tho' we should admit, that the external air were considerably compress'd, or receiv'd a notable impulse when the chest is enlarg'd, yet, in our case, that compression or protrusion will not reach the surface of the water in the shorter leg; because it is there fenced from the action of the external air, by the sides of the glass, and the hermetical seal at the top: but if a person suck'd strongly at the open orifice in the longer leg, the water in the shorter would be depress'd, and that in the longer ascend, at one suck, about an inch and a half; of which the reason is clear on our hypothesis. For the spring of the included air, together with the weight of the water in the shorter leg, and the pressure of the atmospheric air, assisted by the weight of the liquor in the longer leg, balanced one another before the suction began; but when, afterwards, upon suction, the air in the longer leg came to be dilated, and thereby weaken'd, 'twas render'd unable to resist the undiminish'd pressure of the air included in the shorter leg; which, consequently, expanding itself, by virtue of its elasticity, depress'd the contiguous water, and made it proportionably rise in the opposite leg, till its spring being by the expansion gradually more weaken'd, it balanced the gravitation or pressure of the atmosphere. And this is the reason why, when the person who suck'd had rais'd the water in the longer leg less than three inches higher, by repeated endeavours, and that, without once suffering the water to fall back again, he was not able to elevate the water in the longer so much as three inches above its first station. And if in the shorter leg, there was only an inch and a quarter of space left for the air; by several acts of suction, skillfully repeated, he could not raise the liquor in the longer leg above two inches; because, by that time, the air included in the shorter leg had, by expanding itself further, proportionably weaken'd its spring, till, at length, it became as much rarify'd as the air in the cavity of the longer leg; and, consequently, was able to thrust away the water with no more force than the air in the longer leg was able to resist. And hence it appear'd, that the rarification usually made of air by suction, is not near so great as one would expect; probably, because, by the dilatation of the lungs, the air, being still shut up, is but moderately rarify'd; and that in the longer leg can by them be brought to no greater degree of rarity than the air within the chest. For whereas the included air in our instrument was not expanded, at one suck, to above double its former dimensions, and by several successive sucks was expanded, but from one inch and a half to less than four inches and a half, if the suction could have been conveniently made with a great stanch syringe, the rarification of the air would, probably, have been far greater; since, in our pneumatic engine, air may without heat,

heat, and by a kind of suction, be brought to possess many hundreds of times the space it took up before. From this rarification of the air in both the legs of our instrument, proceeds another phenomenon readily explicable by our hypothesis. For if, when the water was impell'd up as high as the suction could raise it, the instrument were taken from the person's mouth, the elevated water would, with violence, return to its wonted station. For the air, in both legs of the instrument, having by suction lost much of its spring, and, consequently of its power of pressing; when once the orifice of the longer leg was left open, the atmospherical air came again to gravitate upon the water in that leg; and the air included in the other, having its spring weaken'd by the preceding expansion, was not able to hinder the external air from violently repelling the elevated water, till the included air was thrust into the space it possess'd before the suction; in which space it had density and elasticity enough to resist the pressure that the external air exercis'd against it, thro' the interpos'd water.

But our hypothesis about the cause of suction, needs only the assistance of our pneumatic engine. For by trials, purposely devis'd and carefully made therewith, we found, that a good syringe being so convey'd into the receiver, that the open orifice of the pipe, or lower part, remain'd under water; if the engine were exhausted, and the handle of the syringe drawn up, the water did not follow it, which yet it would do if the external air were let in again. The reason of which is plain; for the air that should have press'd upon the surface of the stagnant water having been pump'd out, there was nothing to impel up the water into the deserted cavity of the syringe, till the receiver was fill'd with air.

I shall next offer some easy experiments to make out these three propositions.

1. That a liquor may be rais'd by suction, when the pressure of the air, neither as it has weight nor elasticity, is the cause of it.

2. That the weight of the atmosphere is sufficient to raise up liquors in suction.

3. That, in some cases, there will be no suction, tho' there is a dilatation of the thorax; and no danger of a Vacuum, if the liquor should ascend.

1. And first to shew how much the rising of liquors, in suction, depends upon the weight or pressure of the impelling body; and how little necessity there is, where such a pressure happens, that in the place deserted by the liquor suck'd, there should succeed air, or some other visible body; I devis'd the following experiments. We took a glass pipe, fit for the *Torricellian* experiment, but much longer than was necessary for that use; this being hermetically seal'd at one end, the other was so bent as to be reflected upwards, and make, as it were, the shorter leg of a siphon parallel to the longer; so that the tube was now shaped like an inverted siphon, with legs of a very unequal length. This we fill'd with mercury in an inclining posture, and then erecting it, the mercury subsided in the longer leg, as in the *Torricellian* experiment, and attain'd to between two feet and a quarter, and two feet and a half, above the surface of the mercury in the shorter leg, which

Suction may raise a fluid without the pressure or elasticity of the air.

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in this instrument answers to the stagnant mercury in an ordinary barometer. Out of the shorter leg of this tube, we warily took as much mercury as was thought convenient; and this we did so as to hinder any air from getting into the deserted cavity of the longer leg; by which means, the mercurial cylinder retain'd the same height above the stagnant mercury in the shorter. The upper and closed part of this portable barometer must have been free from common air, because, if gently inclin'd, the quick-silver would ascend to the top of the tube; which it could not do, if the place formerly deserted by it were possess'd by the air. The instrument being thus fitted, I caus'd a person to suck at the shorter leg of it; whereupon there presently ensued an ascent of four or five inches of mercury in that leg, and a proportionable subsidence in the longer; yet in this case the rise of the mercury cannot proceed from the pressure of the air. For the weight of the atmosphere is fenc'd off, by that which closes the upper end of the longer tube: and the spring of the air has here nothing to do; since the space deserted by the mercury, is not possess'd by the included air: and the pulsion or condensation of the air, suppos'd by several modern philosophers to be made by the dilatation of the chest, and to press upon the surface of the liquors that are to be suck'd up, cannot here be pretended; because the surface of the liquor in the longer leg is every way fenc'd from the pressure of the ambient air. It remains, therefore, that the cause which rais'd the quick-silver in the shorter leg, upon the suction, was the weight of the collateral quick-silver, superior in the longer leg; which being equivalent to the weight of the atmosphere, there is a plain reason why the stagnant mercury in the shorter leg should be rais'd some inches by suction, as mercury stagnant in an open vessel will be rais'd by the weight of the atmosphere, when the suction is made in the open air: for, in both cases, there is a pipe that reaches to the stagnant mercury, and a competent weight to impel it into that pipe, when the air in the cavity of it has its spring weaken'd by the dilatation that accompanies suction.

The weight of
the atmosphere
may, alone,
raise liquors: in
suction, another
force is employ'd
to raise them.

2. That the weight of the air is sufficient to raise liquors in suction, may appear by arguments drawn from the *Torricellian* experiment; and much more clearly from some we have made with our air-pump. And with the like view, having provided an instrument in imitation of the portable barometer, lately mention'd, but whose legs were not so unequally long; and having in it made the *Torricellian* experiment, after the manner describ'd, we order'd the matter so, that there remain'd in the shorter leg the length of several inches unfill'd with stagnant mercury. Then I caus'd a person to raise the quick-silver, by suction, so to the orifice of the shorter leg, that the orifice being seasonably and dexterously closed, the mercury continu'd to fill that leg as long as we thought fit; and then having put a mark at the surface of the mercury in the longer leg, we unstopp'd the orifice of the shorter; whereupon the mercury that before fill'd it, was depress'd, till in the longer leg it was rais'd five inches, or more, above the mark, and continu'd at that height. This mercury, rais'd by suction, was depress'd; because its own weight

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weight could not here make it fall; since a mercurial cylinder of five inches was far from being able to raise so tall a cylinder of mercury, as made a counterpoize in the longer leg; and therefore, the depression we speak of, is to be referr'd to the gravitation of the atmospherical air upon the surface of the mercury in the shorter leg. And I see no cause to doubt, that if we could have procur'd an instrument, into whose shorter leg, a mercurial cylinder many inches higher might have been suck'd up, it would, by this contrivance, have appear'd, that the pressure of the atmosphere could easily impel up a far taller cylinder of mercury than it did in our experiment. For, if the gravity of an incumbent pillar of the atmosphere be able to compress a parcel of included air as much as a mercurial cylinder equivalent, in weight, to between thirty and thirty-five feet of water is able to condense it, the same atmospherical cylinder may well be able, by its weight, to raise and counterbalance twenty-eight or twenty-nine inches of quick-silver, or an equivalent pillar of water in tubes, where the resistance of these two fluids, to be rais'd and sustain'd by the air, depends, only, upon their own unassisted gravity.

To confirm our doctrine of the gravitation of the atmosphere, upon the surface of the liquors expos'd to it, I shall subjoin an experiment, devis'd to shew, that the incumbent air, in its usual state, would compress other air in the like natural state, as much as a cylinder of twenty-eight or twenty-nine inches of mercury could condense it. But in order hereto, I suppose it known, that about twenty-nine or thirty inches of quick-silver will compress air shut up, in its usual state, in the shorter leg of our portable barometer into half the space it possess'd before.

We provided a portable barometer, wherein the mercury in the longer leg was kept suspended by the counterpoize of the air that gravitated on the surface of the mercury in the shorter, which, we had so order'd, that it reach'd not, by about two inches, to the top of the shorter leg. Then making a mark at the place where the stagnant mercury rested, 'twas manifest, according to our hypothesis, that the air in the upper part of the shorter leg was in its natural state, or of the same degree of density with the outward air, with which it freely communicated at the open orifice of the shorter leg; so that this stagnant air was equally press'd upon by the weight of the collateral superior cylinder of mercury in the longer leg, and the equivalent weight of a directly incumbent pillar of the atmosphere. Then the upper part of the shorter leg that had been before, purposely, drawn out to, almost, a capillary smallness, was hermetically seal'd; which, tho' the instrument remain'd erect, was so suddenly done, by reason of the slenderness of the pipe, that the included air did not appear to be sensibly heated: after this, we open'd the lower end of the longer leg without shaking the vessel; by which means, the atmospherical air, gaining access to the mercury included in the longer leg, did, by its gravitation upon it, so compress the air included in the shorter leg, that, according to the estimate we made, with the help of a ruler, it was crowded into near half the space it took up before; and consequently suffer'd a compressure like that,

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That the ascent
of liquors, by
suction, depends
upon pressure.

This experiment was made a second time, with like success.

3. And to make it yet further appear, how much the ascent of liquors, by suction, depends upon pressure, rather than upon nature's imaginary abhorrence of a *Vacuum*, or the propagated pulsion of the air; I will subjoin an instance, wherein that presum'd abhorrence cannot be pretended. The experiment was thus made.

A glass siphon, like those lately describ'd, with one leg far longer than the other, was hermetically seal'd at the shorter leg, and then, by degrees, we put in, at the orifice of the longer leg, as much quick-silver, as by its weight sufficed to compress the air in the shorter leg, into about half the space it possess'd before; so that, according to the *Peripatetic* doctrine, the air must be in a state of preternatural condensation, and that to a far greater degree, than 'tis usually brought by cold intense enough to freeze water. Then, measuring the height of the quick-silver in the longer tube above the superficies of that in the shorter, we found it not to exceed thirty inches. Now if liquors rose, in suction, for fear of a *Vacuum*, there is no reason why this quick-silver in the longer part of the siphon should not easily ascend upon suction, at least till the air in the shorter leg had regain'd its former dimensions; since it cannot, in this case, be pretended, that if the mercury should ascend, there would be any danger of a *Vacuum* in the shorter leg of the tube; because the contiguous included air is ready at hand to succeed, as fast as the mercury subsides in the shorter leg of the siphon. Nor can it be alledg'd, that, to fill the place deserted by the quick-silver, the included air must suffer a preternatural rarification; since 'tis plain, that, on the contrary, as long as the air continues in the state whereto 'tis reduced by the weight of the quick-silver, it is kept in a violent state of compression, because in the shorter leg it was in its natural state, when the mercury pour'd into the longer leg did, by its weight, thrust it into about half the space it took up before. Yet, having caus'd several persons to suck, several times, as strongly as they could, they were not able, so much as for a minute, to raise the mercury in the longer, and make it subside in the shorter far more than an inch. And to shew, that the experiment was not favourably made for me; the height of the mercurial cylinder in the longer leg, above the surface of that in the shorter, was, at the time of suction, an inch or two short of thirty; and the compress'd air in the shorter leg, was so far from having been expanded, by the exsuction, beyond its natural and first dimensions, that it did not, when the contiguous mercury stood as low as we could make it subside, regain so much as one half of the space it had lost by the precedent compressure; and, consequently, was in a preternatural state of condensation, when it had been freed from that state as far as suction could do it. Whence it seems evident, that it was not *obfusgam vacui*, that the quick-silver did, upon suction, ascend one inch; for

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upon the same score it ought to have ascended two, or perhaps more inches; since there was no danger, that by such an ascent, any *Vacuum* should be produc'd, or left in the shorter leg of the siphon. But, according to our hypothesis, a clear cause of the phenomenon is assignable: for, before the suction began, there was an equilibrium between the weight of the superior quick-silver, in the longer leg, and the spring of the compress'd air, included in the shorter; but, when the person began to suck, his chest being widen'd, some part of the air, included in the upper-part of the longer leg, pass'd into it, and what remain'd, had, by that expansion, its pressure so weaken'd, that the air, in the shorter leg, finding no longer the former resistance, was able, by its own spring, to expand itself, and, consequently, to depress the contiguous mercury, in the same shorter leg, and raise it as much in the longer.

But here it may be objected, that if the compress'd air, in the shorter leg, had a spring equivalent to the weight of the mercury in the longer leg, why is not the mercury suck'd up in this instrument, as well as in the free air; since, according to me, the pressure of the included air, upon the subjacent mercury, must be equivalent to the weight of the atmosphere: yet experience shews, that the weight of the atmosphere will, upon suction, raise quick-silver to the height of several inches?

To clear this difficulty, and shew, that 'tis not insuperable, let us consider, that I make, indeed, the spring of the compress'd air, to be equivalent to the weight of the compressing mercury; and, I have a manifest reason to do it; because, if the spring of the air were not equivalent to that weight, the mercury must necessarily compress the air further; which 'tis granted, in fact, not to do. But then, in our case, there ought to be a great difference between the operation of the spring of the included air, and the weight of the atmosphere, after suction has been once begun. For, the weight of the atmosphere, that impels up mercury, and other fluids, when suction is made in the open air, continues still the same; but the force, or pressure of the included air, is equal to the counter-pressure of the mercury, no longer than the first moment of suction; after which, the force of the imprison'd air, still, gradually, decreases; since this compressed air, being, more and more, expanded, must needs have its spring proportionably weaken'd: so that 'tis no wonder that the mercury was not suck'd up any more than we have said; for there was nothing to make it ascend to a greater height than that at which the weaken'd spring of the expanded air was brought to balance the undiminish'd, and, indeed, somewhat increas'd weight of the mercurial cylinder, in the longer leg; and the pressure of the cylinder of air, in the same leg, lessen'd by the action of him who suck'd. For; when the orifice of this leg stood open, the mercury was press'd upon by a cylinder of the atmospherical air, equivalent to about thirty inches of quick-silver; but, by the mouth and action of him who suck'd, the tube was freed from the external air; and, by the dilatation of his thorax, the

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neighbouring air, that had a free passage through his wind-pipe to it; was proportionably expanded, and had its spring and pressure weaken'd; by which means, the compress'd air in the shorter leg of the siphon, was enabled to impel up the mercury, till the equilibrium was attain'd. And, I must here take notice, that, as the quick-silver was rais'd by suction but a little way, so the cylinder rais'd, was a very long one; whereas, when the mercury is suck'd up in the free air, it is seldom rais'd to half that length; tho', as I noted before, the impellent cause, which is the weight of the atmosphere, continued still the same: but, in our siphon, when the mercury was suck'd up only an inch, the compress'd air, possessing double the space it did before, had, by this expansion, already lost a very considerable part of its former spring and pressure.

Among the more familiar phenomena of the air-pump, none leaves so great a scruple, in the minds of some sort of men, as that, when one's finger is laid close upon the orifice of the little pipe, by which the air passes from the receiver into the exhausted cylinder, the pulp of the finger is made to enter, considerably, into the cavity of the pipe; which doth not happen without a moderate sense of pain in the lower part of the finger: for most of those who are strangers to hydrostatics, persuade themselves, that they feel this painful protuberance of the pulp of the finger, to be effected not by pressure, but distinctly by attraction.

To this, we answer, that common air being a body not destitute of weight, the phenomenon is clearly explicable by the pressure of it; for, when the finger is first laid upon the orifice of the pipe, no pain, nor swelling is produced; because the air, which is in the pipe, presses as well against that part of the finger which covers the orifice, as the ambient air doth against the other parts of the same finger. But when, by pumping, the air in the pipe is made to pass out of that, into the exhausted cylinder, there is nothing left in the pipe, whose pressure can any thing near balance the undiminish'd pressure of the external air, on the other parts of the finger; and, consequently, that air thrusts the most yielding and fleshy part of the finger into the place where its pressure is unresisted; that is, into the cavity of the pipe, where this forcible intrusion causeth pain.

To illustrate this, we took a glass pipe, of a convenient length, open at both ends, whose cavity was near an inch in diameter. To one end of this pipe, we caus'd to be firmly tied, a piece of very fine bladder, that had been oil'd, to make it both very limber, and unapt to admit water; and care was taken, that the piece of bladder, tied on, should be large enough, not only to cover the orifice, but to hang loose, somewhat beneath it. This done, we put the cover'd end of the pipe into a tall glass-body; and the pipe being held so, that the end of it reach'd, almost, to the bottom of the glass, we caus'd water to be pour'd, both into this vessel, and into the pipe, at its upper orifice, which was left open, that the water might ascend, equally, both without and within side of it. And when the glass-body was full of water, and the same liquor level to it,

or

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or a little higher within the pipe, the bladder at the lower orifice was kept plump; because the water, within the pipe, did, by its weight, press as forcibly downwards, as the external water, in the large glass, endeavour'd to press it inwards and upwards. Then we caus'd part of the water in the pipe, to be taken out of it, by a piece of sponge, or by suction with a smaller pipe; upon which, the water remaining in the pipe, being no longer able, thro' want of weight, to press against the inside of the bladder, near so forcibly as it did before; the external water, whose weight was not lessen'd, press'd the sides and bottom of the bladder, whereto it was contiguous, into the cavity of the pipe, and thrust it up therein so strongly, that the distended bladder made a kind of hemisphere within the pipe. Here, then, we have a protuberance, like that above-mention'd of the finger, effected by pulsion, not attraction; and, in a case where there can be no just pretence for having recourse to nature's abhorrence of a vacuum; since the upper orifice of the pipe being left wide open, the air might, freely, pass in and out.

The like swelling of the bladder, we could procure, without taking out any of the internal liquor, by plunging the pipe deeper into the water; for then the external liquor having, by its increase of depth, a greater pressure on the outside of the bladder, than the internal liquor had on the inside of it, the bladder must yield to the stronger pressure, and consequently be impell'd up.

If the bladder, lying loose at the lower-end of the pipe, the upper-end were carefully closed, that the air might not get out; and if the pipe, thus closed, were thrust, almost, to the bottom of the water, the bladder would not be protuberant inwards, as formerly; because the included air, by virtue of its spring, resisted, from within, the pressure of the external water against the outside of the bladder. But the upper orifice of the pipe being unstopp'd, the air, before compress'd, having liberty to expand itself, and its elasticity being weaken'd thereby; the external water would, suddenly, with noise, drive up the bladder into the cavity of the pipe, and there keep it very protuberant.

To obviate an objection, that might be brought, thro' want of skill in hydrostatics, I caus'd such a pipe, as the former, to be so bent, near the lower-end, that the orifice of it stood quite on one side, in a right angle. This lower orifice being fitted with a bladder, and the pipe, with its contain'd liquor, being thrust under water, after the former manner; the lateral pressure of the water forc'd the bladder into the short horizontal leg, and made it protuberant there, as it had done when the pipe was straight.

Lastly, that the experiment might not appear confin'd to one liquor; instead of water, we put into the straight pipe, as much red-wine as was requisite to keep the bladder bulging, when near the bottom of the water; and then saw the superficies of the red liquor, in the pipe, was much higher than that of the external water: and, if the depth of both

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both liquors, were proportionably lessen'd, the difference of height betwixt the two surfaces, would, indeed, as it ought, decrease; but still the surface of the wine would be the higher of the two; because, being lighter in specie, than common water, the equilibrium between the pressures of the two liquors, upon the bladder, would not be maintain'd, unless a greater height of wine supply'd its want of specific gravity. And, if the pipe were thrust deeper into the water, the bladder would be made protuberant inwards, as when it contain'd water. 'Tis, therefore, evident, that these phenomena, without recourse to attraction, may be explain'd, barely from the equilibrium of liquors.

The End of the Second Volume.

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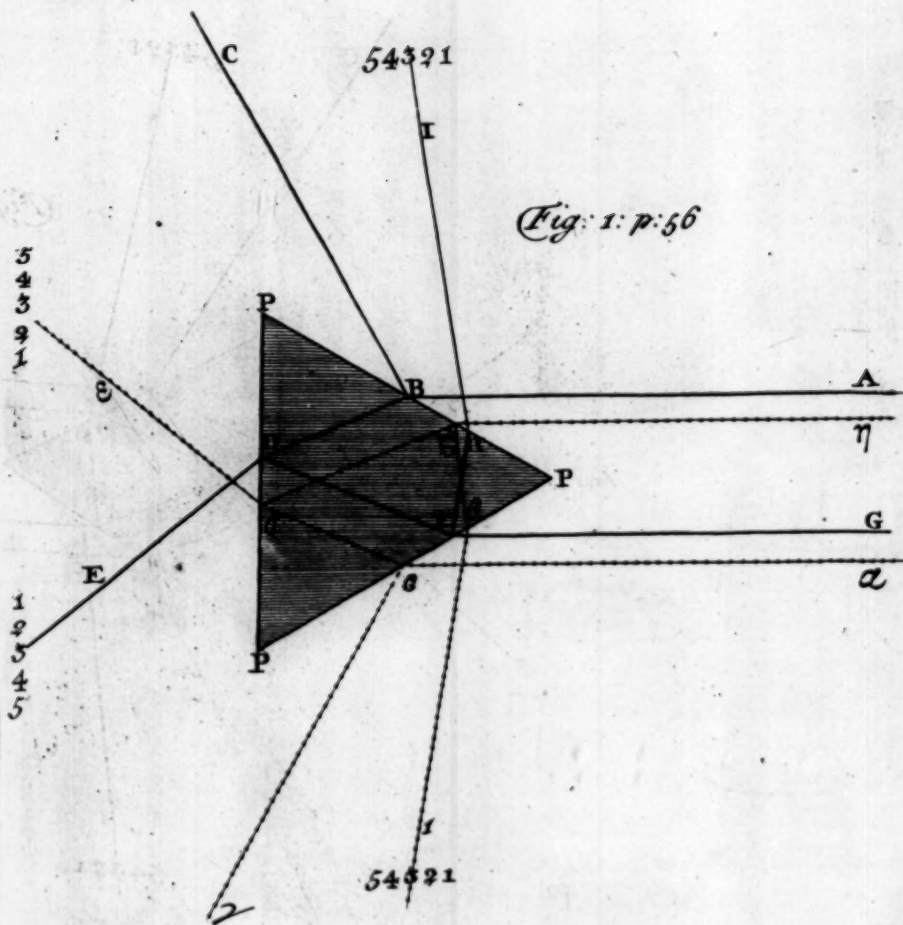
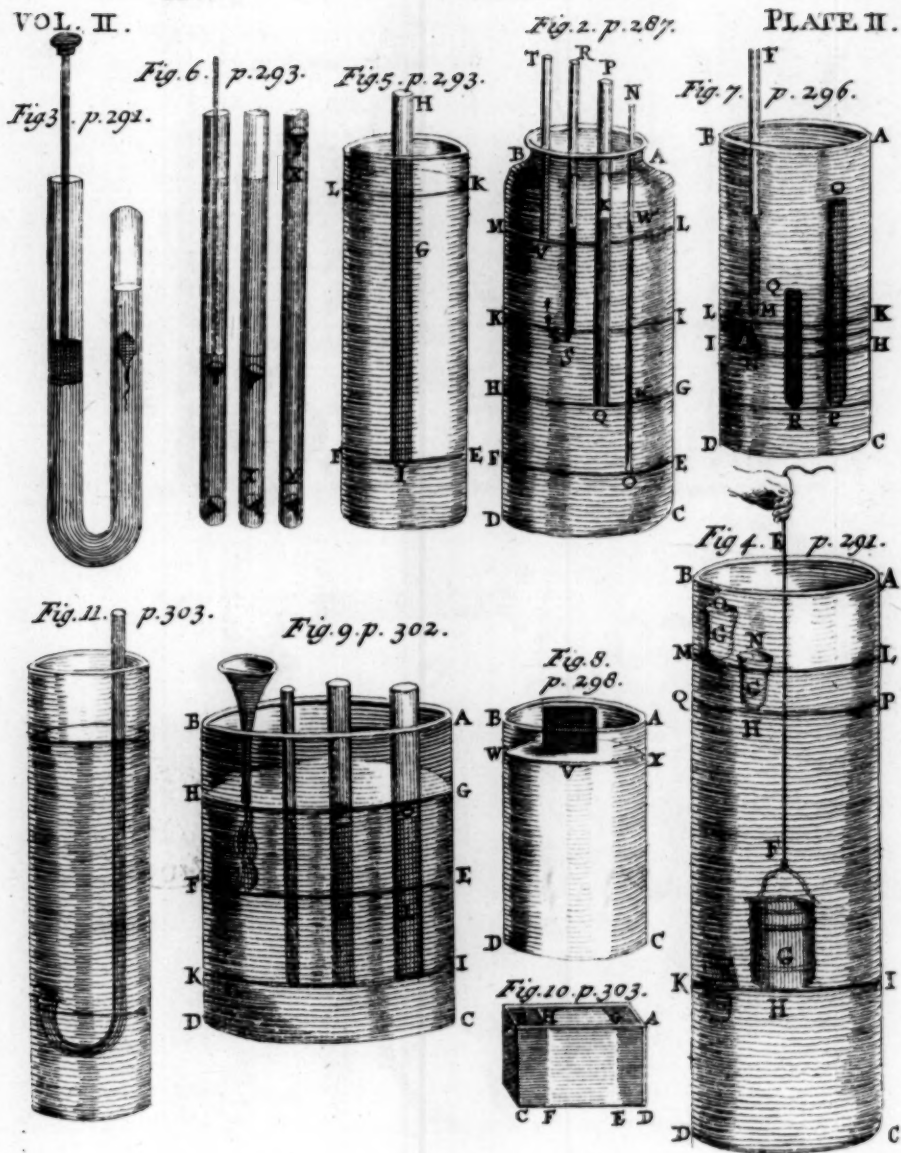


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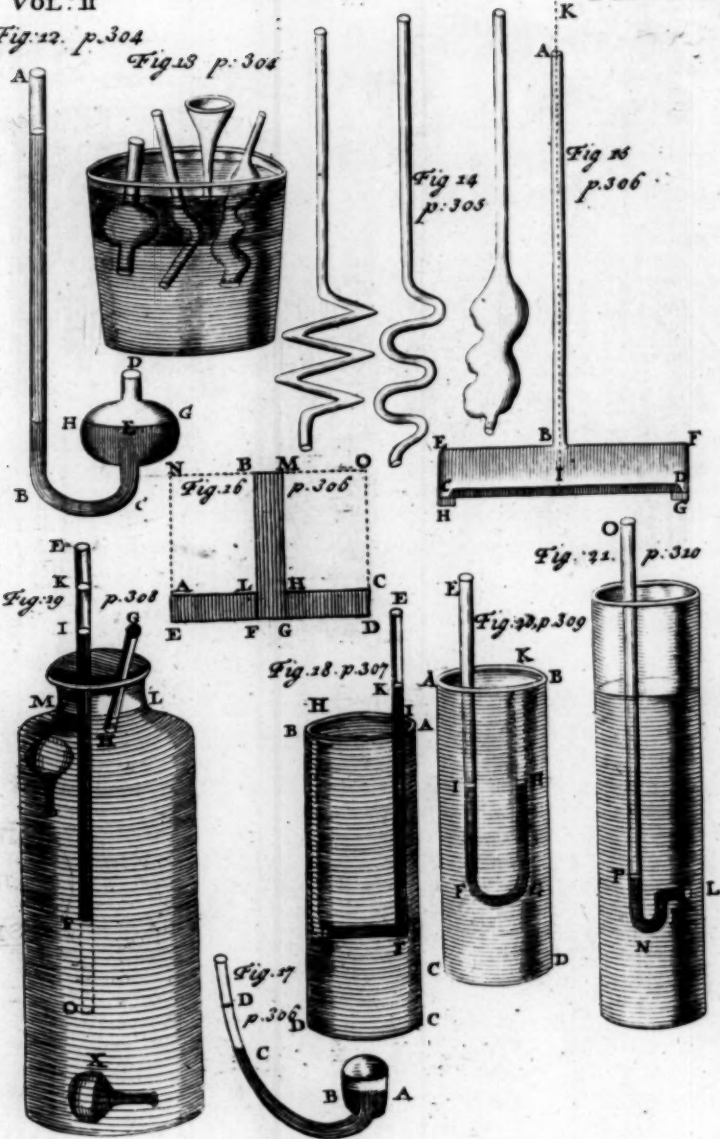


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PLATE III



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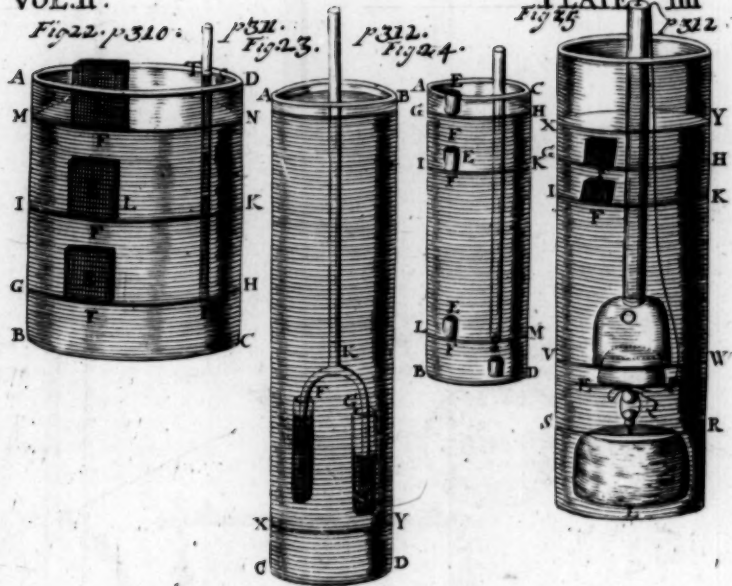
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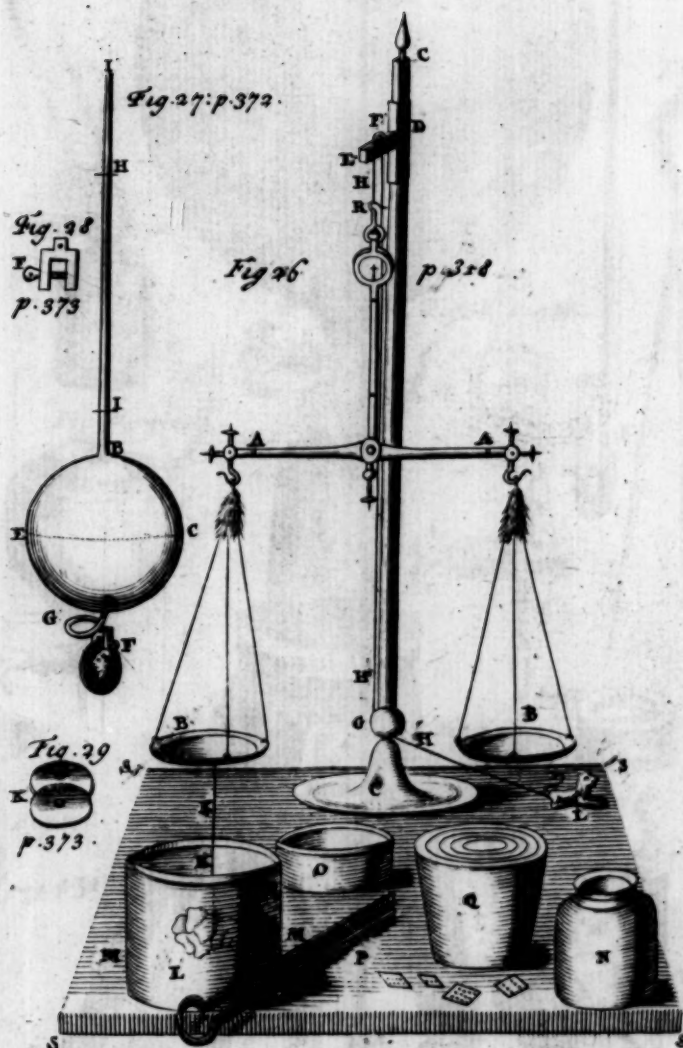
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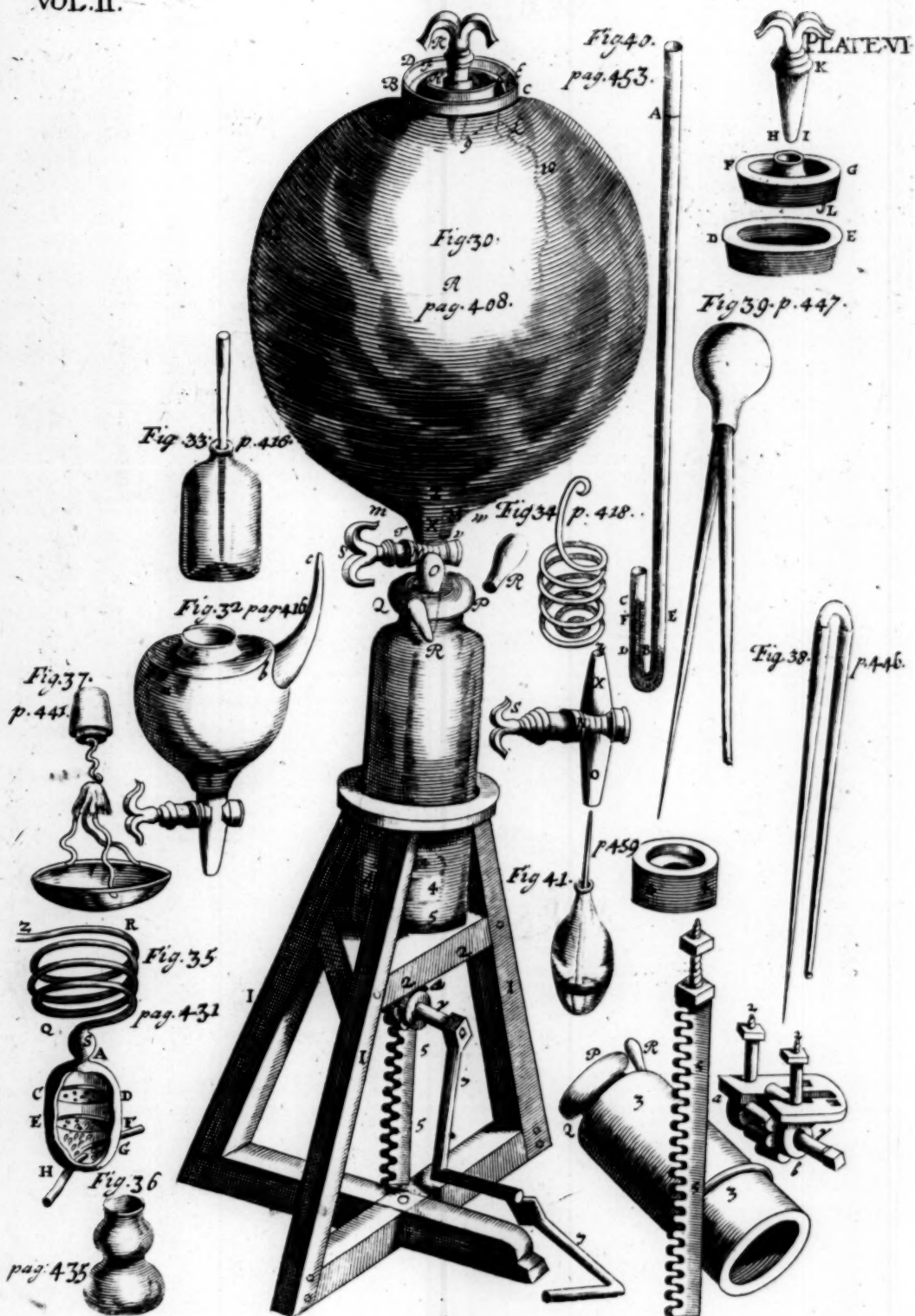
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Fig 24.

PLATE III
Fig 25

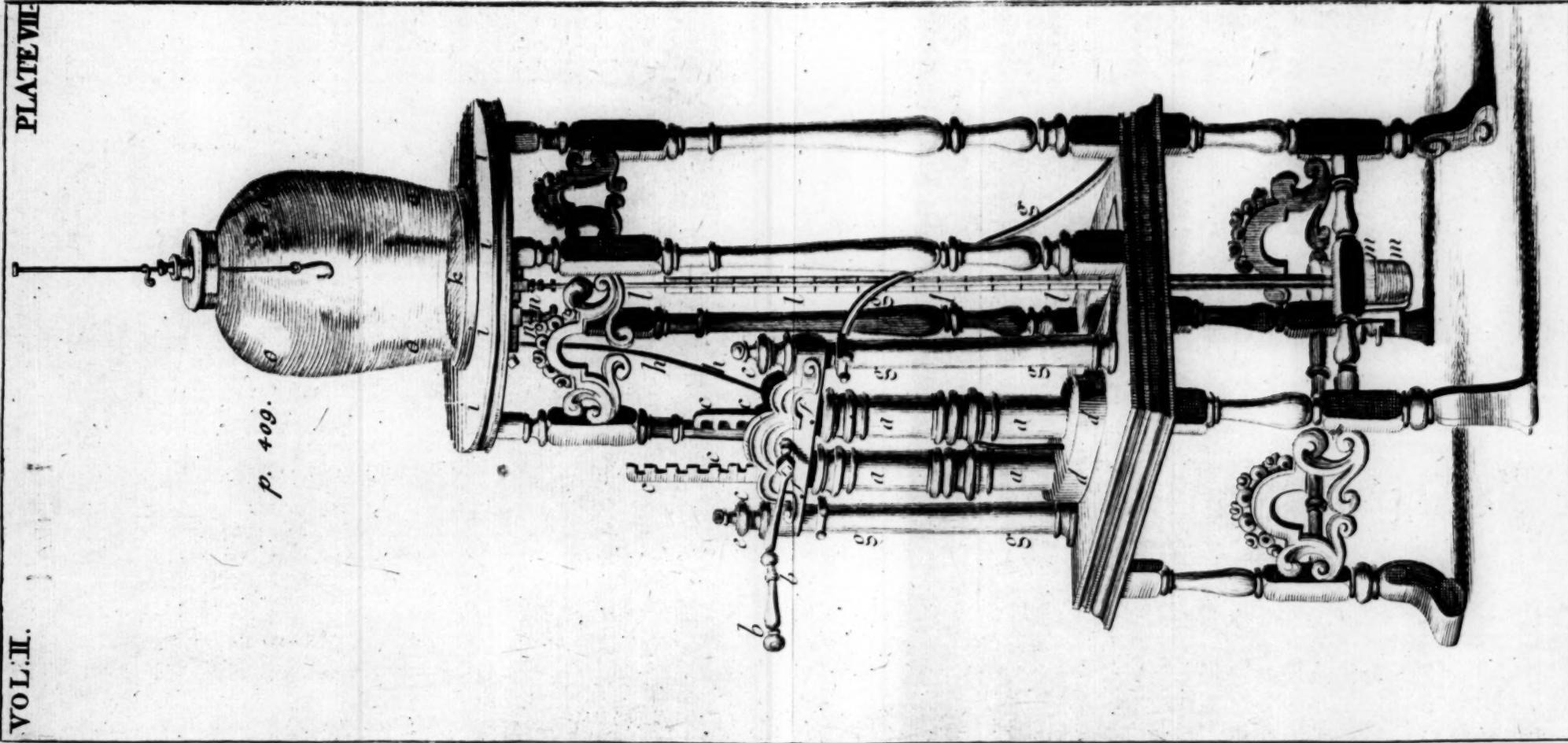
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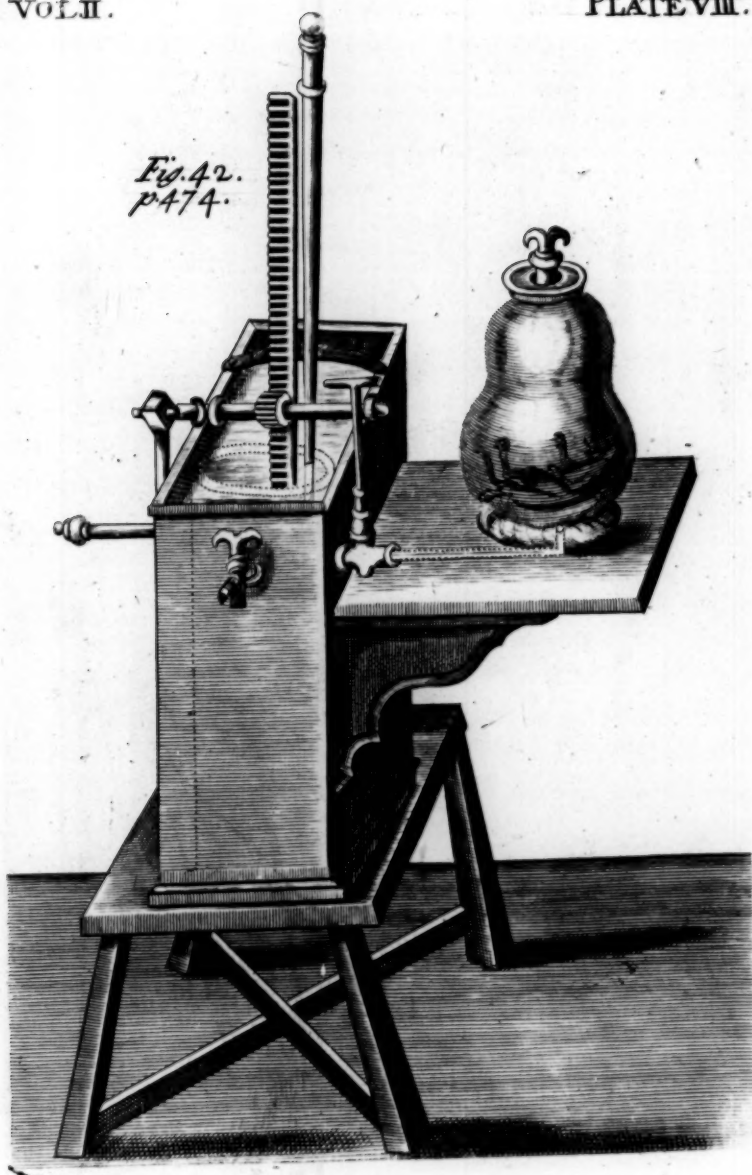


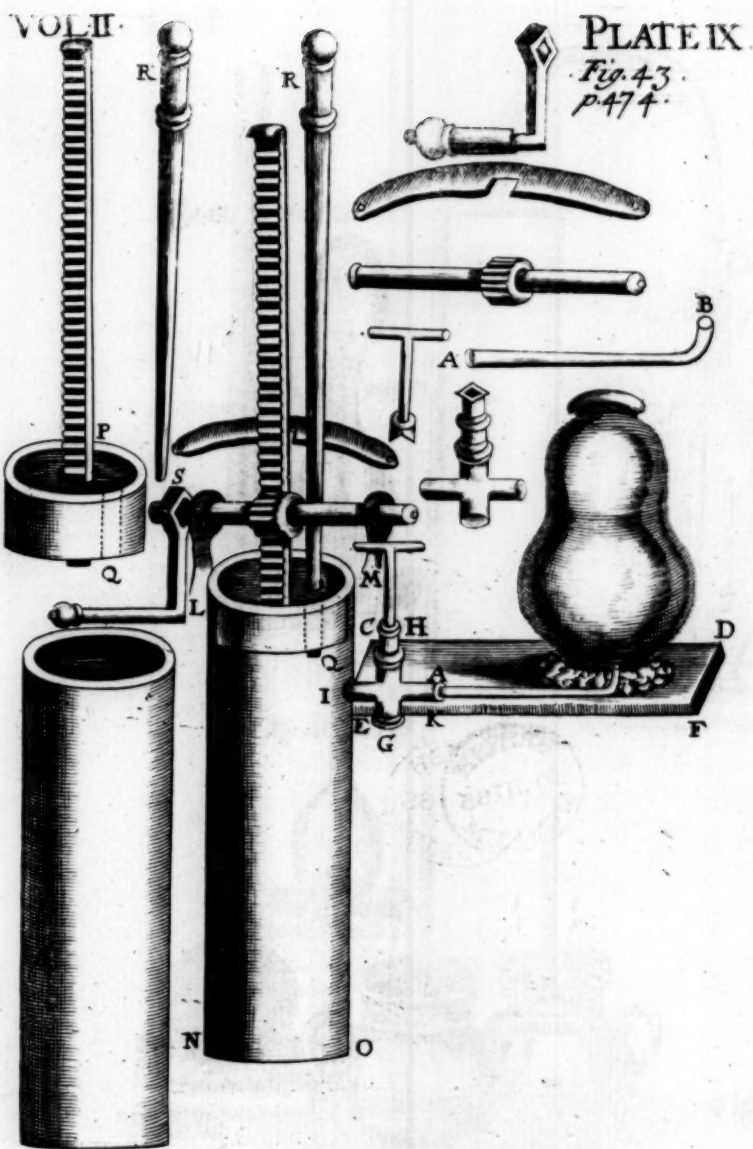


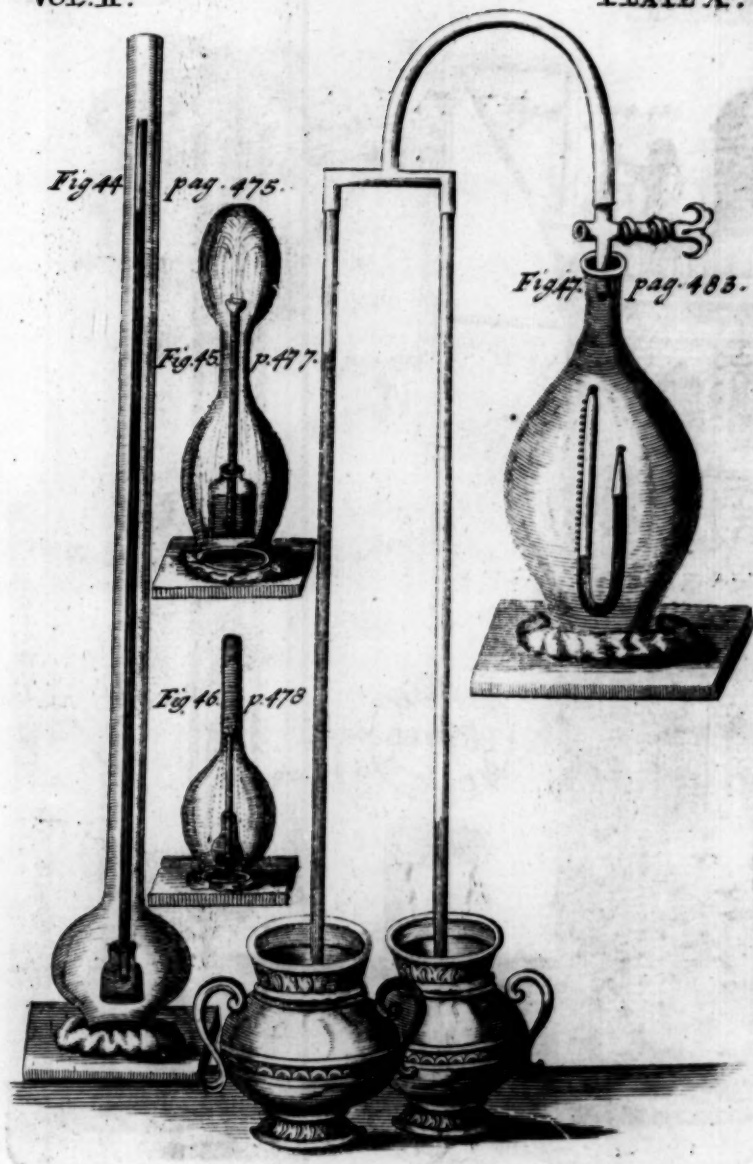
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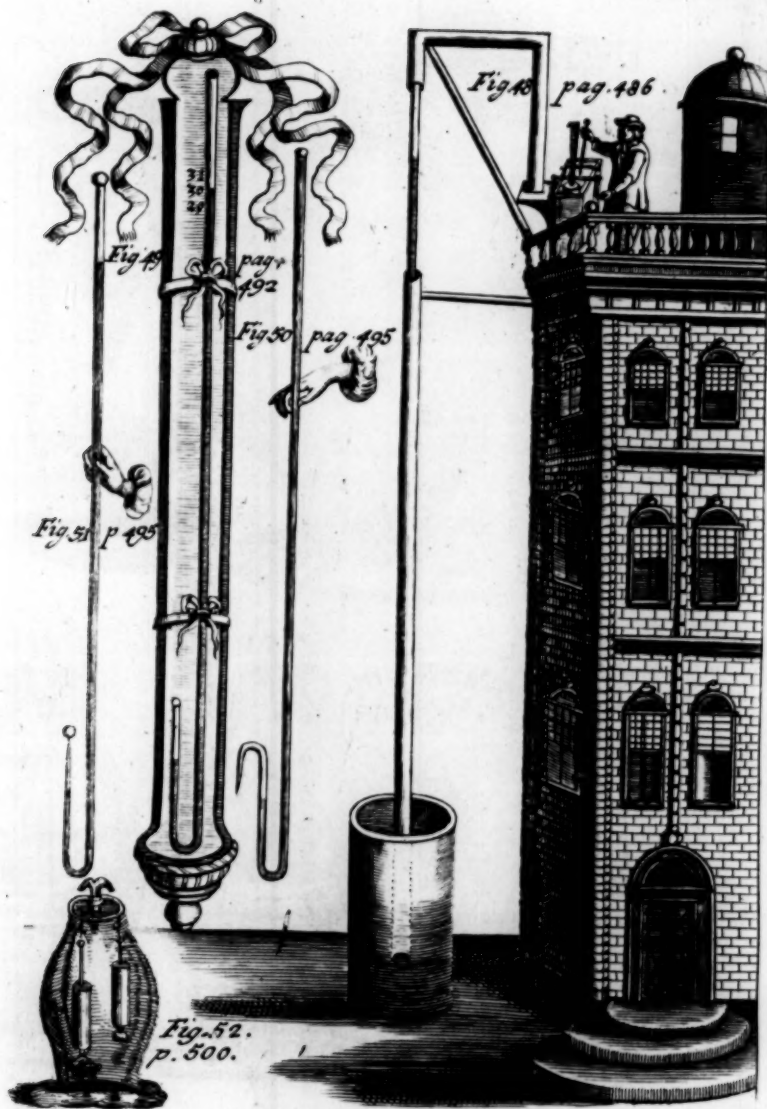


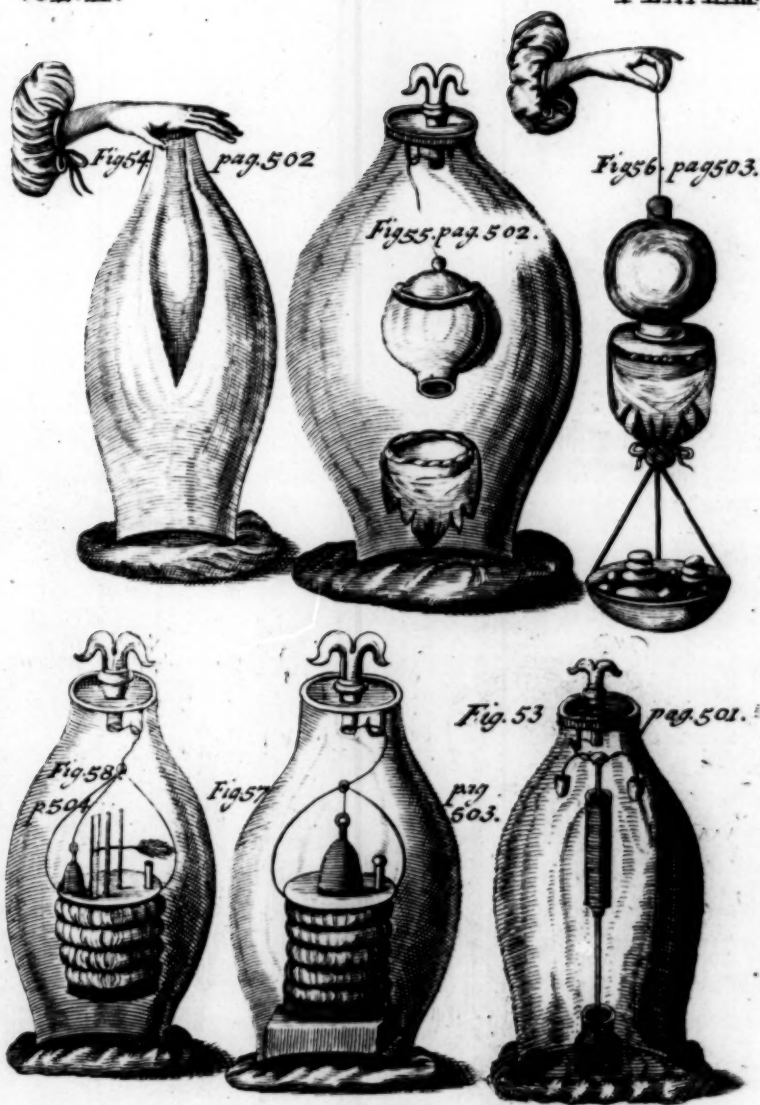
*Fig. 42.
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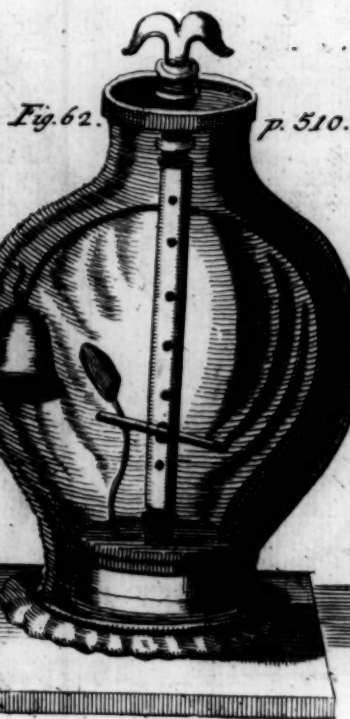












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PLATE XIV.

Fig. 65.



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Fig. 64.



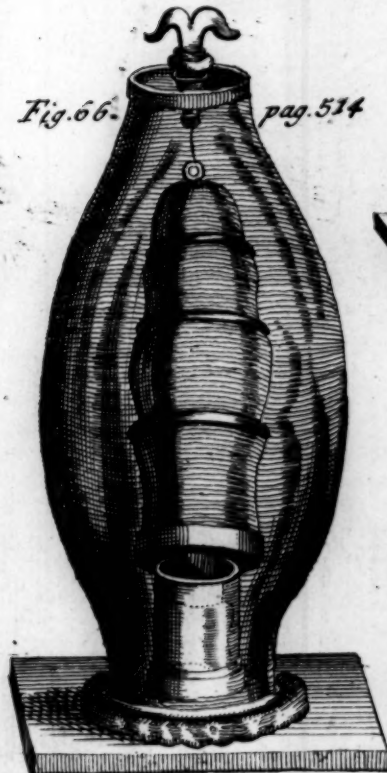
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Fig. 63.



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Fig. 66.

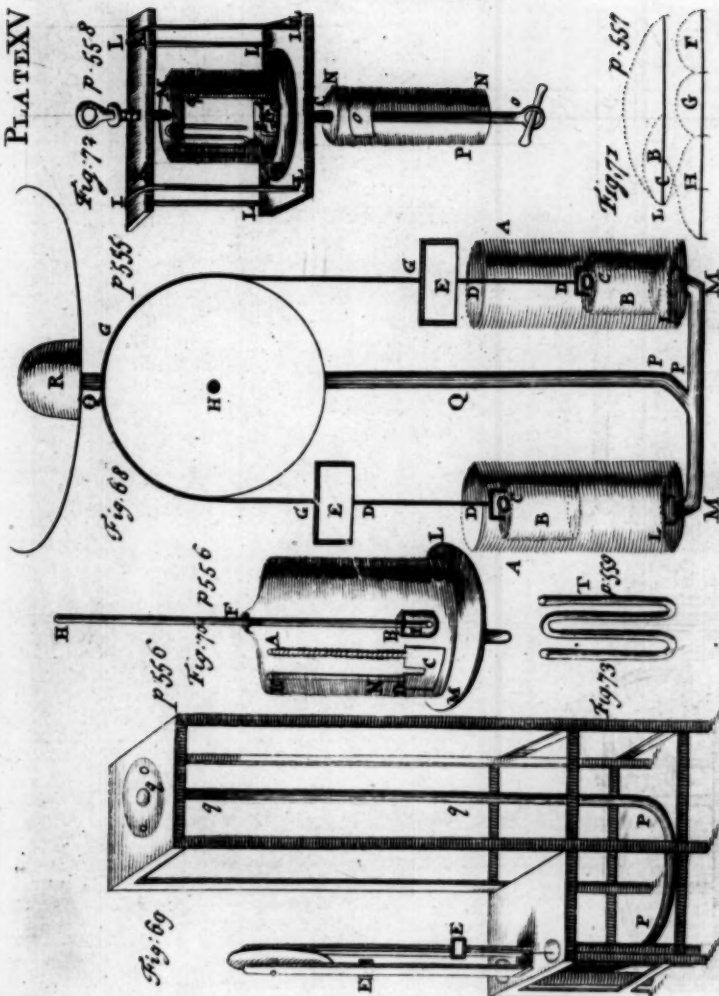


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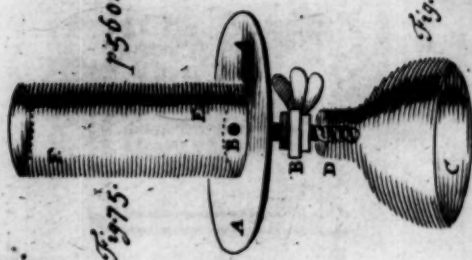
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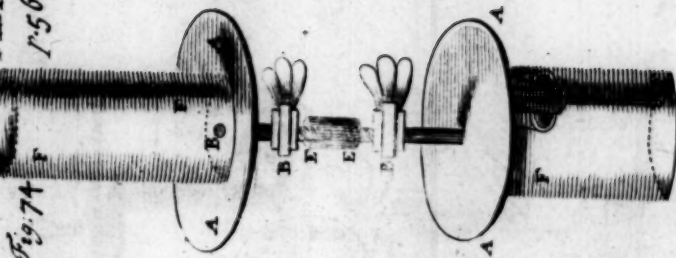
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PLATE XVI.

17560.



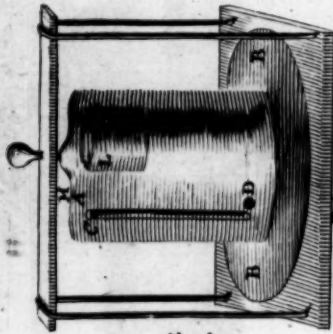


Fig. 77.
p. 561.

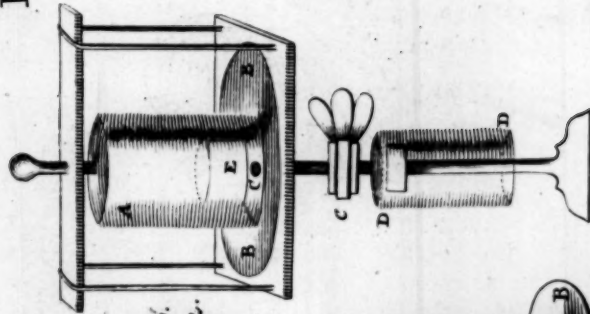


Fig. 79.
p. 562.

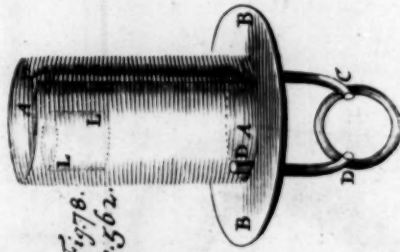
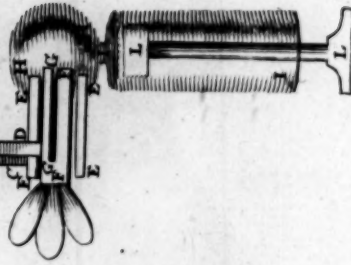


Fig. 78.
p. 562.



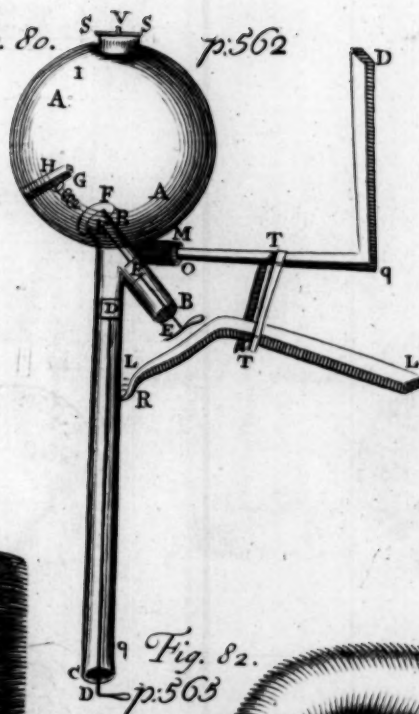


Fig. 81.
p. 564

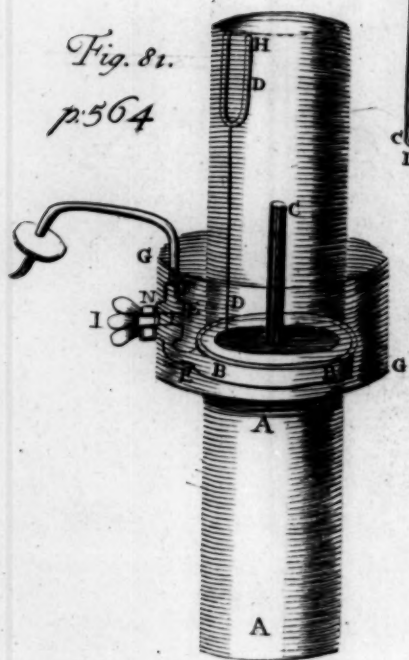


Fig. 82.
p. 563

